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American Character and the American Novel—Griswold
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RESEARCH NOTE

Detecting Sentencing Disparity—Thomson and Zingraff

Review Essay on Bendix—Meyer

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IN THIS ISSUE

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MICHEL VERDON, assistant lecturer in the Department of Social Anthropology at the University of Cambridge, has done intensive anthropological fieldwork on the problems of descent, kinship, marriage, and the family in rural Quebec and among the Ewe of Southern Ghana and Togo and is interested in the theoretical problems raised by the comparative study of social organization.

ANDREW ABBOTT is an instructor in sociology at Rutgers College. In his doctoral thesis, he has examined the departure of American psychiatrists from the mental hospitals founded by their professional forebears, a specific example of the general phenomenon discussed in the article in this issue.

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MATTHEW T. ZINGRAFF, assistant professor of sociology at North Carolina State University, received his Ph.D. from Bowling Green State University in 1976. His current research activities focus on rural-urban differentials in arrest rates and sentencing outcomes.

ERRATUM

In the article "Socioeconomic Status and Mental Disorder: New Evidence and a Sociomedical Formulation" by William A. Rushing and Suzanne T. Ortega (*AJS* 84 [March 1979]), the results were erroneously reported to cover the period from fiscal 1956 to fiscal 1965 (June 30, 1965). The first sentence of the second paragraph of text on p. 1175 should read as follows: "Data are for all 21- to 64-year-old first admissions who were admitted and discharged during the period between calendar 1956 (January 1, 1956) and fiscal 1965 (June 30, 1965) to each of the six Tennessee state mental hospitals."

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1. When author's name is in the text: Duncan (1959). When author's name is not in text: (Gouldner 1963).

2. Pagination follows year of publication: (Lipset 1964, pp. 61-65).

3. For more than three authors, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation: (U.S. Bureau of Census 1963, p. 117).

4. With more than one reference to an author in the same year, distinguish them by use of letters (*a*, *b*) attached to the year of publication: (1965*a*).

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Format of References: List all items alphabetically by author (providing the full list of multiple authors) and, within author(s), by year of publication, in an appendix titled "References." Examples follow:

Davis, K. 1963*a*. "Social Demography." Pp. 124-37 in *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic.

———. 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29: 345-66.

Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32: 5-19.

Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

Weber, Max. (1921) 1968. "Society's Problems." Pp. 12-16 in *Economy and Society*, edited by Guenther Roth and Claus Wittich. New York: Bedminster.

Talcott Parsons and the Theory of Action. I. The Structure of the Kantian Core¹

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This essay presents the thesis that a correct understanding of Talcott Parsons's writings must begin from the assumption of a fundamental congruence of basic structure and method between the theory of action and Kant's critical philosophy. It is already generally understood that this congruence holds true on the metalevel of epistemological assumptions. It is less understood that the Kantian form of argument penetrates down to the level of the object theory of the general theory of action. The core of action theory is the notion that concrete action is to be explained as a result of the inner laws and the characteristic interrelations of analytically distinct subsystems of action. Thus the Parsonian solution to the central problem of social order is not "utilitarian"; nor, however, is it in any simple sense "normative," as it is often taken to be. Parsons's solution lies instead in the notion of the "interpenetration" of distinct subsystems of action. This notion of interpenetration is a derivative of Kantian transcendental philosophy. In this sense, a Kantian "core" structures the theoretical framework of the general theory of action, and Parsons's theoretical development must be understood as a progressive elaboration and refinement of this central core.

A normative orientation is fundamental to the schema of action in the same sense that space is fundamental to that of the classical mechanics; in terms of the given conceptual scheme there is no such thing as action except as effort to conform with norms just as there is no such thing as motion except as change of location in space. [PARSONS (1937) 1968, pp. 76-77]

This position of Kant's is clearly of central importance to the general theory of action. We hold that it is the locus of the most fundamental underlying premises or assumptions of *social* ordering at the human level. It should explicitly be defined not as the *data* of moral problems but as the *transcendental normative conditions of the ordering of such data*. This Kantian

¹ A German version of this essay was published in the Fall 1979 issue of *Soziale Welt*. I am very grateful to Paul J. Gudel of the Department of Comparative Studies in the Humanities, Ohio State University, for providing a competent and careful translation. The English version includes a few additions, most of them minor, for clarification of the argument. This essay is the first part of a two-part study of Parsons (see Münch 1980a).

philosophical position clearly underlies both Durkheim's and Weber's treatment of the moral component of societies, especially modern societies. [PARSONS 1978, pp. 370-71]

For the past four decades, no sociologist has been able to escape the influence of the work of Talcott Parsons. This work, awesome in its scope, depth, and continuity, began with Parsons's dissertation at Heidelberg on the concept of capitalism in the writings of Max Weber and Werner Sombart.² Several essays on Alfred Marshall (Parsons 1931, 1932) and Vilfredo Pareto (Parsons 1936, [1933] 1968) extended these early studies. A first culmination point in his theoretical development was reached with *The Structure of Social Action* ([1937] 1968), a book which has become a classic in its own right through its constructive interpretation of the classic authors. The remaining stages of Parsons's development are likewise marked by major publications, the last of which is the collection *Action Theory and the Human Condition* (1978a). It has become an obligation for every sociologist, not only those working directly on theoretical questions but also all those engaged in the various fields of practical application, to take into consideration Parsons's work—however complete or incomplete, correct or incorrect, it may turn out to be.

Although Parsons's sociology has become in this sense an institution, it is equally true that the conventional attitude toward his theory is one of critical aloofness. Sociologists do not like to be identified with his optimistic judgments on modern American society, or with the label of "conservatism" which Dahrendorf (1955, 1958), C. Wright Mills (1959), Gouldner (1971), and many other critics have affixed to his sociology.³ In addition, the complicated model building in Parsons's sociology causes many sociologists to keep their distance from it.⁴ Certainly only a few

² The dissertation, written in German, has never been published. However, two essays on the same topic appeared in the *Journal of Political Economy* (Parsons 1928, 1929).

³ Elsewhere, I have tried to lay the groundwork for a more positive interpretation of Parsons, as opposed to these ritualized criticisms (cf. Münch 1976a, 1976b, 1978a, 1978b). For important constructive contributions to Parsonian action theory, see Loubser et al. 1976.

⁴ F. N. House, in his review of *The Structure of Social Action*, ranked the potential importance of the work for social theory very high. Although he confirmed this assessment when the second edition appeared, he had already deplored the fact that the work "is so long and abstruse in style; many American students of sociology who would profit by it will be deterred from reading it" (House 1939, p. 130; see also House 1950). Similar difficulties were stated by E. Faris in his review of *The Social System*: "It would be difficult to contend that the book is well written. Even his loyal disciples will often confess that there are parts that are very difficult to understand" (Faris 1953, p. 105). Today we can hardly say that Parsons took the implicit advice of his older friend to heart. The later writings, instead, display at times an even greater complexity in both language and content. House's fear that only a few might force themselves to read *The Structure of Social Action* at all thoroughly has been realized for all of Parsons's writings. This is not so much the

sociologists have taken the trouble to follow Parsons's elaborate technical manipulations of his theory through his numerous works. Outside a narrow circle of disciples, the potential of his work has barely been tapped. Frequently, the argument that his theoretical apparatus can only produce reifications and so blocks any access to reality has been used to avoid having to undertake the difficult task of testing systematically the adequacy of the theory's application to the world. One can use such systematic and thorough testing to exhibit the range and limits of a theory, but only if one is willing to go beyond wholesale criticisms.

The question how much explanatory power a theory as elaborated as Parsons's possesses is not one which can be decided by general arguments or global judgments. After all, Parsons himself demonstrated more than sufficiently how rewarding such a testing of the explanatory power of his theory can be. Not the least part of this demonstration is the very range of his work, from general theory construction down to the analyses, frequently in occasional pieces, of concrete empirical-practical problems. What one comes to realize is that it is exactly this joining of opposites—of general theory development with empirical-practical analysis—which makes Parsons's sociology so fruitful. It makes possible that interpenetration of theory and experience, of logic and practice, which is such a crucial prerequisite for the development of every modern science. In an autobiographical article, Parsons points out this twofold character of his sociological work and emphasizes that his kind of theory building is neither logically deductive nor sheerly inductive, but rather resembles the continual process of the systematization of the law which one finds in Common Law jurisdictions:

In this process [i.e., Parsons's theoretical development] . . . , I have indeed reacted to quite a number of externally presented stimuli of the sort that I have characterized, especially requests to write on topics suggested by others. In a sufficient proportion of such cases, I hope I have reacted somewhat in the manner of a competent common-law appellate judge: namely, that I have considered the submitted topics and problems in relation to a theoretical scheme, which—though its premises were not defined with complete precision and henceforth assumed as fully given in a logically complete sense—has had considerable clarity, consistency,

particular fault of a particular author, however, as it is that of the entire discipline. The interpretation and understanding of every significant contribution in sociology takes, above all, time. It is impossible to arrive at a full comprehension of Marx or Weber or Durkheim or Parsons by just dipping into excerpts, as seems unfortunately to have become the usual procedure in university courses in sociology. In the field of philosophy, a similar method would be regarded as a waste of time. Sociology, however, has been increasingly affected by a large growth in demand from external practical fields and has had to keep adapting continually to a more hectic timetable. The result has been to leave it no time to take adequate theoretical stock of itself and to consolidate its scientific core.

and continuity. In a sufficient proportion of cases, it seems to me that this kind of procedure has yielded empirical insight and rounding out, extension, and revision and generalization of the theoretical scheme. At certain points this has meant intensive concern with formally defined theoretical problems, but at other points primary concern with much more empirical issues. In any case this is essentially what I have meant by the phrase "building social system theory" as used in the title of this essay. [Parsons 1970, p. 868]

We could describe this procedure as a specific form of the interpenetration of two subsystems of the production of knowledge which are themselves subject to their own laws: the subsystem of theoretical research and the subsystem of practical problem solving. In saying this we apply the central concept of Parsons's theory to his own sociological work. Only by developing such zones of interpenetration is it possible to synthesize the results of differentiated subsystems into a unified whole which would possess its own specific character and which would have more power to illuminate the world than either an undifferentiated unity or the sum of the particular subsystems themselves. Here it is theory without intuitions which remains empty, while intuitions without theory are blind.

The significance of this interpenetration of concept and intuition, of theory and experience, as a condition of modern science is nowhere made clearer than in Immanuel Kant's *Critique of Pure Reason* ([1781] 1956b). Among philosophers, it is Kant above all who articulates the specific epistemological structure of modern scientific knowledge, and Talcott Parsons's sociology is everywhere permeated with the structure of the philosophy of Kant. It is not only the aforementioned interpenetration of theory construction and empirical analysis in Parsons's concrete sociological work which leads us to this conclusion. His general theory of action and his theory of social systems are themselves thoroughly Kantian. If we look at Parsons's biography with this connection in mind, we notice that Parsons began to read Kant's philosophy intensively while studying at Heidelberg in 1925-26. In his autobiographical essay of 1970, Parsons describes his experience:

In retrospect it seems to me that this experience was, even apart from the substantive importance of Kant for my problems, especially important training for my later work. It was reinforced by a seminar and oral exam on the same book under Karl Jaspers at Heidelberg in 1926. The importance lay in the fact that I undertook the detailed and repeated study of a great book, the product of a great mind, to a point of reaching a certain level of appreciation of the nature of its contribution, and not being satisfied with the myriad of current rather superficial comments about it. This experience stood me in good stead in working with the contributions of my own authors and coming to what I felt to be a high level of understanding of them in the face of many distorted interpreta-

tions current in the secondary literature, some of which were widely accepted. [Parsons 1970, p. 876]

Although even a superficial study of Parsons reveals the influence of Kant's epistemological conceptions, my larger claim here is that Parsons's general theory of action and theory of social systems are exactly parallel, in structure and method, to Kant's critical philosophy. I believe that this hypothesis gives us a way of reading Parsons which has heretofore been almost wholly neglected and that this neglect constitutes a crucial deficiency in the reception accorded Parsons's work.⁵ Parsons's sociology cannot be understood apart from a consideration of Kant's critical project. In this paper, I will use the philosophical perspective provided by Kant's critiques as a framework for the interpretation of Parsons's work as a whole. My hope is that by doing so I can initiate a reconsideration of Parsons's theory of action and its various concretizations and that this reconsideration will open up perspectives on Parsons's work which will be free of the clichés that up to now have proved to be mostly obstacles to our understanding of it.

My first task is to establish my thesis with regard to the structure of

⁵ Studies on this topic exist, but they remain preliminary and fail to construct a complete interpretive framework. A direct connection between the cognitive theories of Kant and Parsons has been asserted by H. J. Bershady (1973), who in my view, however, fails to grasp the essential nature of the correspondence. Bershady draws a parallel between Parsons's "action frame of reference" of 1937—conditions, means, ends, and norms as a priori categories of every social science—and Kant's categories of the understanding, and he concentrates on the critical assessment of the claim that causal explanation according to the covering law model is possible only if it is based upon this frame of reference. Bershady does not see any continuous development of content in Parsons's elaborations of his theory after 1937; he sees them instead as related attempts to satisfy the conditions of generalization and causal explanation with one theoretical framework after another. In this, he is hampered by an overly narrow, purely functionalist interpretation of the action frame of reference, reducing Parsonian theory to an attempt to formulate necessary conditions for the existence or evolutionary development of social systems, an attempt which is forced to settle for functionalist explanations as a second-best solution, strict causal explanation always seeming to be out of reach. Also unsatisfactory is the quasi-teleological solution to the dilemma of causal explanation vs. ideography which Bershady, following V. Wright, puts forth. This is nothing but a relapse into historicism which sacrifices Parsons's greatest achievement, namely, the constitution of sociology as a discipline which is not forced to understand itself in terms of either the models of strict causal explanation of the natural sciences or the pure ideographic methods of the study of the arts and other cultural products. In order to understand Parsons's basic perspective, we must draw a parallel, not between the action frame of reference and the categories of the understanding, but between the action frame of reference and the structure and purpose of Kant's critical project as a whole, concentrating on the interpenetration between the categories of the understanding and sense data, between the categorical imperative and hypothetical imperatives, between the teleological principle and concrete judgments. Only thus will we illuminate the interpenetration of the normative and the conditional in the theory of action. On this topic, see Schwanenberg 1970, 1971.

the theory of action as it is presented in *The Structure of Social Action* and in Parsons's constructive integration of his classical sources (Durkheim, Weber, and Freud), in order to isolate that core of the theory of action which is systematically expanded, without major changes, in all of Parsons's further writings.

THE INTERPRETIVE PERSPECTIVE: KANT'S CRITICAL PHILOSOPHY

In his last theoretical publication, "A Paradigm of the Human Condition," Parsons, fresh from a renewed, intensive study of Kant, enlarges on the significance of Kant's transcendental arguments for his own theory of action.⁶ Parsons shows special interest in Kant's dualistic construal of the nature of reality:

Kant clearly thought in terms of dual levels: the categories of understanding and the sense data of empirical knowledge; the "categorical imperative" and the "problems" of practical ethics; the canons of judgment and esthetic "experience." There seems to be a striking parallel between his version of duality and the linguist's "deep structures" and "surface structures," the biologist's "genotypes" and "phenotypes," the cyberneticist's "high on information" and "high on energy," and indeed the sociologist's "values," or institutional patterns, and "interests." We therefore suggest that the first term in each of these pairs be used to designate a *metast*structure, which is not as such a property of the phenomena (also Kant's term) under consideration but is rather an *a priori* set of conditions without which the phenomena in question could not be conceived in an orderly manner. [1978c, pp. 355-56]⁷

Kant's *Critique of Pure Reason* is best understood as a reply to Hume's ([1748] 1902, [1739-40] 1973) empiricism and to the skepticism which results from that empiricism. Proceeding according to the tenets of a rigorous empiricism, Hume concluded that the knowledge expressed in the propositions of the natural sciences reduced ultimately to nothing more than collections of sense perceptions ([1748] 1902, pp. 11-17). These sense perceptions, moreover, occur singly and, contrary to the claims of the causal laws of natural science, have no intrinsic connection with each other. There is no bridge from atomized singular perceptions to the general laws of science. According to Hume, habit alone leads us to believe in the necessity of the causal connection between two events which are always experienced as occurring in the same temporal order. Hume's point

⁶ Parsons was inspired to undertake this restudy by a series of discussions which took place at the University of Pennsylvania between 1974 and 1976.

⁷ In the first sentence of this quotation, I have modified Parsons by reversing the pairs of terms. I have done this so that the first term in each pair will be the "transcendental" term and the second the "empirical" term. This makes the pairs of terms in the first sentence formally identical with those in the second sentence.

is precisely that this is mere belief, not knowledge ([1748] 1902, pp. 20–45).

The attempt to construct a completely consistent empiricism had led Hume to doubt the possibility of scientific knowledge. Kant, on the other hand, began from a completely different conception of the nature of science. For him, the validity of scientific knowledge was a given, a fact. His question was, How can we explain how such knowledge is possible?⁸ His explanation took the form of a “transcendental argument,” by means of which he demonstrated that the possibility of scientific knowledge having universal validity for all men depends on the existence of certain preconditions. An important component of Kant’s argument here is the distinction between the specific capacities of a priori categories and of empirical sense experience. It is impossible fully to understand the nature of scientific knowledge by reference either to the order embodied in its abstract principles or to its empirical content alone. For Kant, modern scientific knowledge is explainable neither as a habitual generalization from empirical experience, as in Hume’s empiricism, nor as a series of deductions from the first principles of reason, as in the rationalism of someone like Descartes, but only as the mutual interaction of theory and experience. Experience that can become the touchstone of universal laws is itself only made possible by a table of categories and a set of general theoretical principles, which analytically order our sense perceptions. And it is only insofar as we constantly refer sense perceptions to these categories and principles that we are entitled to say of them that they express empirical and not merely logical regularities.

This connection of opposites—the abstract and the empirical—is a specific historical occurrence whose product is modern Western science. The prototype of this interpenetration of theory and experience is the rational experiment, developed in a historical situation in which conditions were especially favorable to the interpenetration of spheres normally kept separate. These conditions were provided primarily by the scientific associations of the Italian Renaissance of the 15th and 16th centuries and the English scientific movement of the 17th century. These associations united intellectuals with practical men of varying backgrounds—artists, engineers, artisans, merchants, politicians. Their collaboration produced what is by us today considered a matter of course: the interpenetration of theory and experience, of logic and practice, in modern science. Thus there were

⁸ See the “Transcendental Aesthetic” and “Transcendental Logic” in Kant ([1781] 1956b). Elsewhere, I have tried to use this Kantian perspective to elucidate some aspects of the controversy between critical rationalism, Marxism, and critical theory (Münch 1973).

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united for the first time functions performed previously by wholly separate intellectual classes.⁹

Just as the *Critique of Pure Reason* is directed against empiricism in epistemology, Kant's *Critique of Judgment* is an attack on all attempts to construct a theory of aesthetic judgment by generalization from the collection of given individual judgments. A theory of judgment can claim universal validity only if it establishes a connection between a priori categories of judgment (such as the category of "purposiveness as such") and the sensations of pleasure experienced by individuals as they contemplate works of art or the processes of nature ([1799] 1968, pp. 22, 61-68). And the same logic, the logic of the transcendental argument, structures Kant's *Critique of Practical Reason*. The latter work is particularly significant for us, because in it we can discern the main outlines of Parsons's theory of action.

In the *Critique of Practical Reason* ([1797] 1967), Kant argues against any attempt to found moral principles on the subjective considerations of utility of individual actors. That is, he rejects all utilitarian moral theories. Just as we cannot account for the objective necessity of causal laws by reference solely to the content of sense perceptions, so we cannot derive the necessity of a moral law valid for all men at all times from the desires—or the calculations of utility—of individuals. Private calculations of utility may yield different results for different individuals, or for the same individual at different times. The criterion of a moral law, however, is that it is binding for all men at all times. We cannot explain the obligatory force of the moral law as the sum of all calculations of utility, because these calculations would yield extremely variable results, and we would have made no progress toward a concept of true obligation: "The principle of happiness can indeed give maxims, but never maxims which are competent to be laws of the will, even if universal happiness were made the object. For, since the knowledge of this rests on mere data of experience, as each judgment concerning it depends very much on the very changeable opinion of each person, it can give general but never universal rules; that is, the rules it gives will on the average be most often the right ones for the purpose, but they will not be rules which must hold always and necessarily. Consequently, no practical laws can be based on this principle" (Kant 1956a, p. 37).

The characteristic of moral norms is that they lay claim to a validity which is independent of subjective considerations of utility. Our obedience to them is not based on inclination, which is variable, but on duty, which

⁹ This is already a central idea in Weber's work (see Weber [1922] 1973, pp. 596-97; [1920] 1972, 1:1, 10, 414-16, 435-43, 481-84 and 2:143-47, 162-69). The significance of this interpenetration for the development of modern science will have to be examined more fully in another place.

is unchanging. As a philosopher, Kant makes no attempt to provide a sociological analysis of how this sense of duty arises, empirically, in men. His inquiry is directed toward the question, What are the conditions of possibility of a universally binding moral law? Once again, Kant demonstrates that the phenomenon in question is possible only through the linking of abstract categories and empirical ethical problems. Any particular rule of action can be adjudged valid to the extent that it enables us to attain a given end, but there is no direct path from this "hypothetical" validity to a universal, unconditional validity, since our first particular rule might be called into question by other particular rules which function as means to other given ends. To bring order to the multitude of particular rules and to answer the question of their universal validity, we cannot begin from below, but must apply a scheme of categories from above. For Kant, order is produced by a "categorical imperative": "So act that the maxim of your will could always hold at the same time as a principle establishing universal law" (Kant 1956a, p. 30).

Insofar as moral theory takes the categorical imperative as "the supreme principle of morality," it can impose on the multiplicity of particular rules a unifying order which allows us to judge the universal validity of these rules. In this way, the development of a universal moral order is a consequence of the interpenetration of the two parameters along which human action can be judged: (1) the laws of abstraction and of logical consistency and (2) the rules of mutual accommodation and of the satisfaction of needs and desires. A moral order must be in some way a reconciliation of the demands of abstraction and of logic and the needs of practical action. If these are kept separate, the former will generate only empty conceptual systems, while the latter will leave us with a multiplicity of particularized rules of action, each pertaining to a narrowly delimited sphere of human activity, containing no potential for a comprehensive morality.

We know from the comparative studies of Max Weber that it is not a foregone conclusion for any given culture that this interpenetration will occur. A systematic conception of natural law has been developed only in the Occident; in China, as well as in India, the conditions of interpenetration of abstract moral theory and practical regulation essential to such a development were lacking. In both places the two spheres have remained separate and isolated from one another.¹⁰

Accordingly, Kant's philosophy articulates the presuppositions of modern Western society in both the theory of knowledge and the theory of morality. And it is precisely because it has this characteristic that Kant's

¹⁰ The dependence of modern normative order on this interpenetration will have to be discussed in another place (for preliminary information see Weber [1920] 1972, 1:435-38, 2:143-46).

philosophy provides us with a means for understanding Talcott Parsons's sociology, from its beginnings in Parsons's rereadings of Durkheim and Weber, through its incorporation of Freud, to the systematized fullness of the mature theory.

THE CRYSTALLIZATION OF THE THEORY OF ACTION AS A THEORY OF INTERPENETRATION: *THE STRUCTURE OF SOCIAL ACTION*

The significance of Kant's transcendental arguments as a key to the understanding of his own action theory, from its inception in *The Structure of Social Action* ([1937] 1968), was openly acknowledged by Parsons at the other end of his career, in "A Paradigm of the Human Condition" (1978c). There Parsons regards Kant as providing the hinge for the turn from a "positivistic" to a "voluntaristic" theory of action. Kant is therefore a forerunner of Durkheim and Weber, who are read by Parsons in *The Structure of Social Action* as the founders, along with Pareto and Marshall, of a nonutilitarian, voluntaristic theory of action. "This position of Kant's is clearly of central importance to the general theory of action. We hold that it is the locus of the most fundamental underlying premises, or assumptions of *social* ordering at the human level. It should explicitly be defined not as the *data* of moral problems but as the *transcendental normative conditions of the ordering of such data*. This Kantian philosophical position clearly underlies both Durkheim's and Weber's treatment of the moral component of societies, especially modern societies" (Parsons 1978c, pp. 370-71).¹¹

In order fully to appreciate the thrust of Parsons's argument, we have to read *The Structure of Social Action* as the sociological equivalent of Kant's moral philosophy. Only in this way will we be able to understand how this first major work opens out into the theory of action in general and the theory of social systems in particular. Just as Kant developed his theory of action as an alternative to philosophical utilitarianism, Parsons developed his theory of action as an alternative to sociological utilitarianism. This alternative Parsons terms "voluntaristic action theory," the basic principles of which he finds adumbrated in the writings of Marshall, Pareto, Durkheim, and Weber.

A central reference point here is Hobbes's ([1651] 1966) formulation of the problem of social order. A "social order" can be said to exist to the extent that individual actors are connected by a system of shared behavior patterns on the basis of which they can form rational expectations about

¹¹ The misunderstanding due to the failure to see this Kantian character of the core of Parsons's theory of action is extremely widespread. Schutz's critique of Parsons from a subjectivist-idealist standpoint already makes this mistake. See the recently published exchange of letters (Schutz and Parsons 1977).

each other's actions. For example, if someone wants to make use of a pasture, he must be able to determine whether there are any other potential users of this pasture who might claim the same right to use it. If he cannot determine this, his use of the pasture is in principle not secure. At any time, a shortage of pasturage might cause others to dispute his right to use this pasture. He can successfully realize in action his aims and intentions only if he is able to defend himself against the attacks of others, and he can do the latter only if he has sufficient power. So it must be the goal of every actor to gain power over his fellows. The war of all against all is the inevitable consequence. But this state of war, in which every man must fear every other man, is an intolerable life for men. As Hobbes shows, in this situation—as in the so-called prisoner's dilemma—the most rational actions that men can perform on the basis of their individual, subjective calculations of utility result in their being far worse off than if all had accepted some appropriate distribution of rights.¹²

As Parsons points out ([1937] 1968, pp. 89–94), Hobbes expresses in paradigmatic form the dilemma of utilitarian social theories. If the aims of actions are completely arbitrary and if rationality is the only criterion for the selection of the means to those ends, then within a system in which resources are limited and actors act wholly independently of each other, subjective rationality in fact produces irrational consequences for every actor. Looking at this system from the outside, we can easily conclude that all the actors would obtain more satisfactory results if there were a distribution of rights and duties and if everyone could count on everyone else's observance of these rights and duties. In other words, there ought to be a normative order within the system. The utilitarian dilemma consists in the fact that within the system there is no motivation for the actors to try to alter their self-destructive situation. The most rational strategy is still the acquisition of superior power; the acceptance of a normative order requires the confidence that others too will stick to the norms. This in turn requires that everyone treat adherence to the norms not as one end among others, but as a higher end which is never submitted to the conditions of utility calculation. Thus we must give up the utilitarian premise that there are no necessarily permanent ends; the free calculation of utility requires precisely that the rank ordering of ends be allowed to change as the consequences we expect to follow from their realization change. (And this means that utilitarian calculation can choose among ends only by evaluating them as if they were means.) If

¹² Cf. Buchanan 1975, esp. p. 27. Buchanan, in strict conformity with utilitarian theory, attempts to explain "how 'law,' 'the rules of property,' 'rules for behavior,' might emerge from the non-idealistic, self-interested behavior of men" (Buchanan 1975, p. 43), and he ends up, as might be expected, right in the snares of the Hobbesian dilemma: where "private sanctioning" no longer guarantees order, centralized sanctioning has to take over.

the adherence to a norm is to function as a permanent high priority end, it must constitute the limit of the process of the calculation of utility; in other words, it must be ranked so high that it is never subjected to competition from other ends. So long as this condition is not satisfied, everyone is more likely to trust to his own resources of strength than he is to count on the adherence of others to the norms which define individual rights.

If we stay within the framework of a consistent utilitarianism, there is no way out of this dilemma. Hobbes's own solution postulates that the actors, taking the position of observers of their own situation, would realize that it is more useful for them to adhere to a common order. When the need for security becomes an end with a high enough priority, the actors will supposedly be ready to come to an agreement with one another and to transfer all of their power to a sovereign ruler (Hobbes [1651] 1966, chap. 17). But Hobbes's solution is inconsistent; it is not at all rational for the individual actor to make such an agreement: "This solution really involves stretching, at a critical point, the conception of rationality beyond its scope in the rest of the theory, to a point where the actors come to realize the situation as a whole instead of pursuing their own ends in terms of their immediate situation, and then take the action necessary to eliminate force and fraud and, purchasing [*sic*] security at the sacrifice of the advantages to be gained by their future employment" (Parsons [1937] 1968, p. 93).

As long as the individual cannot rely completely on the fidelity of others to their mutual contract, it is not rational for him to enter into such a contract. And he can never trust in the fidelity of others unless such fidelity is no longer the object of utility calculations. This requires, however, a normative limitation of the principle of utility binding for all actors; otherwise it will be safer to rely on "force and fraud" as a means of protection against the predations of others. Such a normative limitation on the principle of utility would have to precede the agreement of the actors in the system to resign power resources; it could never be the result of such an agreement. Hobbes of course realized this, which is why he withholds from the citizens the right to cancel their contract so long as the sovereign can guarantee order.

Thus it is not calculation of utility which motivates the citizens' adherence to the contract, since in regard to the contract we have the same distrust as everywhere else in the state of nature (Hobbes [1651] 1966, chaps. 14 and 15). According to Hobbes, only the threat of external sanctions can assure the citizens' conformity to social norms. Order comes not from a contract spontaneously arrived at, but from a centralized authority. Hobbes thus provides a solution to the utilitarian dilemma which has

become the traditional utilitarian response to the fact that a free play of individual interests does not spontaneously yield social order. The solution turns on the idea of the centralization of decision-making power and the power to enforce sanctions. Essentially the same solution is offered in the model of "collective resources" which James Coleman (1974*a*; see also 1971, 1974*b*) has advanced as a way of dealing with cases in which social exchange by itself proves unable to generate social order. The same can be said for the work of Viktor Vanberg (1978), who like Coleman is a modern utilitarian. The only alternative Vanberg is able to imagine to the formation of order out of a process of free exchange is the centralization of decision-making power. Collective obedience to a norm is simply stipulated by a central authority and imposed through the threat of force. The tensions inherent within the utilitarian solution can be seen in the fact that we are no longer talking about "norms," but only about centralized or decentralized "decisions." In fact, Vanberg tends to use the terms "central decision," "common decision," and "social decision" as at least functional equivalents (Vanberg 1978, p. 671).

What does motivate individuals to accept a centralization of decision-making power? In the various utilitarian models, the motivation can only be the fear of a superior ability to impose sanctions, since the possibility of a spontaneous acceptance generated by a free play of interests has already been ruled out. This implies that a rational agent should want to obtain this superior power to impose sanctions himself. So we have landed back where we started, with Hobbes's war of all against all. A society can be expected to possess a stable order only if it contains a sufficiently pronounced hierarchy of power. But even this would be a very fragile order, endangered by every change in the availability of the resources of power. Thus the appeal to centralization of power is in the end no solution to the utilitarian dilemma, because it fails to touch the core of the problem: the need to limit the possible ends and means that individual actors may choose. "A purely utilitarian society is chaotic and unstable, because in the absence of limitations on the use of means, particularly force and fraud, it must, in the nature of the case, resolve itself into an unlimited struggle for power" (Parsons [1937] 1968, pp. 93-94). Simple centralization of power will be nothing more than a stage in the struggle for power unless it can found itself on some limitation on the usage of power other than the mere existence of a sufficiently hierarchized distribution of power within the society.

This is the argument on the basis of which Parsons maintains that within the system of utilitarian thought no solution to the Hobbesian problem of order is possible. If the human condition contained only those

elements recognized by utilitarianism, there would be no social order. Like Kant in his criticism of skepticism, Parsons begins his own argument with the claim that social order *does* exist, even if in an always incompletely realized form, and that our task should be to try to explain how this is possible. Parsons's solution to the problem of social order is often called a "normative solution." The use of this phrase contains the assumption that Parsons explains the existence of order by reference to a common system of norms or values. Once Parsons has been oversimplified in this way, it is easy to take the next step and accuse him of not really explaining the existence of order at all, but only offering a definition. And thus the utilitarians feel justified in presenting again their old solutions—exchange or constraint or some combination thereof—while ignoring Parsons's demonstration that these solutions are inherently inadequate.¹³

If we do not want to miss the point of Parsons's solution to the problem, we have to read him from the perspective of Kant, and that means first of all recognizing that Parsons's solution can be neither normative nor utilitarian. Parsons presents a voluntaristic solution to the problem of social order. The term "voluntaristic" indicates that social order need not be a completely causally determined factual order. As soon as a centralized force does not provide a factual order by causally determined compliance, social order is only possible as long as the actors voluntarily consent and bind themselves to a common normative frame of reference. A normative order of this kind, as opposed to a merely factual order, thus demands rational justification of particular norms by subsuming them to universally accepted values. In fact, Parsons's exposition of the voluntaristic theory of action requires that he fight on two fronts. We have seen that he defines his own position in opposition to the positivistic-utilitarian theory of action, which recognizes no criterion for the selection of human actions other than the rationality which weighs actions as means to ends. But he must also define his own position in opposition to idealistic theories of action—for example, that of German idealism, which understands all human action as an objectification of *Geist*. In opposition to both of these extremes, the voluntaristic theory of action maintains that human action must be understood as the result of an interpenetration of means-end rationality and a normative limitation on the free play of such rationality. Parsons refers to this relation as one of "interdependence" or "interaction": "The voluntaristic system does not in the least deny an important role to conditional and other non-normative elements, but considers them as inter-

¹³ This type of argument, characteristic of a number of critiques, is flawed at its very foundations. For examples, see Ellis 1971; Vanberg 1975, pp. 161-94, 1978; Schütte 1977. For a more recent form of critique of Parsons's theory of social order, see also the debate between Parsons and Burger (Parsons 1977a; Burger 1977).

dependent with the normative" (Parsons [1937] 1968, p. 82).¹⁴ Already at this stage in the development of his theory, Parsons has in mind that relation between analytically separate subsystems which he will later, in the fifties, refer to as "interpenetration."¹⁵

What precisely is meant by interpenetration between the "conditional" and the "normative"? It does not mean that action is always, in every particular case, determined by both components. It means rather that social order is possible only if there is such an interpenetration between self-interested, rationalized action and a frame of reference which sets the limits of the process of the calculation of utility by ruling certain ends and means out of bounds altogether and by giving to others a stable priority independent of factual conditions and means-end considerations. Order can exist only if the actors are not free to shed their agreements with one another as easily as they might shed an uncomfortably starched shirt. The decisive question of transcendental logic is, then, Given that social order exists, what conditions constitute the framework within which social action necessarily takes place? Kant asks, If there is objective scientific knowledge and if sense experience by itself can provide us with only an unordered "sensuous manifold," then whence the order and universal validity which are the defining characteristics of scientific knowledge? Kant's question and Parsons's question have the same form. And just as for Kant the answer to the question turned on the discovery of the limits set on the arbitrariness of sense experience, so for Parsons the answer to his question turns on the discovery of a limit on the arbitrariness of action determined solely by subjective considerations of utility. A social order cannot be established by utility considerations alone; there must be something else which provides the structure within which alone such considerations are possible. Parsons does not formulate his problem in terms of a given activity of pure utilitarian action which then must somehow be limited by norms. Rather, the limitation itself provides the condition of possibility for that which it limits. This reversal of logical priority is a characteristically Kantian maneuver.

The Kantian critical project is a comprehensive examination of the

¹⁴ An interpretation which falls just short of giving a formulation of this core of Parsonian theory can be found in Alexander (1978). However, Alexander fails to emphasize "interpenetration" as the central idea and instead argues within the limits of the traditional theory of differentiation.

¹⁵ On this topic, see Luhmann 1977a, 1978b; Jensen 1978. According to Luhmann, the concept of interpenetration occurs "only late" in Parsons's writings, while Jensen asserts that it is introduced in the mid-fifties. Both authors fail to see that already in *The Structure of Social Action* Parsons is putting forth a theory of interpenetration. Nor are they aware of the basic affinity of this conception with Kant's philosophy. Luhmann veers away from Parsons's theoretical program before he really understands it, and his attempt at a new conceptualization of the program does not even reach the point at which Parsons left off, much less go beyond it.

specific capacities of the human faculties of knowledge—sense perception, which supplies empirical data, and the understanding, which governs the formation of concepts and judgments. Kant's conclusion is that the orderedness of our experience, as it manifests itself in the fact that the canons of universal validity, the rules of logical consistency, and the laws of causality apply to it, is not something which is merely perceived, but occurs only through the a priori limitation of the possibilities of perception, the drawing of "the bounds of sense," by the rules of concept formation and the modes of unity in judgment. In the same way, Parsons conducts an examination of the specific functions of the various elements of a system of action: (1) ends; (2) a situation consisting of (a) available means and (b) given conditions; and (3) a selective principle according to which means and conditions can be related to ends (Parsons [1937] 1968, pp. 77–82). If the selective principle is a pure means-end rationality (Weber's *Zweckrationalität*)—for example, the calculation of utility—no social order can arise. A social order is possible only if there is a selective principle which exempts certain means and ends from utilitarian considerations and assigns to them a permanent priority. Within the limits thus established, action is motivated by considerations of utility and is as variable as empirical knowledge can be within the rules of the understanding.

As Kant shows, there are only two kinds of selective principles: hypothetical and categorical. Only categorical principles can produce a constancy of choice of actions through the variableness of situations of action. We can make use of Kant's distinction in order to understand Parsons's argument that social order is possible only if action is guided, not solely by conditional selective principles, but also by normative selective principles which determine the scope of the validity of the conditional, hypothetical selective principles. These normative selective principles have the same functional significance for the theory of action that the pure intuitions of space and time and the categories of the understanding have for classical mechanics in Kant's *Critique of Pure Reason*: "A normative orientation is fundamental to the schema of action in the same sense that space is fundamental to that of the classical mechanics; in terms of the given conceptual scheme there is no such thing as action except as effort to conform with norms just as there is no such thing as motion except as change of location in space. In both cases the propositions are definitions or logical corollaries of definitions. But it is not necessary for present purposes even to raise the question whether human behavior is 'really' normatively oriented" (Parsons [1937] 1968, pp. 76–77).

A normative selective principle is a categorical rule in Kant's sense. Its validity is not dependent on the character of the individual situations in which it is applied. Concretely, this means that, if there is to be a social order, a rule such as, for example, that excluding the use of force and

fraud must not have merely a hypothetical validity contingent upon the actors' expectations of profiting from it. If the validity of the rule were merely hypothetical, it would often turn out to be more advantageous for actors to employ means which subvert the rule. This would prove the impossibility of mutual trust, and Hobbes's state of nature would be unavoidable. Further, the motivation to restrict oneself to peaceful means of exchange under a threat of sanctions imposed by an external authority would also be hypothetical, since the individual would accept this restriction only as long as he himself was not strong enough to seize the position of centralized authority and turn it against his fellows. In this case there would be no selective principle guaranteeing a constancy of motives for action and determining the use of peaceful means of exchange as the limit of social interaction.

The first thing we get out of Parsons's answer to the question of the possibility of social order is the realization that the selective principle setting the limits of self-interested action must be categorically valid for all actors. That means that the actors obey the rule because it is their obligation to do so, and not because they think it will be useful for them to do so. The paramount question for a sociological theory of social order must be, How is this categorical obligation possible? Only by understanding the function of the normative rules in Parsons's voluntaristic theory of action from this Kantian perspective can we grasp the full significance of Parsons's theory. From the standpoint of this Kantian perspective, it makes no sense to criticize Parsons for offering as an explanation of the existence of social order the very fact he is supposed to be explaining, that is, the existence of common norms. It is equally beside the point to counter Parsons by proposing again the solutions of traditional utilitarian thought—social exchange and, where this fails, a centralized authority with the ability to impose sanctions (see Ellis 1971; Coleman 1971; Vanberg 1978). Parsons has shown these solutions to be fundamentally inadequate, and it was in uncovering the sources of their inadequacy that he first arrived at a formulation of the criterion a theory of social order must meet in regard to the function of normative rules in a system of social action. It is not enough simply to explain "how norms come into existence." One must explain precisely how a categorical obligation toward common norms comes about within a social system. Norms exist only when every actor in the social system can take general adherence to the norms to be a matter of course. This is possible only if adherence to the norms rests not upon merely hypothetical imperatives, but upon a categorical obligation which sets a limit to the process of utilitarian calculation. Every explanation of "the existence of norms" must simultaneously be an explanation of the source of the obligatory force of those norms. We will not be able to give this latter explanation if we continue to resort to free

exchange or centralization of authority, or any other explanatory factor which remains wholly within the utilitarian framework.¹⁶

Parsons's analysis of the system of social action allows him to pose, in its correct form for the first time, the problem which the theory of social order must solve. It points out the direction in which a solution is impossible and the direction in which we must search if we are to find a solution. But Parsons does not simply replace the utilitarian theory of the genesis of norms with a normative theory which views social action solely as it is determined by categorical obligation. According to the voluntaristic theory of action, norms can be generated only through the interpenetration of action oriented toward a means-end rationality and a categorical normative obligation. Neither the one nor the other by itself yields a concrete social order. Where pure means-end rationality reigns, no social order is possible. But if there is nothing but the categorical obligation to obey norms, with no interpenetration with the various spheres of ordinary self-interested action, the result will be nothing but a "sacred" order which will remain so remote from everyday behavior that it will be incapable of imposing a concrete order.¹⁷ An order permeating all areas of social life can be expected only when the spheres of self-interested action and categorical obligation interpenetrate. This interpenetration is not something which can be taken for granted. The degree of such interpenetration in a given society depends on specific conditions which help or hinder it, conditions which it is the task of any theory of the development of norms to articulate.¹⁸

A theory of the development of norms must therefore be a theory of the interpenetration of opposed subsystems—in this case, the subsystem of action directed by means-end rationality and the subsystem of categorical duty. Nowhere else in the discipline of sociology has this basic idea, so essential for any theory of the development of norms and for every theory of social change, been elaborated as lucidly as in the writings of Talcott Parsons. It is all the more ironic, then, that among sociologists it has become almost a ritual to deplore Parsons's failure to provide an explanation of the development of norms and of social change; explaining

¹⁶ This indispensable precondition of the explanation of the generation of norms as the core of social change has been ignored by all those critics who have declared Parsons's theory unable to explain change. Consequently, all alternative theories exhibit an overly narrow conception of the problem in question and are doomed to failure from the start. This holds true of all contributions in this context from Dahrendorf via Mills right up to Gouldner and others.

¹⁷ If Weber had been able to finish his study of Islam, he would most likely have made this point the center of his argument: "The holy right could not be abolished, nor, in spite of all adaptation, could it be realized" (Weber [1922] 1976, p. 475).

¹⁸ Only from this general perspective can we understand Weber's explanation of the development of modern Occidental society. This in itself goes to prove the explanatory potential of a theory of interpenetration.

social change means, after all, nothing other than explaining the institutionalization of a newly arisen order. The charge has been repeated so often that many now believe it purely on hearsay. One might conclude from this that sociologists in general are but little prepared to treat a truly complex theory adequately.

THE SOURCES OF THE THEORY OF ACTION: WHITEHEAD, DURKHEIM, WEBER, FREUD

The most important influences on the development of the theory of action as a Kantian theory are Whitehead's epistemology and Durkheim's and Weber's sociology. These figures assumed in the course of Parsons's development greater significance than Marshall or Pareto, with whom they are bracketed in *The Structure of Social Action* as founders of voluntaristic action theory. Another very important figure in the development of the theory is Freud, whose work Parsons studied in depth only after the appearance of *The Structure of Social Action*.

Parsons's "analytical realism,"¹⁹ as an epistemological concept, has its roots in the work of A. N. Whitehead ([1925] 1967). Analytical realism claims that empirical phenomena acquire significance for a scientific discipline only when they are formulated in terms of the theoretical frame of reference specific to that discipline. This means that any discipline can consider only certain aspects of reality, never reality in its full concrete richness: "Descriptive frames of reference in this sense are fundamental to all science. But by no means do they exhaust scientific conceptualization. Facts cannot be described except within such a schema. But their description within it has, in the first instance, the function of defining a 'phenomenon' which is to be explained. That is, of the great mass of possible empirical observations we select those which are at the same time meaningful within such a schema and 'belong together.' They thus serve together to characterize the essential aspects of a concrete phenomenon, which then becomes the object of scientific interest" (Parsons [1937] 1968, p. 30). We recognize this conception as a derivative of Kant's epistemology. The particular objects of a scientific discipline are themselves constituted through the interpenetration of empirical observation and a theoretical frame of reference. The first function of the theoretical frame of reference is to differentiate as sharply as possible the various aspects of reality, in order to enable examination of the causal relations that might subsist among these aspects. Its second function is to open up the possibility of abstraction, which makes possible the transference of knowledge from one field of phenomena to other fields of

¹⁹ See the interesting study by Bershadsky (1973), as well as Parsons's positive review (Parsons 1977b) and Bershadsky's comment on it (Bershadsky 1977).

phenomena. These functions are best performed by a structure of categories which can be given increasingly greater specificity and analytical precision through the internal differentiation of a few basic concepts and which does not constantly have to keep introducing new concepts to handle its empirical input. Thus the theoretical frame of reference should make possible the abstraction from particulars, on the one hand, and the particularization of abstractions, on the other.

It is impossible to understand the mechanics of Parsons's theory apart from this background. The theory is designed to perform these two basic functions, and the later development of the four-function schema can be understood as the logical consequence of this fundamental commitment. It is supposed to make possible both abstraction and analytic differentiation, in order to provide the most precise assessment possible of the contributions of various aspects of reality in the causation of certain phenomena. This function of analytical schematization must be kept in mind when one tries to understand the development of the theory of action. Certainly this basic epistemological principle accounts for the emphasis, in all of Parsons's writings, on the process of concept formation as constitutive of the "objects" of sociology. Yet, contrary to the frequent criticisms leveled against it, the theory of action consists of much more than mere "taxonomies." Those who find in Parsons's work nothing but a taxonomy have failed to read him carefully.

Whitehead had a mostly formal influence on Parsons; the influence of Durkheim, Weber, and Freud was substantive. According to Parsons, Durkheim, Weber, and Freud all share a dualistic conception of action. All three managed to avoid the pitfalls of reductionism which lay in both directions. Durkheim was not a sociological reductionist, Weber was not an idealistic reductionist, and Freud was not a biological reductionist—although these charges are frequent even today.

Reviewing the utilitarianism of Spencer in his study of the division of labor, Durkheim ([1893] 1964) points out that social exchange always presupposes the existence of rules which are categorically binding. These rules must be categorically binding if they are to serve as the "frame" for the free association of individuals guided by considerations of utility. If these rules themselves were constantly open to considerations of utility, no one could have any confidence that those with whom he had entered into social agreements would abide by those agreements. Durkheim holds, in Kantian fashion, that a stable limit on the free calculation of utility can only be the product of the moral-categorical authority of norms. Durkheim's overriding concern is with the conditions which allow the generation of such categorical obligations and the moral crises which are engendered in societies by the absence of such conditions.

Durkheim does more than stress the importance of obligation, however.

The normative order must also be related to the needs and dispositions of individuals. Durkheim goes beyond Kant in his recognition that individuals must abide by norms not simply because they must, but because they desire to do so. Hence the crucial point of a theory of moral order must be the connection of social obligation with individual desire. This insight is fundamental to the theory of the interpenetration of the social system and the personality system.

Durkheim's theoretical concerns begin with these basic questions and branch out in the direction of a theory of the institutionalization of norms in social systems and a theory of the internalization of norms within the individual personality (Durkheim 1973*a*, 1973*b*; Parsons 1967, [1937] 1968, pp. 324-408). Particularly important here is Durkheim's theorem that it is only insofar as the individual is part of a group on whose approval he is dependent that norms valid within the group can become categorically obligatory for the individual. For Durkheim, it is the lack of this bond between individual and group which explains the phenomenon of suicide—not only egoistic suicide but also anomic suicide (Durkheim [1897] 1972). But since such bonds exist only within groups which have both intensive and extensive contact among their members, the crucial question is, How can a society avoid particularization of these normatively governed bonds? Durkheim sought the solution to this problem in the strengthening of occupational groups as a basic element of social organizations and in the institutionalization of common responsibilities in common assemblies which would enforce compromise decision making (Durkheim [1893] 1964, pp. 1-31).

Durkheim also shows that the process of binding the individual to norms and the development of personality are not submitted in every case to zero-sum conditions. Division of labor has to be accompanied by the loosening of ties between an individual and a group; otherwise, the individual would be unable to participate in social life outside his primary reference group. The individual must be bound in succession to the normative demands of different groups; this is prerequisite not only for the development of a comprehensive normative order but for individualization as well.²⁰ The "cult of the individual" is in this sense a form of social order in which institutionalization of a normative order and the individuation of the personality do not exclude each other, but reinforce each other. This discovery of Durkheim's is of decisive importance for Parsons's theory of institutionalization and internalization, because here Durkheim sets forth a theory of the interpenetration of social system and personality.

We find such a theory of interpenetration in Weber's work, too, particularly in his comparative studies in the sociology of religion ([1920]

²⁰ See Durkheim (1893) 1964, pp. 228-29; see also pp. 172-73, 198-99.

1972).²¹ At the heart of Weber's account of the origin and development of Western society is a conception of the interpenetration of spheres which outside the West have remained separate or even opposed to one another. An especially important role is played here by the tension between religious ethics and the world, a tension which can be found, in the sense of a process of analytical differentiation, almost everywhere. This tension can be eased by any of four methods: accommodation to the world, escape from the world, reconciliation, or mutual penetration.²²

Accommodation to the world is the solution of Confucianism (Weber [1920] 1972, 1:276-536). This solution is found predominantly in societies in which religious ethics are articulated and expounded by a class which is involved in practical life and has its own status interests: for example, the Chinese literati. The consequence of this dominance of the sphere of means-end rationality over the sphere of religious ethics is the lack of an ethical order which could permeate the whole society. There can thus be only a limited ethical control of practical, utilitarian action.

The paradigmatic example of reconciliation is Hinduism (Weber [1920] 1972, vol. 2). The characteristic of this solution to the problem of the tension between religious ethics and the world is the separation of different spheres having their own internal order (the castes). These mini-societies are integrated on the theoretical level by the ideology of the causality of karma and the transmigration of souls. There is no possibility here for a general religious ethics that would cover all of the different social spheres. There can only be particularized ethics for each sphere.

The paradigmatic example of escape from the world as a solution to our problem is Buddhism (Weber [1920] 1972, vol. 2). This solution tends to arise in situations in which religious ethics is primarily the concern of a class of intellectuals who are isolated from the concerns of practical life. The result is a sharp distinction between the ethics of the priests and the ethics of the laymen and the absence of a general ethical code which could regulate the actions in the spheres of everyday life.

A mutual penetration of religious ethics and the world is found only in the Occident, and within the Occident its most complete realization is ascetic Protestantism (Weber [1920] 1972, 1:17-237). Here alone have the practical spheres of economics and politics been ethically regulated, rather than consigned simply to the realms of utilitarian action and the manipulation of power. The typical Puritan business ethic is neither a mere appendage of economic acquisitiveness nor a pure emanation of

²¹ I have elaborated the significance of Weber's comparative studies elsewhere (see Münch 1980b).

²² In this connection, see not only Parsons's interpretation of Weber in *The Structure of Social Action*, but also especially his introduction to the English translation of Weber's systematic work in the sociology of religion (Parsons 1963).

religious ethics. It is instead the qualitatively new product of the interpenetration of two basically opposed spheres. Nothing comparable exists outside the Occident. This penetration of ethics into the domain of business is for Weber the specific mark of modern capitalism, in comparison with all non-Western and premodern forms of economic behavior. It could have occurred only where two institutionally independent spheres came together while retaining their own essential character.

Although the institutional independence of the Christian religion resulted very early from the institutionalization of the custom of educating priests at universities, the penetration of the economic and political spheres by religion occurred only gradually and by steps. A giant step in this direction was the development of the medieval city, because in it, unlike the non-Western city, the religious community, the political community, and the market economy all were brought together (Weber [1922] 1976, pt. 2, chap. 9, sec. 7). But still there was a large gap between priestly ethics and lay ethics, and the bureaucratic organization of the institutionalized church tended to substitute control of external behavior for the internalization of norms. Yet religion and the world were close enough to create ever more extreme tensions between them as economic activity increased. These tensions were released by the Reformation, through a stronger ethical penetration of the world. The traditionalism of the German Reformation caused this process to be coupled there with a strengthening of absolutism. As a result, only in Calvinism and in the Puritan sects and denominations of England, the Netherlands, and the United States was there a truly reciprocal penetration of religious ethics and the world. Important elements of this development were the radical elimination of any distinction between an ethics for the priests and an ethics for the laymen and the importance given to internal control of behavior, an importance conferred first by Calvin's doctrine of predestination.²³ Of paramount importance, however, was the tight binding of the individual to the group by its approval which was achieved within the free religious communities of Puritanism.²⁴

We cannot understand the emergence of the modern normative order, which manages to be both universal and individualistic, and both rational and activist, apart from this interpenetration of religious ethics and the world. The characteristic of this modern order is that it is able to maintain a common identity while encompassing a range of social activities that includes everything from philosophical theories of morality to prac-

²³ This is the explanatory tack taken by Weber in his essay on the Protestant ethic and the spirit of capitalism (see Weber [1920] 1972, 1:17-206).

²⁴ This aspect is emphasized in Weber's study of the Protestant denominations and the spirit of capitalism (see Weber [1920] 1972, 1:207-36).

tical problems of economic and political action. Only the Occident recognizes the philosophical concept of natural law, and the reciprocal penetration of "sacred" and "profane" codes is peculiar to the Occident. Outside the Occident, these two realms have remained alien to one another, or one has dominated the other. In order to explain the genesis of the unique social order of the Occident, Weber has implicit recourse to a theory of interpenetration. Apart from this theoretical perspective, we will fail to understand Weber's explanation of the origin and development of Western society. It is the merit of Talcott Parsons to have distilled this theoretical perspective from Weber's sociological writings and to have made it the central idea of his own theory of action.

As the third source of the theory of action, Sigmund Freud is considered by Parsons to have made the same contribution to a theory of the interpenetration of social system and personality, from the perspective of the personality, as Durkheim made from the perspective of the social system. For Parsons, this convergence is of fundamental significance for the future development of the social sciences: "This convergence, from two quite distinct and independent starting points, deserves to be ranked as one of the truly fundamental landmarks of the development of modern social science" (Parsons 1953, p. 15).

Through his analytic differentiation of the personality into an id, an ego, and a superego, Freud develops a perspective on personality which allows him to view it as a zone of interpenetration of the structure of drives, external reality, and cultural norms (Freud 1972). In connection with this Freudian schema, Parsons emphasizes that already in the earliest (oral) phase in the psychic development of the child, the reality principle of the ego, as well as the affective motivation of the id, is shaped by interaction with social objects, primarily the mother, and so is the result of symbolic processes. This means that the ego and the id are no less cultural products than the superego, an assertion which Parsons claims that Freud found himself increasingly drawn to.

Freud's analyses of the forms of identification between mother and child, and of the differentiation of the object cathexes in the oedipal stage and in the later stages of latency and adolescence, are of great importance for the understanding of the process of socialization (see Parsons 1956*a*, 1956*b*, 1964*a*, 1964*b*; Parsons and Bales 1956). Identification is the result of the fact that the child in the oral phase is dependent on others for the satisfaction of his needs and desires; its basic mechanism is the cathexis of the libidinal object. This object cathexis progresses from a more particularized dependence on pleasure to a more generalized and symbolically formed dependence on love. Identification is the basis for the internalization of cultural norms, which are psychically represented by the object of identification. In the oedipal phase, however, there is a loss of early

objects of identification, along with a differentiation of object cathexes and a corresponding growth of as many identifications as there are dyadic relations for the child within the different roles within the family and, subsequently, outside the family, in peer groups and classrooms. Simultaneously, a differentiation occurs between the child's identification with individual role-partners in the group and his identification with the group as a whole. In this process of differentiation of object cathexes and the loss of infantile identifications, the internalized cultural norms are generalized and detached from particular identifications. The superego which is the result of this process is thus not a replication of the institutionalized norms of a concrete group but rather a generalization from various systems of norms which enables the individual to take part in social life outside his original reference group and to attain a steadily increasing autonomy. What Parsons primarily takes from Freud is this conception of the simultaneous, mutually reinforcing expansions of the scope of the social system and the personal autonomy of the individual as part of one complex process, a process dependent upon a specific form of interpenetration.

FURTHER PROSPECTS

So far, we have seen that Parsons lays the cornerstone of a theory of interpenetration in *The Structure of Social Action* and that we cannot adequately grasp this theory if we do not interpret it in the light of Kant's critiques. This Kantian perspective is also of the greatest importance for the understanding of Parsons's constructive integration of Durkheim, Weber, and Freud into his theory of action. Through the integration of these classic authors, Parsons is able to exceed by far the theoretical range of the interpretations of them which are usually offered.²⁵ Moreover, without

²⁵ Among the attempts at interpretation of these classic authors which have been developed at least in part in opposition to Parsons, not one has made possible a theoretical development as fruitful as Parsons's. Without exception these attempts lose themselves in certain aspects of the discussion and are unable to open up a perspective from which a general theory can be constructed. This is true of Bendix, who reduces the Weberian perspective to the single aspect of the struggle for power between different social groups, and who, in changing Weber from a sociologist into a historian, sacrifices sociology as a theoretical discipline (Bendix 1971; in this context see Parsons's [1972] review). Nor can Pope, Cohen, and Hazelrigg offer an interpretation of Weber and Durkheim which is as constructive as Parsons's. They reduce Durkheim to a "sociological realist" and Weber to a "theorist of conflict and interests" and, in the same context, charge Parsons with approaching the classics with a predetermined interpretive scheme. But this is exactly the reason Parsons has been able to integrate both authors within an extendable theoretical framework, while the efforts of Pope, Cohen, and Hazelrigg remain mere patchwork. (See Pope 1973; Pope, Cohen, and Hazelrigg 1975; see also the discussion between Pope et al. and Parsons in *ASR*

an understanding of this Kantian core, it is impossible to follow the further development of the theory of action. In fact, all of Parsons's further theoretical development is a series of refinements of this theoretical instrument for the analysis of relations of interpenetration. Before anything else, one needed an instrument with which one could analytically detach subsystems which possessed their own rule-governed order from the concrete manifold of reality. Once this was accomplished, one would be able to analyze these subsystems as pure "ideal types" while also studying the nature and extent of their interpenetration. The fundamental theorem here is not the old doctrine of differentiation, according to which systems can increase their capacities through a process of functional differentiation, but the theorem of interpenetration, according to which only a process which allows both the greatest unfolding of the internal laws of a subsystem and the greatest amount of interpenetration with other subsystems can produce a new level of development for the subsystems and the system as a whole. This new level of development is as much the result of the tension between the subsystems as it is the result of their unity. The interpenetration of subsystems unifies opposites and raises the threshold level of tension which the systems can accommodate while still retaining their identity and unity.²⁶

Ethics and business, for example, are here not isolated spheres with rules and laws wholly unique to each. Yet they are relatively independent of one another. They are shaped by different social groups, and so they remain clearly separable from one another. Their mutual interpenetration, in which, for example, members of the same congregation become business partners, opens the possibility of the ethical regulation of business and forces ethics to be relevant to business life.

We can call a relationship formed thus between business and ethics a dialectical one, as long as we understand that we accept the biases of neither the idealistic nor the materialistic dialectics (in fact, these biases

[Cohen 1975; Parsons 1975, 1976; Pope 1975; Pope, Cohen, and Hazelrigg 1977]. See also Warner 1978 and the subsequent comments by Parsons [1978b] and by Pope and Cohen [1978] in *AJS*.) The German interpretation of Weber's work deliberately talks around Parsons, partly by renouncing sociological theory altogether and constructing an ideal type of the inner development of ideas independent of the instructions of a "sociological perspective" (as does Tenbruck [1975]), partly by maintaining that one can bypass Parsons with a Weberian dialectics of ideas and interests (as Schluchter [1976, 1978, 1979] proposes). Both interpretations are interesting and informative, but neither is able to produce a systematic starting point for theoretical developments beyond Weber. They also lack any access to Weber as a theorist of interpenetration. See my critical discussion of these problems (Münch 1980a).

²⁶ This has been explicitly pointed out as a theoretical problem by Luhmann (1977a, 1977b, 1978a, 1978b) in his most recent work. Luhmann, however, does not actually make systematic use of Parsons's achievements (cf. also Jensen 1978.)

prevent the idealistic and materialistic "dialectics" from being true dialectics at all).²⁷ In the zone of interpenetration between business and ethics there appears a new phenomenon: business ethics, which is neither pure business nor pure ethics, but something in between. It brings previously separate spheres together without incorporating one into the other, and it raises the level of each subsystem's tolerance for what had previously been incompatible with it, although this incompatibility can in principle never be totally eliminated. In such a system the possibilities of both economic action and ethical action are expanded. Interpenetration increases the scope and power of both spheres.²⁸

Parsons's theoretical development after *The Structure of Social Action* moves clearly in the direction of refining his theoretical instrument for the analytical differentiation of subsystems and the analysis of the relations of interpenetration among them. The major landmarks in this development are: (1) the differentiation of cultural system, social system, and personality, and the analysis of institutionalization and internalization in *The Social System* (1951) and *Toward a General Theory of Action* (Parsons and Shils 1951); (2) the differentiation of levels of systems and the macroscopic-microscopic application of the four-function paradigm in *Working Papers in the Theory of Action* (Parsons, Bales, and Shils 1953); (3) the introduction of the hierarchy of cybernetic control and of the media of interchange (a) on the level of the social system (money, political power, influence, value commitments) in *Economy and Society* and after (Parsons and Smelser 1956; Parsons 1969a, 1969b, 1969c); (b) on the level of the system of action (intelligence, performance capacity, affect, definition of situation), which dates from *The American University* (Parsons and Platt 1973; Parsons 1977c); and (c) on the level of the human condition as a whole in "A Paradigm of the Human Condition" (1978c).

In all of these works, Talcott Parsons has created a theory which might be compared with Kant's critical philosophy. The task of examining the substantive and methodological issues in his work in order to reach a just determination of Parsons's contribution to the social sciences will occupy social scientists for a long time to come.²⁹

²⁷ The materialist position has, at least in many of its versions, become a crude functionalism, which is satisfied that it has explained a phenomenon as soon as any kind of positive functional relation, no matter how abstract, with the system of capitalism is discovered. It might be said, therefore, that authors like van den Berghe are directing their arguments in the wrong direction (see van den Berghe 1963).

²⁸ This is the true meaning, e.g., of Benjamin Nelson's conception of the development "from tribal brotherhood to universal otherhood" as the main line of development of the process of civilization itself (see Nelson 1969). Weber worked within a similar conception.

²⁹ The further development of Parsons's theory from *The Social System* to *Action Theory and the Human Condition* has been elaborated elsewhere (see Münch 1980a).

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American Character and the American Novel: An Expansion of Reflection Theory in the Sociology of Literature¹

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Attempting to specify how literature "reflects" society, this study describes the analysis of a random sample of 130 novels published in the United States during the late 19th and early 20th centuries. Novels by Americans are compared with those by foreign authors over four time periods. The sample novels reflect the different market positions occupied by the two groups of authors owing to the presence or absence of international copyright protection, the formal demands of the genre, the sex of the author, and several distinctive national characteristics, including treatment of race, middle-class protagonists, and domestic settings.

The nature and consequence of the connections between a society and its literature are the province of literary sociology. One approach to these connections, reflection theory, simply states that cultural products such as literature in some way mirror the social order (Peterson 1979; Albrecht 1954). Hardly a surprising idea, yet much energy has been expended in maintaining this premise that literature is not a discrete entity, *sui generis*, but has links with the social world. It is my contention that this battle has been won, and that sociologists of literature should now redress some of the imbalances that occurred while the field was coming of age. Two such imbalances require particular attention. Some scholars have been so adept at exploring the influence of cultural markets or literary elites that they have paid relatively little attention to the actual content of the literary or cultural works (Escarpit 1971; White and White 1965; Clark 1977, 1979; Hirsch 1972). Others have taken content extremely seriously but have wanted to attribute it to a single cause in the social world, be

¹ I am deeply grateful to Ann Swidler, who has provided abundant intellectual and practical guidance throughout this study. Steve Brint, Paul DiMaggio, Matthew Hamabata, Lauri Perman, Richard Peterson, David Silverstein, and Beth Stevens generously gave advice and criticism. The indefatigable research team consisted of Andy Borowitz, Cheryl Dixon, Michael Downing, Patty Hurtado, Marion Krotzer, Shauna Miller, Caroline O'Brien, Marjorie Solomon, Susan Stevenson, and Stacy Stone. A summary of this paper was presented at the seventy-fourth Annual Meeting of the American Sociological Association, Boston, August 1979.

it characteristics of literary elites, structures of class consciousness, or changes in popular ideology (Lowenthal 1961; Goldman 1967; Williams 1966; Wright 1975). These imbalances have produced excellent iconoclastic studies necessary to counter the emphasis on the text and nothing but the text that used to prevail in traditional literary analysis. Now the time has come for sociologists to consolidate their gains by doing literary research that both takes content into account and considers a variety of causal influences from the social world. The following study was undertaken in the spirit of this enterprise.

Is the American novel unique, as it is often said to be? If so, do its peculiar properties reflect some American character or experience? I have examined the relationship between American novels and the society that produced them by analyzing a random sample of 130 novels published in the United States between 1876 and 1910. The analysis shows that seemingly mundane things such as copyright laws have had considerable influence on the content of American novels. My findings demonstrate that the concept of literature as reflection must be expanded to include reflection of production circumstances, author characteristics, and formal problems, as well as the preoccupations of any particular society.

To begin, consider the background of the production and consumption of novels in the late 19th century. Americans during this period were highly literate and interested in reading (Cipolla 1969, table 21). Following the Civil War there had been an unprecedented growth of the reading habit in the United States (Tebbel 1975).² The literacy rate and the demand for books had always been high in the new nation, but early 19th-century books were too expensive for widespread purchase. Technological advances made by the middle of the century—including the steam-driven cylinder press, the use of stereotyped plates, and cheaper techniques of paper manufacture and book binding—made the mass production of books possible for the first time (Steinberg [1974] 1977). Within 10 years of Appomattox, books, magazines, and newspapers were proliferating, and the publishing industry was expanding rapidly, with some 200 houses operating by the early 1880s.

Fiction led in sheer quantity of titles published. The fiction flood began in the 1870s and continued until 1908, the first year when novels did not lead all other categories of books published. For the year 1886, *Publishers Weekly* (January 27, 1887, p. 185) broke down the 4,676 titles published

² Tebbel cites one indication of the national obsession with the written word: the rapid growth of the country's library system. In 1852, the first public library opened its doors, in Boston; by 1896 the United States had 33 million books on the shelves of 4,000 libraries. Cipolla (1969) has calculated that 80% of the adult population in the United States was literate in 1870, 89% by 1900.

by categories; the top 10 were fiction, 1,080; law, 469; juveniles, 458; literary history and miscellaneous, 388; theology, 377; education and language, 275; poetry and drama, 220; history, 182; medical science, 177; and social and political science, 174.³

The typical reader was a young woman. Women have always been slightly in the majority as consumers of fiction, and it appears that the reading habit in general decreases with age.⁴ This average reader was more likely to come from the city than from a small town or rural area and more apt to be located in the Northeast than in the South or Midwest.

New York had long dominated the publishing industry, followed by Boston, Philadelphia, and Chicago.⁵ Mass production necessitated mass retailing. Much to the disgust of traditional bookstore owners, who felt that the commercialization of literature was a fine thing but only up to a certain point, John Wanamaker had opened the door to department stores and dry-goods dealers stocking books along with their other merchandise. And such mass marketing of books was associated with a surge of advertising and display techniques, disdained by publishers before the Civil War. Some decorum was to be preserved, however; in the 1880s, a copywriter who described *Leaves of Grass* as "a daisy—and don't you forget it!" was promptly fired (Tebbel 1975, p. 154).

In view of the vast number of novels being written and read in late 19th-century America, it seems reasonable to examine these novels to see if they reveal anything about the society that produced them. The first question that must be asked is whether there was anything peculiarly American about the American novel.

³ The summary of the annual trade report for 1876, including the breakdown of the 4,676 titles published that year, is quoted in Tebbel (1975, p. 861).

⁴ There exists no single reliable study of the late 19th-century novel-reading public, so most accounts of the typical young female reader of fiction, including the present one, are based on conjecture. Some evidence supporting this image of the average reader is offered by contemporary sources, such as the *Hour*: "Millions of young girls and hundreds of thousands of young men are *novelized* into absolute idiocy. Novel readers are like opium smokers; the more they have it the more they want of it . . ." (Tebbel 1975, p. 171). I am aware of no 20th-century study that focuses specifically on the readers of novels, but there exist a number of studies of the reading habit in general. All of these support the picture of the reader as young, slightly more likely to be female than male, educated, middle class, urban, and Caucasian. The most recent such survey, prepared by Yankelovich, Skelly, and White (1978) for the Book Industry Consulting Group, is summarized in *Publishers Weekly*. See also Mathews (1973), Knight and Nourse (1969), Ennis (1965), Sharon (1973-74), and various Gallup Polls (Gallup 1972): "Literary Quiz," November 14, 1953, p. 1185; "Reading Habits," August 13, 1955, p. 1353; "Reading Habits," May 15, 1957, p. 1487; "Cultural Activities," February 17, 1963, p. 1805; and "Book and Bible Reading," February 5, 1971, p. 2285.

⁵ Two-thirds of our sample novels were published in New York, 15% in Boston, 12% in Philadelphia.

I

The rise of the novel has been well charted, particularly by Ian Watt ([1957] 1974). Watt shows that the 18th-century novel in England was the product of an age in which the human personality was believed to be essentially knowable, knowledge of it coming from the accumulation of evidence drawn from the detailed observation of behavior. This interest coincided with two other 18th-century developments. One was the rapid expansion of a new audience for literature, the literate middle class, especially the leisured middle-class women. Lacking the education and inclination to read Latin or serious verse, yet wanting diversion, these women offered a ready market for a not-too-demanding literary form. The second development was the decline of patronage and the appearance of its economic equivalent for writers, the bookseller, who encompassed the activities of publisher and printer as well as merchant. The booksellers knew that their customers wanted hours of entertainment, not moments of exquisite feeling. Therefore, they paid authors by the page. For the author, in consequence, "Speed and copiousness tended to become the supreme economic virtues" (Watt [1957] 1974, p. 56).

This confluence of interest in the human personality, audience, and economic institutions gave rise to a genre that was easy to read, long, written in prose, fictitious, devoted to subjects of interest to middle-class women and to an analysis of character through detailed description of behavior—the genre we know as the novel. Eighteenth-century novelists explored the subjects of particular interest to their readership: love and marriage, economic individualism, the complexities of modern life, the possibility of personal morality in a corrupting world.

It has often been held that although "the novel" looks like what I have just described, "the American novel" does not. One version of this argument is that classic American fiction is about men removing themselves from society, especially from women who seem to represent the constraints of social and domestic life. Often with a dark-skinned male companion, these men, or boys, flee to the ocean, the forest, the unknown lands down river and there test their individual strength (Marx 1964). Leslie Fiedler (1966), basing his case on classic works which fit the man-fleeing-society pattern, has asked,

Where is our *Madame Bovary*, our *Anna Karenina*, our *Pride and Prejudice* or *Vanity Fair*? Among our classical novels . . . the best attempt at dealing with love is *The Scarlet Letter*, in which the physical consummation of adultery has occurred and all passion burned away before the novel proper begins. For the rest, there are *Moby Dick* and *Huckleberry Finn*, *The Last of the Mohicans*, *The Red Badge of Courage*, the stories of Edgar Allan Poe—books that turn from society to nature or nightmare

out of a desperate need to avoid the facts of wooing, marriage, and child-bearing. [Fiedler 1966, pp. 24-25]

Fiedler attributes this theme of flight from society to an American pathology, a social and sexual immaturity, tinged with racial guilt, that manifests itself in the American novelist's inability to write about adult subjects such as heterosexual love and the reconciliation of individual freedom with social life.

D. H. Lawrence ([1923] 1977) similarly maintained that American authors were unable to face the facts of life. They perpetuated the American lie, writing of democracy and conventional morality, while beneath the benign surface of their art lurked the unacknowledged recognition that democracy is a failure, inequality is inevitable, and the passions are ultimately victorious over rationality. Lawrence believed the American promise lay in the sloughing off of the dead skin of European ideas, such as democracy and equality, leading toward the eventual freedom of heroic souls to follow their inner promptings, without the overlay of conventional ideas. And he saw the first signs of the fulfillment of this American promise in the American novels. Artists lie, but their art tells the truth.

Recently, Ann Douglas (1978) has found a different type of dishonesty in the American novels of the 19th century, that of sentimentalism. Sentimentalism was the cluster of attitudes which attempted to affirm values, such as gentleness, kindness, and spirituality, that the society as a whole was vigorously denying in its rush toward industrial capitalism. Douglas argues that this sentimentalism stemmed from the influence of disestablished women and ministers who, removed from any real economic and political power in the early part of the century, used culture as an arena for their private wars and interests. These interests included promoting domestic, spiritual, noneconomic virtues, the only virtues they were allowed to possess. The few authors who rebelled against sentimentalism wrote about strident masculinity and avoided domesticity altogether, and these are the authors regarded today as the classic American writers.⁶

All of these commentators find American novels to be unique, although they differ somewhat on just what is unique about them. The premise underlying all three arguments is the same: American society was fundamentally different from European, and this difference was reflected in, and accounts for, the peculiar characteristics of American novels.

⁶ The collision between major 19th-century American authors and the demands of the middlebrow reading public has been recently discussed by Henry Nash Smith (1978). Smith points out that the middle-class reader, imbued with "naive nationalism," shunned the introspection and analytical approach that Hawthorne, Melville, and James were introducing into their novels. These authors themselves vacillated between scorning a readership that demanded happy endings and lamenting the failure of their books to win large sales.

Intriguing though these discussions are, their methodology is open to question. Lawrence treats just a few American classics, meaning those works that were regarded as classics in his time, even though some, like *Moby Dick*, were commercial and critical failures at their initial publication. Fiedler is wider ranging but equally selective. He pointedly disregards novels about women or about men coping with society rather than escaping it. This results in some blatant omissions, such as the absence from his discussion of Edith Wharton, William Dean Howells, and most of the works of Henry James, and excessive reliance on works that support his case, such as *Pierre*. Douglas draws her support from wide but distinctly eclectic sources. Her argument rests on equating tough, masculine novels with classic works and sentimental, domestic novels with forgotten and forgettable works, yet one cannot prove the masculine = classic, feminine = popular thesis by focusing on a few examples. Furthermore, her argument is tautological: is *Moby Dick* high art because it is masculine? because it didn't sell well? She establishes no independent criteria for separating classic from popular literature, but her case rests on making just such a distinction.

Moreover, Victorian sentimentality was hardly an exclusively American phenomenon. But all three of these analyses and most of their fellows lack a comparative approach. Instead, they rely on talking about the European novel or "the novel" as if its characteristics were beyond dispute and as if European writers were turning out nothing else. It is misleading to read all of American character from either *Moby Dick* or *Fern Leaves from Fanny's Portfolio*, but it is equally misleading to assume that every European novel of the period was a *Middlemarch* or *Madame Bovary*.

Any attempt to read American character from American novels should begin with a more systematic attempt to determine just what Americans were writing and reading. Then, one must ask whether these American novels were different from European novels, and how they compared with our conception of the "standard novel" as the genre was formulated in the 18th century. To the extent that the peculiar character of American novels is substantiated, one should look for some possible causes for these distinctive American traits other than simply some uniqueness of the national psyche. Reflection theory is not necessarily wrong, but it can be used to encompass more complex societal/literary relationships than most of its proponents have thus far demonstrated.

II

My search for links between American society and American novels entailed taking a random sample from all novels published in the United States between 1876 and 1910. The source of the sample was the *American*

Catalogue, a series begun in 1876 that recorded every book published in this country. A sample of 130 novels was divided into time periods. Period I covered novels published from 1876 to 1884, Period II from 1884 to 1895, Period III from 1895 to 1905, and Period IV from 1905 through 1910. About half of the novels were written by American authors, and the other half were American reprints of foreign works. Analysis focused on a number of variables pertaining to plot, author characteristics, and bibliographic information (see Appendix A).⁷

The sampling method is not intended to replace the more traditional approach to studying fiction through examination of classics or best-sellers. Instead, sampling supplements these two approaches, for it has a different set of strengths and weaknesses. "Classics" may reveal more about the period that so designates them than about the period that produced them; the post-World War I rediscovery of Melville is a case in point. And best-sellers, even if they can be reliably located, tell us something about readers' tastes and something about publishers' differential promotion budgets, the different prices of books, the touting of certain works by influential critics, and so forth. A sample of works published cannot reveal what people were actually reading; no retrospective study can, with any precision. But the sample reveals what editors thought

⁷ *The American Catalogue* resulted from a project directed by F. Leypoldt for the R. R. Bowker publishing house. The actual compilation, done by Lynds E. Jones, was intended to include every book "in print and for sale (including reprints and importations)" as of July 1, 1876. This initial volume appeared in 1880. It was followed by seven others, which attempted to keep the *Catalogue* up to date by recording what had been published since 1876. All books whose publication had been reported in *Publishers Weekly* were listed. I was able to locate about three-quarters of the novels selected from the *Catalogue*; the study made heavy use of the Interlibrary Loan system. The remaining quarter either were not to be found in any libraries in the interlibrary system or were not available for circulation. Thus the sample is biased against obscure works not produced in great quantity or not deemed important enough to be retained by libraries to the present day. This same bias may favor European authors, whose works had to be of some popularity to be imported by the American publishers in the first place. Regarding periodization, two volumes of *The American Catalogue* were used for each of the four periods. Period II extends four years beyond the Platt-Simmonds Act of 1891, which drastically changed the market positions of American and foreign novels by extending international copyright protection to the latter in the United States. It seems reasonable to assume that it would take about four years for the effect of international copyright to begin to be reflected in the content of novels; any other cutoff point would have been equally arbitrary. It was impossible to include popularity or sales as a variable of the sample novels because of the lack of data. Several attempts to isolate the most popular books of our period have been made; all of them acknowledge the speculative nature of trying to identify a best-seller after the fact; see the discussions in Mott (1947), Hart ([1950] 1963), and Hackett (1977). Only five of the sample novels have been identified as best-sellers: *Seth Jones*, *The Hoosier Schoolmaster*, *Brewster's Millions*, *Beautiful Joe*, and *Bob, Son of Battle*. This number is too small to allow any meaningful comparisons with the larger sample, but one intriguing fact is that two of the five are dog stories.

the public was reading, and editors earn their livings by knowing just that. Therefore, a sample drawn from the population of novels published may be assumed to have a close fit with the tastes of the contemporary readership.

One further comment on the sample: because the foreign authors are drawn only from those whose novels were published in the United States, it might be argued that American editors were selecting a particular type of foreign novel, in keeping with American tastes, and hence the foreign works in the sample do not represent faithfully what was then being published in England and elsewhere. Some selectivity undoubtedly took place. But the actual results of the comparisons to be presented show that the foreign novels in this study were in fact much like the traditional 18th-century novel; when deviations from the norm took place, they were usually on the part of the American authors. And within the sample some persistent differences between the American and the foreign authors do appear. Thus, although it would be desirable to follow up this research with an examination of novels actually published in England during the same time, I am confident that some of the differences were imported along with the texts.

The study began with two propositions. First, the overall differences between American and foreign novels might be less impressive than has often been supposed. The search for "the American novel" may in part have been self-fulfilling. For in seeking some quintessentially American literature, scholars must pass by *The House of Mirth* and seize upon *Huckleberry Finn* because of the latter's very uniqueness; novels like *The House of Mirth*, dealing with marriage, money, and the social world, are too indistinguishable from their European counterparts to lay claim to being representative of "the American novel." The Huck Finns and Moby Dicks, although written originally for a market, as is every other novel, become canonized as "classic American fiction," and go off on a special orbit, in which they are commented on by scholars, responded to by writers, and used as the exemplars and measuring rods for American fiction. Thus the distinction between the American novel as represented by these classics and the European novel may be to a considerable extent tautological. If it is, a comparison of a broad sample of American and foreign novels, keeping in mind the norm set by 18th-century English novelists, should reveal fewer differences of content and treatment than the reflection-of-national-character theories would suggest.

My second proposition was that, to the extent that 19th-century American novels did contain a unique set of themes and subjects, it was neither because American readers were not interested in novels about love and marriage nor because American authors lacked the capacity to write such novels. Instead, American authors had economic incentives to deviate

from the standard subjects of the genre. This hypothesis derives from the history of American copyright legislation.

During most of the 19th century, American copyright laws protected citizens or permanent residents of the United States but not foreign authors (Clark 1960). The result was that British and other foreign works could be reprinted and sold in the United States without royalties being paid to their authors, while American authors did receive royalty payments. Many interests in the United States benefited from this literary piracy and lobbied to maintain the status quo. (Actually, piracy is something of a misnomer, for the practice was perfectly legal.) The nascent printing industry was kept busy. Publishers made huge profits from reprinting foreign books. Readers had available the best foreign literature at low prices; for example, in 1843 *A Christmas Carol* sold for 6¢ in the United States and the equivalent of \$2.50 in England.

Of course, the two groups injured by this happy state of affairs were the European and the American authors. Native authors petitioned Congress repeatedly, gave lectures, wrote letters, and tried mightily to move a complacent public toward supporting international copyright. For, as one contributor to *Publishers Weekly* put it, "What inducement has an American publisher to publish original native works, and pay copyright on them, when he has to compete with ten, fifteen, and twenty-cent reprint editions of the best books in both current and standard English literature, on which no copyright is paid?" (Cornwallis 1886). The answer was, no inducement at all. American authors were thus excluded from much of the fiction market, and most were unable to earn their livings from writing.

After almost a century of ineffective pressure from American authors for legal relief from this competition, in 1891 the Platt-Simmonds Act extended copyright to foreign authors. Ironically, the turnabout in Congress was the consequence of the American publishers themselves being undercut by printers of "cheap books," those immensely popular, flimsy reprints of classics and best-sellers, which flourished in the 1880s (Shove 1937).⁸ The pirates were being pirated. Unable to withstand this competition, the major publishing houses suddenly became sensitive to the moral justification for international copyright and successfully pushed the passage of the act through Congress.

I contend that the choices American novelists made regarding the subjects and themes of their novels, insofar as these choices differed from those made by their foreign counterparts, were due less to the differences

⁸ Mott (1947) emphasizes the distinction between dime novels and cheap books. Dime novels, as developed and marketed by Erastus F. Beadle in the 1860s, were original works by American authors, whom we would today call hacks, writing specifically for the dime-novel publications. Their popularity paved the way for the cheap books of the 1870s and 1880s, which were reprints of foreign and, increasingly, American authors of established reputation and marketability.

in American character or experience than to different market constraints. American writers tended to be discouraged from writing about subjects that European writers were treating, for the latter had a distinct market advantage with American publishers. (An advantage that brought them no benefits, of course.) Some American novelists tried to solve this problem and win a publisher by writing about something different. In effect, the American publishing industry became a filter which held back some American treatments of traditional subjects while allowing more unusual works to get through to the reading public (Hirsch 1972; Escarpit 1971). Platt-Simmonds erased the different market positions of the two groups of authors, and the incentive for American authors to select nontraditional subjects disappeared. Therefore, I hypothesized considerable thematic divergence between the American novels and the foreign novels published in the United States prior to 1891, and convergence after that year.

I looked at the differences between the American and foreign novels of the sample during the four time periods, the first two falling largely before Platt-Simmonds, the second two after. Four specific predictions are based on the two initial propositions:

1. During the earlier periods, the novels by foreign authors should be significantly cheaper than those by Americans; this difference should disappear in the later periods.

2. For the earlier two periods, the sample should contain more foreign novels than American ones; during the later periods the reverse should be true.

3. Overall, there should be more consistency than variation between the American and foreign authors' novels, and both groups should resemble the standard novel that deals with love, marriage, and money.

4. Some plot variations between the foreign and American novels in the earlier two periods should decrease or disappear during the later two. This decreased variation represents the hypothesized convergence following 1891.

III

Competition with 10¢ and 15¢ cheapbooks, plus the 20 years of recessions that began with the Panic of 1873, kept the price of books low until the turn of the century. For the sample novels published in Period I, the average price was \$0.83, and it remained \$0.83 for Period II. A more vigorous economy and the demise of the cheapbooks brought the price up to \$1.20 for Period III and to \$1.27 in the final period.

But this picture of 20 years of steady prices followed by 15 years of increases conceals what was happening within the book market. As figure 1 shows, during the first of the four periods, books by American authors

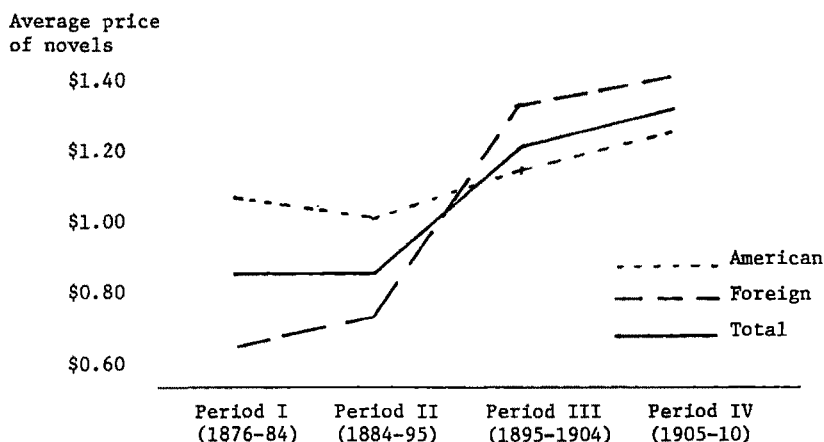


FIG. 1.—Price of novels for four periods

were considerably more expensive than those by foreign authors, their respective average prices being \$1.04 and \$0.64 for the sample novels. This difference was maintained in Period II, though in somewhat diminished strength. It was only during the last two periods that the foreign novels exceeded the American ones in price. In Period III, foreign novels averaged \$1.25, American \$1.17; while in Period IV, foreign novels sold for an average \$1.38, American for \$1.22.

The cause of the abrupt switch in the relative prices of foreign and native works was international copyright. Before 1891, foreign reprints were both cheaper in the hardcover format and dominant in the cheap-book field. Following 1891, foreign works were no longer less costly for publishers to produce, cheapbooks disappeared, and foreign reprints began to sell for slightly more than their American counterparts.⁹

The impact of international copyright was also reflected in the numbers of American and foreign novels published. In 1894, for the first time, the majority of novels published in the United States were by Americans (Tebbel 1975, p. 687). The numbers of foreign and American authors of the sample novels followed the expected pattern (table 1). Foreign authors dominated during the first two periods and were in the minority thereafter.

It is madness to feel that I am no longer pure. Am I to live forever, a guilty thing? Is there no escape for me through all life? Cannot this evil on the threshold of my life be repaired?

⁹ *Publishers Weekly* joyfully proclaimed the "almost entire cessation of the cheap and undesirable fiction—French and English—appropriated by piratical publishers and printed in villainous typography and worse paper in ten, fifteen, and twenty-five cent 'libraries'" (*Publishers Weekly* 1893, pp. 1063-64).

American Character and the American Novel

Aye, yes! Granger shall wed me! He shall make me his wife!
[Ingraham 1876, p. 50]

Love, marriage, and seduction were the subjects of the 18th-century novels. I was interested in seeing whether these original preoccupations had persisted, and what forms they had taken by the time under consideration.

Adult heterosexual love continued to be of overwhelming importance, being the key to the plot in 55% of the sample novels and of considerable importance in an additional 33%. The love complications usually revolved around the question of marriage, important in 64% of the novels. Matters of love and marriage generally worked out in satisfying, unsurprising ways. Most often, the marriage took place as anticipated, though occasionally one or both of the expected partners married someone else.

Other forms of love were given less, though considerable, attention. Love between adult members of the same sex was seldom the center of the novel's action but was of some importance in over 40% of the novels, and a similar proportion of them dealt with the love between an adult and a child. And although love and marriage were nearly omnipresent, seduction, the keystone of Richardson's novels and many to follow, was not a standard feature of the sample novels, figuring in about one-fifth of the plots.

American and foreign authors did not differ in their emphases on love and marriage, and both groups wrote about these subjects in the majority of their novels. Nor was there any evidence of an increase or decrease of stress on these subjects over time, although for both groups of authors there was a slight decline in Period II. There were no differences between American and foreign authors about the likelihood of the anticipated marriage occurring, the emphasis on love between adult members of the same sex, or the importance of love between adults and children. These last two findings cast doubt on Fiedler's stress on the homoeroticism of

TABLE 1
AUTHOR NATIONALITIES

Period	American Authors	Foreign Authors	Total
I.	19	21	40
II.	11	21	32
III.	13	8	21
IV.	24	12	36
Total.	67	62	129*

* It will be recalled that the sample numbered 130 novels. The nationality of one author, Linda Churchill who wrote *Interweaving*, has eluded identification.

American novels, on the one hand, and Douglas's stress on the Americans' excessive sentimentality, on the other.¹⁰

Of the protagonists, 60% were male (table 2). Both American and foreign authors generally favored heroes instead of heroines, and this overall male predominance occurred in all four periods. Although neither the nationality of the author nor the period in which the novel was published accounts for the masculine predominance among protagonists, there was a strong relationship between the sex of the author and the sex of the protagonist.¹¹

The Douglas "feminization" thesis does not seem to be supported in the sample. The majority of both American (55%) and foreign authors (64%) were men. Men wrote about male protagonists in three-quarters of their novels, and women wrote about women in more than one-half. The difference between the two may indicate that, although it is easier for any author to write about members of his or her own sex, male protagonists were regarded as somewhat more interesting in general. Thus close to half of the female authors were induced to write about heroes rather than heroines.¹²

TABLE 2
PERCENTAGE OF AUTHORS WITH
MALE PROTAGONISTS

AUTHORS	PERIOD			
	I	II	III	IV
American.....	47	73	69	67
Foreign.....	57	76	50	42
Total.....	52	75	62	58
<i>N</i> novels.....	40	32	21	36

NOTE.—Sex of author \times sex of protagonist—male authors: male protagonist = 74%, female protagonist = 26%; female authors: male protagonist = 41%, female protagonist = 57%.

¹⁰ There is no denying that American editors advocated the sentiment in novels that their readers favored. Smith (1978) tells how even Henry James was bullied into supplying a happy ending for *The Europeans* after he had resisted doing so for *The American*. The question is whether American novels, as a whole, were any more sentimental than English or Continental ones, as a whole; it is not appropriate to compare *The Lamplighter* with *Madame Bovary*. The sample does not indicate any specifically American favoring of sentimentality.

¹¹ The tables include only raw percentages. The majority of the differences, when corrected for the relatively small *N*'s, were not statistically significant below the .05 level, generally agreed to be persuasive in social science. Therefore my argument about differences and convergences rests on a common pattern of percentage differences rather than on the presence or absence of statistical significance. Some findings were significant, however; these are given in Appendix B.

¹² I also found the author's sex to be associated with whether the action was located outside the home, whether climactic scenes were located outdoors, and whether racial

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Young adults constituted 60% of the protagonists, somewhat over 10% were middle-aged, and an additional 10% were followed through several stages of their lives. This emphasis on young adulthood, roughly defined as 18-30, held true for both American and foreign novels and did not change over time. The vast majority of our protagonists were single at the beginning of their respective novels (table 3). By the end of the novels, most were married or about to marry. Period II, with its lesser emphasis on love and marriage, deviates from the pattern somewhat, but overall, the trend for protagonists to exchange their initial single status for a married one is clear.

Paul liked to saunter through [his father's] shops, looking at the work-people, and talking with them in a half supercilious, half hail-fellow way; it added to the consciousness of his own importance. . . . To a company of young girls shut up in a close room, to ply one monotonous task from the beginning of the year to its close, the advent of a handsome, polished young man was a very pleasant event. It must have been humiliating, if they remembered the fact that outside of that shop he never recognized them; they did not belong to "his set." [Ames 1870, p. 16]

The novel is traditionally regarded as the genre read by, and concerned with, the middle class. Surprisingly, though, the majority of the sample novels did not feature middle-class protagonists (table 4). The American authors were significantly more apt to have protagonists begin in and, especially, end up in the middle class; the foreign authors favored upper-

TABLE 3
MARITAL STATUS OF PROTAGONISTS (%)

STATUS	PERIOD AND AUTHORS							
	I		II		III		IV	
	American	Foreign	American	Foreign	American	Foreign	American	Foreign
Single at beginning of novel	84	95	91	81	92	88	88	82
Married or engaged at end of novel	68	67	36	43	54	75	68	46
N novels		40		32		21		36

minorities were present; all three elements were significantly more apt to appear in novels by male authors than by female. This cluster of attributes is explained by the male-authored adventure stories, such as those about military exploits, Indian fighting, exploring Africa, and so forth.

class protagonists. Both groups of authors seemed to feel that working-class characters were not likely to be interesting protagonists. The difference between American and foreign authors regarding the protagonist's class at the beginning of the novel was greatest in Period I and diminished thereafter. The difference in outcomes was more persistent, Americans being significantly more apt to have the hero or heroine end up in the middle class during all periods except the third.

Sample protagonists rose in social ranking in about one-quarter of the novels, occasionally descended, but usually (68%) stayed put. Again, a breakdown by period is revealing. In the first period, American authors were significantly more inclined to depict social mobility; 53% of the American novels presented socially mobile protagonists, compared with 19% of the foreign novels. *Eirene* (Ames 1870) is typical: the heroine starts out as the daughter of a poor but loving farm family, goes through hardships as a hand in a New England textile mill, and ends up in a New York mansion, married to the scion of one of the city's old Dutch families.

TABLE 4
SOCIAL CLASS OF PROTAGONIST (%)
A. CLASS AT BEGINNING AND END OF NOVEL

PART OF NOVEL AND CLASS	AUTHORS	
	American	Foreign
Beginning:		
Poor.....	22	18
Working.....	13	15
Middle.....	40	21
Upper.....	24	44
Other.....	...	3
End:		
Poor.....	5	8
Working.....	12	13
Middle.....	49	23
Upper.....	34	50
Other.....	...	5
<i>N</i> novels.....	67	+ 62 = 129

B. PROTAGONIST IN MIDDLE CLASS

PART OF NOVEL AND AUTHOR	PERIOD			
	I	II	III	IV
Beginning:				
American author.....	42	46	39	36
Foreign author.....	14	24	38	18
End:				
American author.....	53	64	31	48
Foreign author.....	19	29	38	9
<i>N</i> novels.....	40	32	21	36

But in the later three periods, the American and foreign authors are nearly identical on this variable, treating social mobility in about one-third of their novels.

In all periods, in both native and foreign novels, the protagonists were unambiguously good people, with fewer than one-fifth having bad or mixed characters. Some of the goodness of the characters may have come from good breeding; heredity was stressed in about 30% of the novels. Most protagonists in both American and foreign novels engaged in wide social interaction, contrary to what the Fiedler theses would lead one to expect. Society and social pressures gave rise to the central conflict in about one-quarter of the novels; more often the conflict originated with an individual antagonist. The resolution of the conflict was generally the result of the protagonist's own character or resourcefulness, although he often had help from a benefactor.

The impulse which moved us to write this book was primarily indignation—indignation at facts. . . . Convict 9009—John Collins—exists only in our imagination. But everything that happens to 9009 within the prison is something which has happened to some convict in some prison (American prison) some time. . . . The result, we think, is a simple, clear, compressed story, all of action, which shows how Society creates a Monster. [Hopper and Bechdolt 1909, preface, pp. vii–viii]

Prison conditions, temperance, the treatment of women, the plight of the poor, and cruelty to animals were subject to the reforming zeal of the sample authors. Social reform was an important theme in 39% of the American novels and 29% of the foreign ones. The difference between the two groups of authors was greatest in the first period, when American authors dealt with social reform in 69% of their novels, foreign authors in 40% of theirs. Thereafter, the two groups converged, with about one-quarter of their novels dealing with reform.

About one-third of both American and foreign novels were set in large cities. Americans favored small towns as well, locating 30% of their novels in small towns, a setting less popular with foreign authors (15%). The American preference for small towns was strongest in Period I, in which 53% of their novels had small town settings. Few novels were set in the wilderness and, contrary to the Fiedler and Lawrence arguments, Europeans were slightly more likely to utilize wilderness settings (15%) than were Americans (7%). Of the American novels, 75% were set in the United States; of the foreign ones, most of them written by Europeans, 78% were set in Europe.

Foreign authors were likely to set their action in the home (57%),

Americans were likely to set it elsewhere (66%). This difference, persisting through all four periods, seems to contradict the Douglas argument, although Fiedler presumably would not be surprised. For both American and foreign authors, the outdoors was the second most popular setting; both groups used it for about one-quarter of their novels. The Americans also set another quarter of their novels in institutions other than the home, such as the penitentiary of "9009" (Hopper and Bechdolt 1909) or the New England women's college of *Two College Girls* (Brown 1889).

The action generally took place in the present or the immediate past. Foreign authors showed somewhat more interest in exploring the remote past, especially in the first two periods, in which about one-quarter of the foreign novels took place before the 19th century. The difference disappeared in the last two periods.

Money, the necessity of having it, its acquisition, and occasionally its loss, was a central element in 57% of the plots. During the first three periods there was a considerable difference between American and foreign authors, with the latter writing about money in three-quarters of their novels, Americans in about half. Period IV shows an unexplained drop in the importance of money in foreign novels (27%), so while convergence may be taking place, the trend is by no means clear.

About one-third of the sample novels dealt with religion, and there seems to have been an uneven decline in its importance over the 35 years. The American and foreign authors, Douglas and Lawrence to the contrary notwithstanding, were equally likely to write about religion. Supernatural elements, the ghosts and gothic devices that Fiedler argued Americans substituted for the excitements of passionate adult love, played a role in only 15% of the novels. Foreign authors were somewhat more likely to include supernatural elements during the first two periods than were Americans; during the last two periods, the two groups converged.

Humor is often cited as a characteristic of American literature. Although only 11% of the sample novels contained significant humor, the American novels were slightly more apt to be humorous (16%) than the foreign ones (6%). This difference between the two groups of authors was most pronounced in the first two periods, during which no humorous novels by foreign authors appeared in the sample.

Nonwhite characters appeared in about one-third of the novels; Americans were more likely to include them, especially in the last two periods, during which a number of Asian characters appear in American novels (table 5). In general, Americans tended to write about blacks, while foreign authors divided their attention among blacks, American Indians, and other racial minorities. Americans were more apt to write about unidimensional figures, the minority character being purely evil, virtuous, humorous, or appearing only in a supporting, servant role. Foreign authors,

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TABLE 5
RACE: NONWHITE CHARACTERS IN NOVELS (%)

AUTHORS	PERIOD			
	I	II	III	IV
American.....	19	27	62	47
Foreign.....	20	33	13	25
<i>N</i> novels.....	40	32	21	36

although less apt to include a nonwhite character, seemed more inclined to endow those they did include with some degree of complexity.

The following summarizes the similarities, differences, and convergences discussed in this section:

Areas of consistency between the two author groups:

- Protagonist's marital status at beginning
- Protagonist's marital status at end
- Protagonist's age
- Protagonist's sex
- Protagonist's overall character
- Protagonist's social interaction
- Importance of adult, male/female love
- Importance of marriage
- Importance of adult love between members of the same sex
- Importance of adult/child love
- Importance of religion
- Importance of heredity
- Importance of seduction
- Urban settings

Areas of convergence after initial differences:

- Social mobility of protagonist
- Protagonist beginning in middle class
- Importance of social reform
- Use of small town settings
- Use of the remote past
- Importance of supernatural
- Presence of humor
- Importance of money (?)

Areas of persistent differences between the two author groups:

- Setting (American vs. European)
- Treatment of race (increasing divergence)
- Use of domestic settings
- Protagonist ending up in middle class

IV

A sociological approach to literature assumes that literary works are in some way linked to the society that creates and/or reads them (Escarpit

1971). Reflection has been a popular metaphor in the attempt to explain recalcitrant literary phenomena. This study demonstrates the need for an expanded conception of how literature reflects the social world.

What have the sample novels reflected? The most consistent finding is the mutual resemblance among the novels, both between those by American and by foreign authors and among those of all four periods. And, lest this be regarded as an artifact introduced by American publishers seeking any novels, native or foreign, that catered to some unique American reading tastes, one should remember that the characteristics shared by the sample novels are much the same as the 18th-century archetypal features of the genre. Like their forerunners, the sample novels are about love and marriage, money and social life. Their protagonists start out single and end up married. They operate in a complex social world familiar to their readers, they are basically virtuous, they encounter a sequence of emotional and moral dilemmas, they often improve their lot.

This resistance to changes of subject matter that novels seem to possess is especially striking when one considers the self-imposed limits of the genre, the many things about which novelists do not write. Missing most conspicuously is material related to work, such as career histories or the depiction of on-the-job working relationships. Also missing is an intensive examination of married life, posthoneymoon, especially as it involves the rearing of children; this may be the feminine counterpart to the absence of novelistic treatment of male work. While it is true that novels present much of the quotidian detail of life, only a brief phase of life seems to be of interest: the stage when the young adult is making decisions regarding a marriage partner. Once this critical juncture has been passed, the next 40 or 50 years presumably take care of themselves. Novels are practical comedies: they end with a marriage, and are much concerned with money and with other means of achieving that marriage.¹³

Much of this continuity of subject matter can be explained by considering what I shall call the imperatives of the genre. The novelist has a formal problem very different from those of the playwright or poet. Inasmuch as lines of prose do not require sounding out and their individual words do not, as a rule, demand close attention, prose can be read quickly and without intense concentration on the part of the reader. Reading poetry is hard work (although, Johan Huizinga [(1950) 1955] would add, it is also play). Reading novels is easy.

But the fact that novels are long means that they are normally not

¹³ R. J. Dorius has discussed this practical nature of comedies. "By and large, comedy is a more secular and earthbound form than tragedy. It concentrates upon the local, established, familiar, upon problem-solving, the probable rather than, as in tragedy, upon mystery and wonder." One could easily substitute the word "novel" for "comedy" in this description (Dorius 1965).

consumed in one sitting. Novels require a considerable investment of time, and several decisions to sit down and read rather than do something else. This poses a formal problem for the novelist: he needs not only to attract the initial interest of his reader but also to influence a subsequent series of decisions. Working within such a genre, the novelist cannot orchestrate an emotional tension and release, such as that of tragic catharsis; he cannot set the stage and structure the emotional experience to provide his audience a brief transportation to another world. The novelist's problem is to interest the reader enough so that he keeps deciding to pick up the novel again and to enable the reader to slip easily back into the novel's world at almost any point.

One device by which the novelist can facilitate easy reentry into his novel is to include in it a number of connection points with the reader's own life and experiences. Love, money, marriage decisions, familiar terrain, and a recent time period—all of these are immediately recognizable to the average reader. Literature transports, but the readers of novels are not transported very far. These multiple connections with the reader's own experience enable him to pick up the novel after several days' neglect and quickly reenter the world of its characters, for it is a world very much like his own.

But the connection points must also be able to attract the average reader's interest in the first place. Most aspects of most lives are not sufficiently dramatic to satisfy the need for escape that we desire in an aesthetic experience. The novelist must write about some aspect of common life that is emotionally engaging, that arouses his reader's feelings and curiosity. And this curiosity must ultimately be satisfied, so the novelist needs a subject that lends itself to dramatic shaping by having a distinct climax and resolution.

Love, especially love associated with the selection and winning of a marriage partner, is the perfect solution. It is familiar to most readers, it is full of intense feeling and emotional conflict, and—unlike working life or child rearing, which share those two features—it reaches a definite resolution. When recounting a love affair or possible marriage, one can always say "how it turned out." Love and marriage solve the novelist's formal problem by offering both connection points and dramatic structure. The imperatives of the genre compel many novelists to give love and marriage an inordinate amount of attention, disproportionate to the amount of time they occupy in most people's actual experience. The sample novels reflect this. Many of the resemblances among novels stem from these formal imperatives.

But although the sample novels share a preoccupation with love and marriage, they also exhibit some differences between the works of American and foreign authors. Most of these differences—social reform themes,

the class of the protagonist, the concentration on small towns, the depiction of the remote past, the treatment of money, the presence of humor—follow a common pattern: the difference between the American and the foreign authors is most marked during the first one or two periods, and during the later periods the two groups converge. In many cases of early divergence, it was the American authors who deviated from what we regard as the standard subjects and treatments of the novel, writing more about social reform and less about money, using more humor, and so forth.

If these differences had persisted over all four periods, they might have constituted a reflection of some peculiarities of the American character or experience. But the pattern of differences in the 1870s, which then converge sometime in the 1890s, supports the proposition that what was being reflected in the initial differences were the different market positions occupied by the American and the foreign authors. Both were subject to the imperatives of the genre and the traditions of the novel. But the American authors had greater incentive to deviate from the norm, to write on nontraditional themes that the European authors had not effectively monopolized. After 1891, there was no longer the same incentive for deviation, the novelistic imperatives took over, and the American authors swung into line with everyone else. So in addition to reflecting imperatives of the genre, the novels reflected differential market positions brought about by the state of American copyright laws.¹⁴

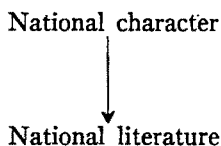
Concerning race, social class, and the use of domestic settings, as well as national setting, the sample showed persistent differences between the American and foreign novels. These differences support a "literature reflects national character" argument and point the way toward further research. The difference with respect to race is perhaps the easiest to explain. Both in the tendency to include minority characters and in the unidimensional treatment of them, American novels were different. This difference may reflect the American experience with racial minorities, non-white persons being present in the life of the average American to a far greater extent than they were for the average European. It may reflect

¹⁴ The argument might be made that something else happened in the 1890s that brought Americans into the European cultural tradition to a greater extent than before. This would be the "American enters the world" view that often cites Henry James as an example of the increasing sophistication of American letters during the late 19th century. But I venture the opinion that any such sudden increase in European influences took place only at the highest literary levels; it did not affect popular literature to any great degree, and thus would not be likely to influence the broad sample used here. Furthermore, the copyright argument is given support by the fact that American authors were intensely aware of the debate over international copyright and its effect on their own practices and opportunities; Clark (1960) documents many of the letters, speeches, and arguments of prominent and obscure authors on the subject. Therefore, some of the post-1891 change in American novels may have been quite deliberate.

also a national uneasiness about race, a need to categorize racial minorities into stereotyped roles so as to avoid encountering them as full, complex human personalities. Two specific historical events of the late 19th century may be playing a part here: the consolidation of Jim Crow laws some 20 years after Reconstruction, and the American imperial adventures in the Far East and the Caribbean. Both of these confronted Americans with the issue of their relationship with other races yet made it desirable to deny the full humanity of nonwhite persons in order to escape the moral contradiction that would follow if the nonwhites were regarded as equals. I would hypothesize that the American treatment of racial minorities, qualitatively thinner but quantitatively greater than that of foreign authors, persisted until the mid-20th century, reflecting what has long been called a peculiarly "American dilemma." Similar "reflection of American character" accounts might be developed, and tested, for the relative American preference for middle-class protagonists and disinclination toward domestic settings.

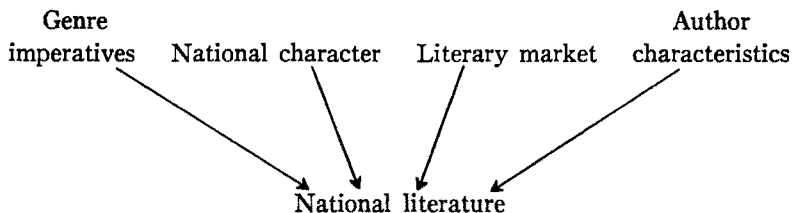
Thus I have tried to show that while reflection is a convenient metaphor with which to conceptualize the links between a society and its literature, we need a model of reflection complex enough to encompass all of the various types of reflection. Even if we confine ourselves to regarding literature as a dependent variable, which is to look at, in Weber's words, "only one side of the causal chain," we find reflection not to be simply as in *A* below:

A



Instead, reflection must encompass at least the variables shown in *B* below:

B



This expansion is quite in keeping with the reflection metaphor. For, after all, a mirror reflects not just one thing but whatever is placed in

front of it. The sociologist of literature must place a variety of subjects in front of the literary mirror in order to realize the full potential of reflection theory.

APPENDIX A

Summary of Coding Questions and Reliability Controls

- I. Characteristics of protagonist:
 - Sex
 - Age
 - Class at beginning of novel
 - Class at end
 - Interactions with other people
 - Character (ultimate)
 - Initial presentation
 - Heredity as factor in determining character
 - Marital status at beginning of novel
 - Marital status at end of novel
- II. Questions pertaining to love, marriage, seduction:
 - Adult love—male/female—importance to plot
 - Adult love—same sex—importance to plot
 - Love between adult and child—importance to plot
 - Marriage's importance to plot
 - Does the anticipated marriage occur
 - Was seduction a significant element in the plot of the novel
- III. Questions pertaining to plot and treatment:
 - Setting of main action
 - Location of main action
 - Time period during which novel is set
 - Location of general action
 - Location of climactic scenes
 - Importance of money in the novel
 - Importance of religion in novel
 - Importance of supernatural (ghosts, voices, visions, dreams, etc.)
 - Importance of social reform theme in novel
 - What nonwhite characters, if any, appeared in the novel
 - How were the nonwhite characters presented
 - Did this novel seem intentionally humorous

One of the reasons why the content of large samples of novels has seldom been studied by sociologists is the simple fact that novels take a

long time to read. Even under the injunction to skim, looking for certain elements for coding, readers required two to three hours per novel for just reading, summary preparation, and coding. The project was feasible only with multiple readers/coders, of whom there were 10. Hence problems of reliability had to be addressed.

Reliability controls included training, checking, and keeping obscure the original hypotheses. First, the project began with all of the coders reading and coding the same novels and discussing the reasons for whatever differences appeared in their coding. Some questions were straightforward, for example, the sex of the protagonist; for questions requiring more judgment on the part of the coder, we developed a series of decision rules. For example, several questions ask whether something such as money "was important in the novel." Our rule here was, if the plot would be considerably different in the absence of this element, code it as significant; if the element is occasionally discussed but is not necessary to the development of the plot, code as insignificant. All questionable codings were brought up and discussed by the group; I made the final decision in ambiguous cases. (Some of the questions that arose were quite unforeseen, such as, "How should we code protagonist age, sex, and class when the protagonist is a dog?") I think an acceptable degree of standardization of our coding was achieved by this procedure.

Second, each coder wrote a detailed summary of the plot for each novel. Other project members read each summary and compared it with the coding for the novel; questions that arose were reconciled with the original coder.

Finally, none of the coders knew the nature of the comparisons I was interested in making. Nor, usually, did they know any author characteristics beyond perhaps sex, for the author's origins were coded after the novel's contents. Thus it is reasonably sure that no bias in favor of the hypotheses was present.

Despite the efforts to increase reliability and prevent bias, data on the content of novels are inevitably messier than, say, data on voting patterns. I have not imposed sophisticated statistical tests beyond those the data warranted. As footnote 11 indicates, I think it is appropriate to err on the side of modesty in using such data. In the exploration of interesting questions, sometimes less than perfect techniques must be tolerated.

I would be happy to provide the full coding sheets and the list of sample novels on request.

APPENDIX B

TABLE B1
VARIABLES SIGNIFICANT AT .05 LEVEL

Variables	Significance	Strength of Association	Table*
By AUTHOR GROUP (AMERICAN VS. FOREIGN)			
Protagonist's class at beginning:			
All periods.....	.051	Cramér's V = .215	4
Periods I and II.....	.049	ϕ = .263	
Protagonist's class at end:			
All periods.....	.007	Cramér's V = .276	4
Period I.....	.051	ϕ = .352	
Period II.....	.037	ϕ = .428	
Social mobility of protagonist:			
period I.....	.059	ϕ = .352	...
Action located in the home:			
All periods.....	.012	ϕ = .236	...
Period III.....	.023	ϕ = .596	...
Humor: Periods I and II combined	.023	Cramér's V = .337	...
Location of action: Europe vs.			
United States.....		Not included in tables, but this difference was signif- icant below the .01 level for all periods.	...
By AUTHOR SEX			
Sex of protagonist.....	.0007	Cramér's V = .338	2
Action located in the home.....	.021	ϕ = .220	See n. 12 above
Location of climactic scene outdoors	.015	ϕ = .232	See n. 12 above
Inclusion of racial minorities.....	.031	ϕ = .228	See n. 12 above

* Several tables are not included here; I will provide them on request.

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Marriage, Women, and Social Stratification: A Theoretical Critique¹

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In recent years, sociological research has been concerned increasingly with patterns of intergenerational and marital mobility of women, patterns of marital homogamy, and consequences of marital heterogamy. This research can make further substantial contributions only if it becomes integrated into a more adequate theoretical framework. In the first part of the paper, elements of such a framework are developed. Social stratification is conceived of as a process of differential association emerging as a complement to that of class formation in order to develop and preserve collective social identity within a world characterized by pervasive economic inequality. The outcome of this process is a long-term macrosocial reproduction of inequality when it penetrates two strategic forms of social relations, parent-child and husband-wife relationships. The second part of the paper applies this theoretical approach to the marital relationship by discussing traditional and alternative hypotheses about stratification mechanisms within the decision to marry, patterns of marital homogamy, and consequences of marital heterogamy. The new framework avoids some problematic assumptions of the dominant tradition of research and is able to provide a more adequate explanation for many empirical findings.

In recent years, sociologists have been concerned increasingly with the role of women in processes of stratification and mobility. A series of studies has concentrated on patterns of marriage (Pavalko and Nager 1968; Rubin 1968; Elder 1969; Glenn, Hoppe, and Weiner 1974a; Chase 1975) and intergenerational mobility of women (Tyree and Treas 1974; Treiman and Terrell 1975; Featherman and Hauser 1976; McClendon 1976). In these studies, a number of objections are leveled against much of the earlier work in this area because of its neglect of the role of women. In a related line of research, empirical data have been collected con-

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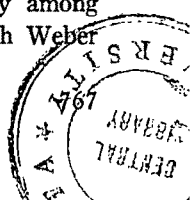
cerning the problem of the unit of stratification analysis and the role of the woman within marital and familial life (Barth and Watson 1967; Haug 1973; Felson and Knoke 1974; Rossi et al. 1974; Ritter and Hargens 1975). Whereas theorists traditionally have maintained that the family is the true unit of social stratification (Schumpeter 1951; Parsons [1953] 1966, [1940] 1954a; Barber 1957; Goode 1966), recent critics have argued that this position is no longer tenable (Watson and Barth 1964; Acker 1973; Safilios-Rothschild 1975). The aim of this paper is to answer the question whether one can retain the emphasis on the family as the unit of stratification and at the same time allow for the increasingly active role of women in the public and private world. It is argued that this is possible but only within a revised theory of stratification which is outlined in the first part of the paper. In the second part, this framework is applied to the relationship between marriage and social stratification and the role of the woman therein.

ELEMENTS OF A THEORY OF SOCIAL STRATIFICATION

In order to analyze the role of marriage and family in the process of stratification, it is first of all necessary to elaborate the concept of stratification. Many observers have noted that the frequent use of the concept of social status stands in contradiction to a lack of theoretical clarity (Rose 1958; Burchard 1960; Coleman 1966; Cicourel 1970; Lasswell and Benbrooke 1974; Otto 1975). In this paper, I argue that we should distinguish between two types of mechanisms whereby social inequality is generated and transformed through time, namely, social stratification and class formation. This conceptual distinction, well-known in classical German sociology (Tönnies 1966; Weber 1968), has been taken up again recently by several authors (Martindale 1972; Bendix 1974; Krauss 1976; Hechter 1978). Its relevance is documented by the fact that many studies of social stratification (Pfautz and Duncan 1950; Coleman 1966; Blau and Duncan 1967; Lasswell and Benbrooke 1974) and class formation and conflict (Dahrendorf 1959) seem to presuppose it. In this part of the paper, therefore, I attempt to (1) elaborate a concept of social stratification, (2) show how social stratification is linked to structures of social inequality and class, and (3) demonstrate that the parent-child and the husband-wife relationships are the central features of social stratification.

Deference as a Mechanism of Identity Maintenance in Social Interaction

Social stratification is understood here as referring to those processes whereby social honor or prestige becomes distributed unequally among the members of a society. But in spite of the importance which Weber



(1968) and other scholars in his tradition attributed to this notion, they did not elaborate a theory of social honor (Parkin 1971, p. 33). If we are to develop such a theory we are faced with at least three questions: What are the social functions of the differential distribution of honor? What are the bases for granting honor to persons and collectivities? In what kinds of social action is social honor realized? The first of these three questions will be dealt with in this, the second and third in the subsequent two, sections.

What are the social functions of the differential distribution of honor? My first argument is that these functions are related primarily to social interaction—a relationship largely neglected up to now (Cicourel 1970, p. 13). If we want to understand the functions of the differential distribution of social honor in society, we have to look at the characteristic features of social interaction. Two theoretical approaches to the study of social interaction may be distinguished: the theory of the "socialized actor" and that of "social identity" (Cancian 1975). They suggest two different points of departure for the analysis of social stratification in terms of interaction. In the social identity approach, which I follow here, the peculiar character of social interaction is seen in the fact that human beings interpret and define each other's actions. The key feature is that a human being is thought of as having a self or an identity, which implies that he can be the object of his own actions (Blumer 1962). It is assumed, in this approach, that individuals are motivated to maintain particular identities. Social norms are collective beliefs about what actions will cause others to validate a particular identity. Here, conformity to norms is essentially a mechanism for validating one's identity, that is, an instrumental mechanism and not a mechanism adapting individuals to a coherent, common value system (Cancian 1975, pp. 137–39). In this framework, it is obvious to connect the mechanism of social stratification with interaction processes and, in particular, with forms of intimate social interaction. From this point of view, we can agree with Shils (1962, p. 767), who writes that face-to-face relationships are of particular significance in the stratification system and that individuals can perceive their own status in the judgment of others only within such relationships.

However, social stratification would hardly become a fundamental and durable social process if it operated only in the transitory encounters of everyday life. In fact, the participants in such encounters try continually to establish control over these processes of interaction by restricting them to certain categories of persons, that is, by establishing social boundaries. These boundaries have the twin functions of preventing "unequal" people from entering one's own sphere and restricting one's own interaction possibilities to "equals." Social boundaries, thus, exclude others, but they also create and preserve identity. In this way, social distance between individ-

uals and collectivities emerges. Social distance as understood within such an interaction framework (see McFarland and Brown 1973) thus defines those persons as distant from or near to each other who avoid or maintain a variety of intimate and close personal relationships (van den Berghe 1960; Banton 1960; Ingham 1970; Schmitt 1972; Lopata 1973). Through such forms of "distancing" or "social closure," the highly discontinuous and intermittent character of deference behavior and status symbols (see Goffman 1951, 1959; Shils 1969) is transformed into persistent social structures.

These considerations lead to a view of social stratification as a mechanism establishing boundaries on social interaction. Social distance emerges between acting units with different degrees of participation in, or possession of, social goods (Krauss 1976) insofar as these goods or "macrosocial properties" (Shils 1969) are the basis for the formation of interaction patterns and identity. Social distance is thus seen as emerging because particular groups have distinct life experiences and develop specific views of themselves and their society. A common position in the macrosocial structure of inequality leads to the development of similar identities among the members of a group who are characterized by close interaction indicating that they consider each other equals. From this point of view, the formation of social strata is in a certain way a "microsocial" phenomenon. In its self-constitution, a deference stratum is concerned primarily with the boundary lines it draws about itself and less with those defining other social strata (Shils 1969, p. 338; Coleman and Neugarten 1971, p. 264).

A more or less coherent deference system develops and is accepted throughout a society because there is only a restricted amount of social wealth or social goods and because, in general, the higher the stratum the greater the share in most of those goods. The structure of the distribution of these goods and their relation to social interaction touches the second issue to be discussed here, namely, that of the mechanisms leading toward social inequality.

The Reproduction of Social Inequality through Class Formation and Social Stratification

According to Shils (1962, 1969), those macrosocial properties of an individual are deference entitling that have "charisma," that is, provide a person with a sense of participation in the value center of society. A large number of social characteristics can have such charisma: education and occupation, ethnicity, religion, area of residence, and so forth. Parsons ([1953] 1966, [1940] 1954a, 1970) and his followers (Barber 1957; Lauermann 1966, 1973) and some other theorists (Parkin 1971) reduce these macrosocial properties to one fundamental dimension, the structure of oc-

cupational positions. In this approach, occupational positions, not individual persons, are the units of evaluation. This idea is incompatible with the assumption of the social identity approach that acting individuals or collectivities are the main units in deference behavior.

The question to be concerned with here is how to conceptualize (1) the relevance of these "macrosocial properties" for stratification processes, (2) the internal structure of these properties, and (3) the social forces determining the emergence, stabilization, and modification of these structures. Up to now, I have referred rather loosely to macrosocial properties as the bases for social stratification. The question is, however, how the internal structure and the relative importance of these properties can be grasped theoretically. This leads us to the second issue raised above, namely, the causes of the emergent structures of social inequality. What is necessary here is to specify the particular function of social stratification within the totality of the processes contributing to the long-term reproduction of social inequality. To do so, it is necessary to introduce another concept, that of social class or class formation. In order to clarify the relation of this concept to that of stratification, two levels of analysis may be distinguished, namely, the distributive and the relational levels. The first tells us how different "social goods" (Krauss 1976, pp. 9-10) are distributed among the members of a society. All those scarce products of human work which people strive for can be considered such goods. The reproduction of social inequality in the sense of the social production and distribution of these goods, however, can be understood only by reference to the second level of analysis, that of social relations. It is through the processes of class formation and social stratification that social inequality as a distributive feature is maintained and transformed through time. The central characteristics of the two processes may be summarized as follows (see also table 1).

Class formation and conflict have their roots in the economic sphere because they concern the forms whereby scarce socioeconomic goods are produced and distributed among social groups and sectors. As such, they involve not only the immediate spheres of production and distribution—enterprises, firms, and markets—but also the whole range of social and political institutions created in order to ensure the functioning of the economy and to strengthen the relative bargaining power of the interested parties. A central feature of class formation is the degree to which the collective groupings located within specific positions in the relations of production and distribution become aware of particular interests which are "strategic" insofar as they are contrary to those of an opposite class. Today, in most advanced industrial, capitalist societies, the typical form of interaction between classes can be characterized as "antagonistic co-

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TABLE 1
CHARACTERISTIC FEATURES OF CLASS FORMATION
AND SOCIAL STRATIFICATION

	Class Formation	Social Stratification
Primary objective.....	Production and distribution of scarce socioeconomic goods	Construction and maintenance of individual and collective social identity
Place of pursuit.....	Economic (enterprises, markets) and political sphere	"Private" sphere: families, schools, neighborhoods, and other "social circles"
Acting units.....	Employers, managers, workers, and their organizations	Individual persons, families, and voluntary associations
Main reference units.....	Members of the opposite class	Members of own stratum
Typical kind of relationship within/between classes/strata.....	Solidarity and/or competition; antagonistic cooperation	Sympathetic personal relationships; role-specific, utilitarian interactions
Time perspective.....	Economic, social, and political history	Individual life and intergenerational family history

operation" insofar as it is based on acceptance of the existing socioeconomic order (Montague 1956).

It is easy to see how the particular features of social stratification differ from those of class formation. The latter aim deliberately at the monopolization and exploitation of scarce societal resources (e.g., capital, skills, and labor power). These aims are attained through the creation of economic and political organizations (e.g., firms, crafts and professions, employer organizations and unions). Social stratification, in contrast, relates to the interests and aims of persons and families as individual actors. Only in particular instances and in a mediate way can their concern with access to societal positions and resources be furthered by the development of organizational means (e.g., schools, associations). In general, social strata are not able to develop anything comparable to "class capacities" (Wright 1978; see also Krauss 1967, p. 135).²

From a certain point of view it can be said that the relationship of individual persons and families to societal structures of inequality is of a reactive nature. Since the essential feature of social stratification is the limitation of close, personal interaction (and sympathetic feelings) to

² This thesis should be modified. The capacity of a deference stratum to distance itself from other strata increases the higher the relative position of the stratum. In fact, the highest social stratum probably can maintain its privileged position in the long run only to the degree that it is able to establish itself also as a "ruling class" (see Baltzell 1958, p. 34).

others located in similar positions and the restriction of interaction with "others" to role-specific utilitarian matters (such as that between professionals and clients), social strata cannot become more than loosely bound "communities" or "aggregates" (Martindale 1972, p. 272; Krauss 1976, p. 16).³ But even so, social stratification has to be considered an essential mechanism contributing to the long-term reproduction of social inequality when it penetrates into the most fundamental types of social relationships. It follows that the processes of class formation and stratification must constitute the starting point for the analysis of social inequality from a theoretical point of view. Existing structures of social inequality as distributive features—amount and distribution of wealth, skills, and knowledge and number and types of occupational opportunities, positions, and so forth—are doubtless of crucial importance in the relationship between class formation and social stratification. It seems misleading, however, to look for some set of "fundamental dimensions" of inequality (e.g., Lenski 1954; Runciman 1968) because these dimensions themselves are contingent on processes of class conflict and social stratification.⁴ This is evident in the case of class formation, which hinges on conflicts about the ways in which societal production and distribution are organized. But structures of social inequality are contingent on social stratification, too, even if in a more mediate way. Here, the uncoordinated pursuit of interests by millions of individuals and families constitutes a latent but nonetheless strong force maintaining old barriers and boundaries even when fundamental changes occur in the distributive aspects of social inequality (see Boudon [1974] and Bowles and Gintis [1976] for the effects of this process

³ This point should not induce wrong conclusions in regard to class formation. Social classes, too, cannot be considered fixed and "real" entities but only processes going on with varying intensity and outcomes in different times and places (see Jarvie 1972; Parkin 1974; Przeworski 1977).

⁴ How questionable such a strategy ultimately is becomes evident in theories which consider men and women as classes or strata in their own right (Lenski 1954; Collins 1971; Morgan 1975). The proponents of these theories are doubtless right in pointing to the elements of power, violence, and exploitation within intimate sexual and familial relationships. In my opinion, however, they overstate this view, ignoring the fact that the concepts of class and stratification lose their meaning if applied to the category of sex. F. Parkin expressed this point in the following way: "... if the wives and daughters of unskilled laborers have some things in common with the wives and daughters of wealthy landowners, there can be no doubt that the differences in their overall situation are far more striking and significant. . . . In modern society the 'vertical' placement of women in the class hierarchy . . . appears to be much more salient to female self-perception and identity than the status of womanhood per se" (Parkin 1971, p. 15). But this statement also can lead to false conclusions. Parkin is certainly right in indicating the fundamental importance of class and status inequalities for women's personal situation. But it is doubtful that these aspects can simply be set off against experiences in sexual life and motherhood. For reasons of space, it is not possible to discuss these problems extensively here: a systematic consideration of the theory and politics of the feminist movement would be required.

of "pluralistic adjustment" on the development of educational opportunities). It is justified, therefore, to claim that the descriptive analysis of dimensions of social inequality constitutes an important research domain (see Stone and Form 1953; Ellis 1958; Duncan 1968; Blau 1974; Pappi 1976). It is evident, however, that a theory explaining the emergence and maintenance of social inequality encompasses more than this descriptive level of analysis, which grasps only the outcome of class formation and social stratification at certain times.

Two final points should be made concerning the relationship between class formation and social stratification. First, I would like to stress once more the complex and mutual character of that relationship. At the level of economic and political transactions, class formation has to be considered the moving power for changes in the distributive features of social inequality and, subsequently, for stratification, too. Recognition of this fact helps to avoid interpreting social inequality as an immutable feature of human society⁵ or falling back on individualistic or functionalistic explanations of social inequality, as is done even by such authors as Schumpeter (1951, p. 150) or Weber (see Hechter 1976). It helps to understand why certain social goods become valued more than others and, consequently, a societal value hierarchy emerges, conferring "charisma" on incumbents of its center (Shils 1969; Eisenstadt 1970). The reason is that the creators and the holders of organizations and occupations in this center have monopolized important, powerful, and prestigious social activities and rewards. The development of such a value hierarchy and center is, therefore, mainly a consequence of the development of a corresponding power center (see also Tausky 1965). At another level, social stratification has significant effects on class formation. Through the process of strata-specific "pluralistic adjustment," aims and strategies of class organizations as well as the development of class consciousness may be thwarted but their accomplishment may also be facilitated. The first happens when social boundaries are created between people belonging to similar positions in the relations of production. The second takes place when activities and interests based on class criteria overlap social boundaries based on status-related criteria (e.g., ethnic membership; see Gedicks 1977). Thus, both class formation and social stratification must be conceptualized as concrete historical phenomena distinguishable by different contexts, actors, and temporal structures.⁶ Therefore, it can be said that

⁵ This tendency prevails among writers who emphasize the analogies between animal and human societies (e.g., Martindale 1972).

⁶ In this regard, I do not agree with Bendix (1974), who understands class and status-group formation as historically exclusive processes in the sense that when one of them prevails the other is not active. Class formation and conflict continue just as much in periods when economic change slows down, although they may be less visible

in a certain sense class-related mechanisms are those that generate social inequality, while status-group formation serves as a protective device against class-based inequalities. From this point of view, corporate status-group consciousness helps people in underprivileged positions to develop stable social identities. But this "reactive" character of social stratification for underprivileged social groups becomes an active strategy with members of middle and higher strata. Status-group formation as "social closure" here becomes a powerful force, creating and preserving social inequality in its own right (see also Parkin 1974). The persistent tendency of stratification mechanisms toward the formation of at least three distinct strata and, thus, some "middle layers" undermining the pure dichotomy of class society (Wallerstein 1975) confirms the complex intertwining of the two processes.

The second point concerns the role of conflict versus integration within the mechanisms of class formation and social stratification. Here I would like to stress that it is impossible simply to see class formation as a process of conflict and social stratification as one of integration.⁷ It is true that social stratification, through the generation of social distance, helps to conceal effectively in everyday life the objective amount of inequality and thereby functions as a primary integrating mechanism. But in Western "consociational democracies" many aspects of class relations, too, seem to have the function of obscuring divergent interests more than that of bringing them to the public consciousness. On the other hand, it would be misleading to overlook the fact that to a large degree social stratification, like class differences, involves aspects of conflict, violence, and exploitation. This fact has been elaborated convincingly by W. P. Archibald (1976). He argues that class, status, and power differences are threatening, and, therefore, encounters between members of different class and status groups tend (1) to be avoided as far as possible, (2) to take place on a narrow, role-specific rather than on a personal basis, (3) to be initiated and controlled by those with more power, and (4) to contain a high degree of latent hostility (see also Collins 1971). But even if these formal sim-

in such times (see also Hechter 1976). Likewise, there is no reason to assume that class structures are less stable than status structures, which are far from being "fixed entities" over long periods of time (see A. O. Haller 1970). Also, in the present approach it makes little sense to assume that "class situation" is predominant over "status situation" (Giddens 1973, p. 50), just as there is no reason to assume that "status politics" has surpassed class politics in advanced societies (Lipset 1964). I would argue, rather, that class relations continue to be predominant within the economic and political sphere of capitalist societies as do stratification mechanisms within the reproduction sphere.

⁷ I will show below some problems of a "pure" conflict theory of stratification. It might be noted here that similar difficulties arise for a pure conflict theory of class, which in the last instance must lose any concrete historical and political anchorage (see the critique of Dahrendorf's class theory by Weingart [1968]).

ilarities between class formation and social stratification are conceded, it should not be overlooked that power and status differences play quite different roles in them.⁸

Intergenerational Status Transmission and Assortative Mating— the Basic Stratification Mechanisms

It is one of the central arguments of this paper that a concept of social stratification based on interaction must pay explicit attention to the specific types of interaction under study. This point, which has rarely been recognized (e.g., Banton 1960; Ingham 1970), has important methodological implications. Each single case of social interaction is determined simultaneously by characteristics of the interacting units and by properties of the social system in which the interaction occurs (Scheiblechner 1971). In this critique, the position of a person in the structure of social inequality is a relevant trait of the interacting unit. The particular type of interaction involved, however, represents a system property which has to be taken into account from the theoretical viewpoint just as do the characteristics of the interacting unit.⁹ A related problem is that of the units in these interactions and relationships. In this section, I argue that (1) marriage has to be considered one of the two fundamental stratification relationships, (2) an accurate understanding of this relationship requires a focus on the acting units, and (3) these two arguments are consistent with the thesis that the family is the fundamental unit of social stratification. I am thus dealing with the third question to be answered by a theory of stratification.

O. D. Duncan (1968, pp. 683–85) considers assortative mating “a special instance of differential association” which should best be regarded

⁸ In fact, it may be possible to ground the theory of social inequality on a theory of social action in the following way. Starting from a distinction between the substantive and the relational dimensions of social action, one could see—following Habermas (1968) and Kreckel (1976)—work and interaction as the two essential substantive types of social action, and—following Weber (1968), Goldhamer and Shils (1969), and Kemper (1973)—power and status as the two relational types of action. The last two forms coordinate the actions of individuals: power enforces compliance through the orders of one actor to the other, status causes one actor to grant it voluntarily to the other. Work and interaction, power and status then can be thought of as forming a two-dimensional space with four types of social action, two of which are of particular importance in social reality. That is, power becomes the predominant form of inducing compliance within work settings and status within social interaction, thereby establishing the twin processes of class formation and social stratification. For reasons of space, it is not possible to elaborate this idea and its limitations here (see also Strasser 1976).

⁹ The importance of this point seems not to have been adequately recognized in studies which have analyzed particular types of interaction, such as friendship patterns, as “best” indicators of social distance between strata (Laumann 1966; Pappi 1973; Reuband 1974).

as a condition influencing the intergenerational transmission of status. Other sociologists have put more emphasis on the marital relationship as a central feature of the stratification process. Weber (1968), Schumpeter (1951), and Wrong (1972) regarded frequent intermarriage as the most important characteristic of typical class behavior. Barber (1957, p. 123) notes that "the social consequences of courtship and marriage are more far-reaching than those of the other forms of intimacy." In most of the social distance scales which aim at representing the distance between persons, the marital relationship stands for the most intimate relationship (Beshers 1963; Laumann 1973).

The assumption that the marital relationship looms large in the process of stratification has not been satisfactorily substantiated in these studies from a theoretical point of view. An implicit answer has been given within the Warner approach to stratification, which sees marital homogamy and intergenerational status transmission as the central features of a "class society" (Coleman and Neugarten 1971, p. 5). This approach maintains that the persistence of family status over several generations requires "an equivalence of place not only for the marrying pair of one generation but for many generations which succeed each other" (Warner and Lunt 1941, p. 93).

From such a point of view, assortative mating has to be considered the necessary complement to intergenerational status transmission. If the choice of marriage partners occurred largely at random, such a phenomenon as "family status" could hardly emerge. The absence of homogamy seems to have been one of the most important reasons why, for instance, African societies did not tend to develop relatively enduring, culturally distinct social strata. The explanation is that "given the practice of heterogamy (or open connubium), it is clearly difficult to maintain or institutionalize class differences, that is, internal differences of culture distinct from those based on expenditures alone" (Goody 1971, p. 590). Homogamy must therefore be considered as a necessary condition for the development of family life-styles varying from one stratum to another. Students of marriage have long been aware that marriage patterns have important implications for the persistence of social structure because they constitute a primary segregating as well as an interlinking mechanism (Slater 1963; Talmon 1964; Fallers 1966; Leach 1970; Hazelrigg and Lopreato 1972). Homogamy can therefore rightly be considered "a mechanism to maintain the status quo" (Eckland 1968, p. 78) and marrying out an important "equalizing mechanism" (Talmon 1964, p. 497).

But why should the process of mate selection tend toward a pattern of homogamy in modern industrialized societies, of which it is said that "the tendency to marry for love may tend to equalize the status attainments of females from the different social levels" (Glenn et al. 1974b, p.

685)? The most common explanation of the persistence of patterns of homogamy in such societies, which lack strong and open forms of institutional control over mate selection, is that parents and peer groups still exercise such control, though more informally (Barber 1957, p. 126; Goode 1970, p. 156). This explanation based on social control, although it no doubt contains some truth, is not wholly satisfying from the point of view proposed here.

A social relationship is understood here as a relationship involving each participant as a "personal entity," that is, as a distinct individual known to the reference person from prior interactions (McCall and Simmons 1966, p. 169). Like any other social relationship, the marital relationship has to be considered part of the process of identity formation, which is influenced fundamentally by the macrosocial properties of both partners (Berger and Kellner 1970; Lopata 1973). By and large, young people who have enjoyed similar childhood and youth experiences and developed similar identities and outlooks will find opportunities for "falling in love" with each other. By creating interaction boundaries, social stratification restricts the pool of eligibles to those with a similar position in the system of social inequality. According to this view, structural constraints and individual choice are not mutually exclusive but interlocking aspects of the mate-selection process.

Within such a framework, one controversial issue in stratification theory can be resolved more satisfactorily, that is, the nature of the unit in social stratification. Is the notion that individual persons are the units in the marital relationship consistent with the contention that the family is the unit of social stratification? From the viewpoint of symbolic interaction theory, social actors are considered the units in interactions and relationships. These actors, however, need not be individual persons; they can also be collectivities insofar as the members cooperate in solving common problems (Blumer 1962, p. 187; McCall and Simmons 1966, p. 175). In this sense, the relationship between a man and a woman can be considered a collective unit. The question is, then, In which cases and under what conditions can we expect a marital pair to represent such an acting unit?

As Simmel (1923, pp. 61-63) noted, two-person relationships in general hardly develop a sense of unity or objective reality. This is valid especially for love relationships, which are typically characterized by some feeling of "uniqueness." Two characteristics, however, tend to provide the marital relationship with a certain sense of objectivity (Simmel 1923, p. 61): its close and intimate character, and the existence of laws governing the forms of contraction and dissolution of marriage and the rights and obligations of the partners. A further element enters in when a couple is establishing a household. This element comprises access to common artifacts of living and the multiplicity of activities engaged in together

over long periods of time. Both of these operate to strengthen the sense of unity of the couple in a fundamental way. Sentiments and activities, laws and possessions together introduce an element of ascription into the relationship, thus contributing to its consolidation and objectification (McCall and Simmons 1966, p. 174). Only to the degree that these sets of forces are effective can we think of the marital pair as becoming a "social unit."¹⁰ On no account, therefore, are a husband and wife to be accorded automatically the same social status from their wedding day onward. Only to the degree that they act together on common tasks or stick together against outsiders "to maintain proper fronts" (McCall and Simmons 1966, p. 176) can they be regarded as a unit.¹¹

From this point of view, the model of the nuclear family and its place in the stratification system as developed by functionalist stratification theory (Parsons [1953] 1966, [1940] 1954a; Barber 1957) seems highly questionable. Three assumptions of this model are relevant here: (1) individuals derive their status from their families, (2) the family derives its status from the male head of the household, and (3) the status of women is derived from that of their husbands (see Watson and Barth 1964; Acker 1973). Recent critics of these assumptions have argued that they are no longer tenable because increasingly large numbers of married women are working and because in many of these marriages the educational and occupational level of the wives exceeds that of their husbands (Barth and Watson 1967; Haug 1973). Generalizing this critique, C. Safilios-Rothschild (1975) has hypothesized that not only can working wives "enjoy two status lines"—one achieved by their own occupation and one derived from their husband—but that this may be true for their husbands as well.

¹⁰ Anticipating an example from the discussion in the second part of this paper, it follows from these assumptions that the stability of the marital relationship must be highest when each kind of force operates strongly. This is true of one social group or class in advanced industrial societies, namely, the petty bourgeoisie (farmers, small shopkeepers, craftsmen, etc.). Research on divorce patterns by class and status shows that this category is in fact characterized by the lowest divorce rate (see Haller [1977] and other contributions to Chester [1977]).

¹¹ It is possible to ascertain the mechanisms leading toward status unity within the family without referring mainly to cultural and institutional support. According to Scott (1972, p. 586), the tendency toward status unity exists in order to motivate parents to take on the responsibilities of child care; parents, conversely, are motivated to take on these responsibilities because the family constitutes a status unit. This argument is not only circular, but also untenable from a sociological point of view. Doubtless there exist social norms reenforcing parental responsibilities, but these norms alone would hardly be sufficient to motivate parents to care for their children. Parenthood as a social relationship has to be explained in the same way as any other relationship, namely, by reference to the needs and expectations of its participants. From this point of view, it is clear that parenthood entails many burdens but also significant rewards and gratifications. Fletcher (1966, p. 149) is one of the few family sociologists who finds this trivial yet fundamental point worth mentioning.

The arguments of these critics are sound but they do not go far enough to be of theoretical significance. Indeed, the critics themselves have not abandoned one of the basic propositions of the functionalist approach, namely, that social status is gained only through a full-time occupational role. It is evident, however, that this assumption is incompatible with the theoretical point of view proposed here, which conceives of occupation as only one among a series of dimensions of social inequality. It is at odds, too, with empirical findings, which show that the status of the wife has an important impact on family life and on the future of children even if, for example, the wife has a lower educational or occupational position than her husband or does not work at all (see Warner and Lunt 1941, p. 90; Krauss 1967; Barth and Watson 1967; Kohn 1969; Coleman and Neugarten 1971, pp. 71-79, 247-48; Jennings and Niemi 1971; Szinovacz 1973; Ridge 1974). Neglect of this fact is related to one further questionable assumption within the functionalist approach. When functionalists speak of the family as the fundamental unit of stratification, they are referring only to the nuclear family. But if we are to understand the relations between family and stratification, we must consider not only one marrying pair and its children but a sequence of marital and familial units (Warner and Lunt 1941, p. 93; Schumpeter 1951, p. 177). The reason that the phenomenon of "social inheritance" encompasses more than two generations does not lie, however, in the peculiar character of marriage and family relationships. The reason is that families living in a world characterized by pervasive social inequalities develop strong interests in preserving a privileged position once they have acquired it.¹²

STRATIFICATION MECHANISMS WITHIN THE MARITAL RELATIONSHIP

As outlined above, the analysis of social stratification as an interaction process requires the simultaneous investigation of types of interaction and characteristics of the persons involved. In order to comprehend fully the role of the marital relationship in the process of social stratification, it would be necessary, therefore, to study its particular features and outline its specific meaning in contrast to all other forms of social interaction and relationship, such as those between parents and children, siblings, kin, friends, or neighbors (for the case of siblings see, e.g., Sweetser 1973,

¹² It is not surprising, therefore, that the three-generational phenomenon of "class regression" is weak compared with the strength of the status inheritance between parents and their direct offspring (Glass 1954, pp. 266-87; Allingham 1967; Ridge 1974). It is likewise plausible that acquired wealth and status can be preserved over many generations even among high-status families only if strict rules of inheritance are observed (see Lebergott 1976). That such rules often stand in contradiction to the personal feelings and happiness of individual family members is a recurrent theme of many writers (see, e.g., the famous novel *Die Buddenbrooks* by Thomas Mann).

Müller 1975). In doing so, one should record not only the presence or absence of a relationship but also its frequency, degree of regularity, intensity, content, and any other related and relevant characteristics (see McCall and Simmons 1966; Scheiblechner 1971). It is the main thesis of this paper that the unique features of the marital relationship confer on it a particular importance vis-à-vis most other types of social interaction and relationship. Here I will try only to delineate some specific features of the marital relationship insofar as they are relevant to the process of social stratification.

We may start with the premise that every relationship can be conceived of as having a career and in its course may become quite different from that initially intended (McCall and Simmons 1966, pp. 167-201). In general, every relationship contains a "strain toward totality" by including progressively larger segments of one's identity and greater amounts of one's time and resources. Thus, it tends to change from a personal attachment to a commitment and at times to an ascription. In some instances, the elements of ascription may eventually outweigh the initial attachment, and the relationship may lose part of its rewarding character for one or both of the parties involved.

In the following discussion, I will concentrate on three aspects of the development of the marital relationship which are of particular importance here: the decision to marry, patterns of marital homogamy, and consequences of marital heterogamy. I have selected these three themes because they have been either relatively neglected in related discussions or treated rather one-sidedly and therefore offer the possibility of demonstrating the explanatory power of the present approach in contrast to the dominant tradition.

The Decision to Marry

From the point of view proposed in the first section, it follows that the mate-selection process as a whole cannot be considered a unitary psychological act of choice but only a process of building a relationship over time (see Bolton 1961; Kerckhoff 1964; Broderick and Hicks 1970). The analysis of marriage as part of the process of social stratification cannot be limited, therefore, to the analysis of the patterns of homogamy, but must also be directed to the processes whereby such patterns come about. In fact, it can be assumed that the decision whether to marry or not is an important stage among these processes.

Three different elements can be distinguished here. The first concerns expectations about a love and marriage relationship. These are shaped fundamentally by the experiences of early and later childhood and adolescence. Two sets of influence may be distinguished here. The first con-

cerns the patterns of marital and familial life as they have been exhibited by the parents of both partners. Not only the form and quality of their conjugal life as such but also intra- and extrafamilial division of labor have an impact on the younger generation's images and expectations. And it is evident that many of the features of those styles of life—cooperation of the parents in a family enterprise, employment of the mother, experiences of separation or divorce, etc.—vary according to societal position. A second set of influences is related to educational and occupational experiences. Attending school through the period of adolescence not only imparts knowledge but also allows a kind of privileged, "prolonged socialization" (Rosenmayr, Kreutz, and Köckeis 1966). The same is true for the occupational career, which is all the more central and significant a part of life experience the more highly qualified a person is and the more steadily he advances in his career. Such aspects of work and career as the degree of work commitment (Haller and Rosenmayr 1971) or occupational self-direction (Kohn 1969) have a fundamental impact on personal identity formation. Both education and occupation, therefore, through their impact on identity formation, have significant effects on the meaning of the marital relationship for the persons involved (see also Lopata 1973).

A second, related, element in the decision whether to marry or not regards the resources of the persons involved. From this point of view, education and occupation constitute strategic assets providing the base for a stable income and livelihood as well as enabling the development of cultural interests and a distinctive "style of life" (Shils 1969, p. 309; Scanzoni 1970). The relevance of this element to the decision to marry can be demonstrated particularly in regard to property as a source of income. Analyses of characteristics of people relying on marriage advertisements (Mertesdorf 1967; Haller 1980) and of marital pairs adopting children (Donati and Nicoletti 1974) show clearly that marriage here is seen to a large degree as a "status symbol" (Goffman 1951) and/or a means of acquiring heirs and transmitting property.

A third element in the decision to marry is the mate-selection process itself and its interplay with other experiences and turning points throughout the cycle of life. From this point of view, education and occupation are important insofar as they structure one's life course, especially during youth and young adulthood—the period when mate selection takes place. One obvious aspect relevant here is the availability of a spouse with a similar educational and occupational level, that is, the immediate structural determinants of the patterns of homogamy. The hierarchically stratified systems of education as they exist in many advanced industrial societies sort young people into physically separate groupings just at the time when they are dating and marrying (Garrison, Anderson, and Reed

1968, p. 116). In view of the cultural and institutional constraints ascribing to the husband the "economic provider role" for the family (Parsons [1942] 1954*b*; Grønseth 1970), the availability of an "adequate" spouse doubtless plays different roles for men and for women. As long as the status conferred on the family by the public is more contingent on the husband's status (see Rossi et al. 1974), it is not really surprising that women are more prone than men to remain single when they cannot marry someone of at least equal standing (see Coleman and Neugarten 1971, p. 248).

Thus, a variety of forces related to the position of men and women in the macrosocial structures of social inequality affect the decision to marry. Aspirations and experiences, successes and failures within the educational and occupational career are particularly relevant to aspirations and expectations about the marital partnership. Empirical research has shown that early school leaving and poor occupational prospects often lead to an ill-considered marriage, sometimes also in connection with an unplanned pregnancy (Elder 1969; Haller 1973*b*). On the other hand, successful graduation from an educational institution as well as good occupational prospects will certainly reduce the need for relying on marriage as an evasion. All in all, it is not surprising that the net effect of educational and occupational experiences turns out to be that unmarried females are concentrated in the higher social strata, while the reverse is true for men (see Carter and Glick 1970, p. 685; Findl 1978). For better educated women, most of the processes mentioned here reduce the need for relying on marriage as an "institution of maintenance" and at the same time increase expectations about a marital partnership. Hence neither "spinsterhood as choice" nor "marital rejects" seems an appropriate label for the complex interplay between structural constraints and personal experiences and aspirations in a woman's decision not to marry (see also Havens 1973).

Patterns of Marital Homogamy

If marriage has to be understood as a relationship between two individuals involving and affecting their personal identities, it follows that the analysis of patterns of marital homogamy must concentrate on the macrosocial properties of the individual persons involved in that relationship. This simple but fundamental assumption, which has been neglected in many recent studies, will be discussed in the present section.

Three sets of assumptions and methodological procedures are of particular relevance here. The first concerns the problematic strategy of investigating marital homogamy by comparing the occupational status of husbands' and wives' fathers, respectively (Laumann 1966; Rubin 1968; Hazelrigg and Lopreato 1972). The reason for taking the occupational

status of the spouses' parents as indicators for patterns of marital homogamy was stated, for instance, by Rubin (1968, p. 752) as follows: "It seems clear that the jobs held by young women before marriage do not provide reliable indicators of their social status." This assumption seems to pervade much of the literature on this topic (see Laumann 1966, p. 76; Eckland 1968, p. 80; Parkin 1971, p. 15; Mayer 1977, p. 172; one rare exception is Oldman and Illsley [1966]). Upon closer examination, however, it does not stand firm. Glenn et al. (1974*a*, p. 684) have rightly criticized Rubin's study because it "relates the origins of women to the origins of their husbands and does not deal with the adult status of the husbands. To illustrate, by the Rubin procedure, an insurance salesman's daughter married to a physician whose father was a craftsman is classified as downwardly mobile." Doubtless there exist substantiated sociological arguments for regarding social origins of husband and wife as significant determinants of their marriage patterns. But considered from such a point of view, marital heterogamy is a multidimensional phenomenon in a three-fold way: (1) within the parental generation, (2) between husband and wife, and (3) between these two generations. It is evident that the consideration of only one of these units must confound these different effects. This point has been clearly recognized by J. M. Ridge (1974, p. 47), who notes that the adequacy of a view of the family as a social context composed of individual members can be established only by including the "emergent properties" of the family as a system together with the individual attributes of its members. It has to be noted in this regard, moreover, that the frequent assumption that women's own occupation is a poor indicator of their status does not correspond to reality. Most of the systematic comparisons to date between patterns of intergenerational status of men and women have concluded that they do not diverge substantially (see DeJong, Brawer, and Robin 1971; Felson and Knoke 1974; Treiman and Terrell 1975; Featherman and Hauser 1976; McClendon 1976). Even though this generalization has been attacked recently on plausible grounds (Sørensen 1979), it is a fact that male and female educational and occupational status distributions do not diverge substantially (even though they are concentrated in different occupations and industrial sectors). From this point of view, it is hardly an improvement to compare the occupational status of wife's father with that of her husband (see Glenn, Ross, and Tully 1974*b*; Mayer 1977).

A somewhat different justification for not considering the marital relationship one of the two fundamental "stratification relationships" has been presented by E. O. Laumann. He argues that friendship patterns may be more important for maintaining a stratification system because they "serve to socialize persons into the appropriate attitudes of a given level in the stratification system" (Laumann 1966, p. 84). It is questionable, however,

whether friendship relations are in fact characterized to a higher degree than marital dyads by status congruity of the partners,¹³ and it is, moreover, unlikely that friendship relations have as long-lasting consequences as a marital relationship, which involves much more than occasional voluntary encounters and activities.

A third line of research does not aim to analyze marital patterns *per se*, but asks whether women's intergenerational mobility through marriage is similar in extent to men's (Tyree and Treas 1974; Glenn et al. 1974*b*; Chase 1975). The theoretical assumption behind this question seems to be the same as that already criticized above, namely, that the main determinant for the status of a woman is the status of her husband (see Chase 1975, p. 484). From this point of view, it is logical to ask, not what determines the status of women themselves, but "what determines their access to men of different social status" (Pavalko and Nager 1968, p. 524). But in view of the just mentioned fact that the intergenerational occupational status attainment processes of men and women do not diverge very strongly, it is hardly tenable that "most female mobility has been (and perhaps is) through marriage" (Glenn et al. 1974*b*, p. 684; one exception to this assumption is Tyree and Treas [1974]). It is also misleading to draw conclusions about the general pattern of female mobility by investigating only marital mobility of women (e.g., Chase 1975).

Thus a good deal of existing research on patterns of marital homogamy is at odds not only with the theory of social stratification advanced here but also with a social theory according women, be they employed outside their household or not, a status of their own.

Consequences of Marital Heterogamy

In spite of the fact that the mate-selection process operates as an assortative mechanism, a considerable number of marriages turn out to be heterogamous in some of the macrosocial properties of the partners (see Barth and Watson 1967; Haug 1973). A theory of stratification which focuses on marriage patterns should obviously allow us to deduce hypotheses about consequences of marital heterogamy, too.

It is not necessary to stress again that only the macrosocial characteristics of the individual partners involved can be the starting point for such an analysis. This point has been acknowledged by Pearlin (1975, p.

¹³ I do not know a study answering exactly this question. Existing evidence seems to confirm the hypothesis that the marital relationship is characterized by status congruity to a higher degree than is any other type of relationship. So, for instance, a series of studies have reported that homogamy, especially in relation to education, is much greater than similarity in status between parents and children (see J. Berent in Glass 1954; Blau and Duncan 1967, pp. 169, 354; Garrison et al. 1968; Haller 1973*a*, p. 548).

346), for instance. In his study of status inconsistency and stress in marriage, he states that "the status a wife initially brings to marriage is likely to be as important to her husband as his is to her." But this statement seems to be only lip service: the author goes on to say that an interest "in the effects of hypergamy and hypogamy on men no less than on women [is] best served by comparing spouses' backgrounds." For this neglect of women's own status attributes, Pearlin has been criticized by Weiser and Magnani (1976). In their own study, however, these authors seem to accord a status of their own only to those women who are active members of the labor force.

Apart from this problem, already dealt with in the preceding section, the question remains what hypotheses about consequences of marital heterogamy can be deduced from the present approach to stratification. At first sight, the model of status inconsistency seems to be suitable for the analysis of marriages in which husbands and wives differ in their respective social characteristics. Watson and Barth (1964, p. 16) have therefore proposed this as a "logical extension" of inconsistency research (see also Barth and Watson 1967, p. 400). Superficially considered, there is already a difference between inconsistency at the individual and inconsistency at the familial level. At the individual level, it is necessary to postulate that (1) a number of parallel vertical hierarchies exist, and (2) the consistency of various combinations of status attributes is defined by socially established sets of rules and norms (Lenski 1954, p. 405; Meyer and Hammond 1971, p. 92; Randall and Strasser 1976). In the case of homogamy, at least the first of these postulates seems unnecessary since husband and wife may be compared on one and the same status dimension. But since such a comparison will scarcely make sense unless the second assumption is valid, too, the difficulties of inconsistency analysis are hardly overcome.

The present approach, however, diverges in a fundamental way from the inconsistency approach. Macrosocial properties like education and occupation are not considered here as separate independent dimensions of something like a "stratification space." Instead, they are considered important only to the degree that they are related to specific features of the process of individual and collective identity formation. In this perspective, heterogamy is expected to have effects on the marital relationship particularly if it involves differences in social experiences, attitudes, and behavior. Barth and Watson (1967, p. 400) have expressed this point of view well: "Class-linked differences in life-style involve different attitudes, beliefs, values, habits, interests, activities and possessions. Therefore, it seems likely that the presence in the same nuclear family of parents from different occupational worlds, and/or with other life-style related differences, and/or from families of orientation of different life-styles, would generate various dilemmas for all family members. Pressures to participate

in different types of literature, to rear different types of children using different types of social controls, would complicate the organization of family activities." I have quoted this passage because it makes plausible why heterogamy may have significant consequences but at the same time conceives them in the traditional manner. In fact, like studies of inconsistency (see Randall and Strasser 1976, p. 2), discussions of the consequences of heterogamy have been rather one-sided insofar as they focus almost exclusively on negative consequences for the individual partners.

It seems untenable, however, to assume that heterogamy can lead only to negative consequences for the marital relationship. Glenn et al. (1974a, p. 543) have argued that "there are theoretical reasons to expect a dissimilarity in class backgrounds to exert an integrative influence on certain marriages under certain conditions." In fact, one generalization found frequently in textbooks on marriage and family—namely, that dissimilarity of childhood class backgrounds of spouses contributes to marital maladjustment and divorce—receives almost no empirical support (Glenn et al. 1974b). Similarly, studies comparing the extent of heterogamy among divorced and nondivorced couples could find only weak evidence for a higher incidence of heterogamy among the divorced. In fact, this hypothesis appears to apply only among couples with extreme differences in their respective status attributes (Bumpass and Sweet 1972; Boigeol and Commaille 1974).

The one-sidedness and failure of many hypotheses about the consequences of marital heterogamy are based on a theoretical bias inherent in the inconsistency approach to stratification. If the geometrical analogy is taken seriously, it is obvious to argue that inconsistency and heterogamy as such will have definite consequences (see Hope 1972). This assumption, however, is questionable in view of the fact that the consequences of heterogamy are quite different according to its origin and direction. According to Pearlin (1975, p. 356), this fact disproves a theory of stratification based on cultural differences since it seems "not consistent with the fact that those marrying up are much less likely to have marital disruptions than those marrying down although cultural differences are equally present in each group." The latter fact, however, does not prove the irrelevance of culture- or subculture-based explanations of consequences of heterogamy; it shows only that such differences have an opposite meaning in up- and downward movements within the status structure. The reason is obvious. Whereas in most instances social ascent means an improvement of one's overall life situation, social descent may involve considerable deprivation. Whereas in most cases social ascent may have been actively striven for, partly through "anticipatory socialization," quite different kinds of aspirations and expectations may have preceded and accompanied experiences of social descent. But here again it would be

erroneous simply to conclude that the person downwardly mobile through marriage goes through only negative experiences (just as it would be erroneous to see only the positive aspects of marrying up).¹⁴ The broadening of one's mind as well as effects on intrafamilial influence and power (Szinovacz 1973, pp. 213–20) would have to be weighed against the feared or real loss in status (see also Haller 1974).

An explanation of consequences of marital heterogamy (and status inconsistency in general) based on cultural differences between strata has to be supplemented, therefore, by a "theory of social position" (see Boudon 1974, p. 29; Cherkaoui and Lindsey 1977). Such a theory must specify the fundamental differences in social situation, experiences, and ambitions of people located at different points in the social hierarchy or having experienced different forms of social mobility. From this point of view, it is evident that much of the social and cultural identity of persons and groups develops out of their particular position, deprived or privileged, low or high, in the class and status structure. Since upward and downward marital mobility have fundamentally different meanings, it is evident by now that it hardly makes sense to hypothesize that marital heterogamy can have only negative effects.

It is only such an extended perspective that enables us to see and evaluate the macrosocial potential of marriage patterns and marriage policy as strategies to reduce social distance between strata and to further social equality and justice. While marriage doubtless offers possibilities in this regard, they are clearly limited because the task of bridging social and cultural barriers through marriage is imposed exclusively on individual persons and families. From this point of view, it is evident that there exists a dilemma in modern society over the "incompatibility between family stability and equalitarianism as values," deriving from the fact "that the kinds of kinship paradigm which seem to promote family stability are those that apparently function to perpetuate class stratification in society" (Farber 1970, p. 93; see also Curtis 1964).

This dilemma, however, is not necessarily inherent in the relationship between marriage and social differentiation. It can become attenuated to the degree that two forms of societal development can be implemented: first, new guiding images and styles of life related to marriage (e.g., a greater involvement of husbands) and more adequate forms of public support for families and children with particular problems (e.g., single parents) and, second, a reduction of social inequality in those spheres where

¹⁴ Negative consequences of upward marital mobility become manifest particularly in the case of divorce when societal institutions seem unwilling to support the weaker party. In a study of divorce decisions in Viennese courts in the early 1970s, I found (Haller 1976) that the judges accorded to the divorced women alimonies corresponding only to their life-style before marriage if they had married up.

it is created and maintained by interests and strategies of collective groupings at the level of class relations. But unless these changes occur, the consequences of failed attempts to overcome cultural and social barriers will be much heavier for the partner belonging to the lower stratum. The consequences will be particularly grave if this partner is the woman because the burdens of bringing up the children continue to rest overwhelmingly on her (see Brandwein, Brown, and Fox 1974). In view of this fact it may look surprising that in most advanced industrial societies wives are taking the initiative to end unsatisfying marital partnerships by divorce in spite of the fact that husbands are more often the guilty party (at least according to the judicial attribution of guilt; see Haller 1977; Chester 1977). But this fact again quite clearly shows the active and decisive role which women play in regard to marriage and in the reproduction and transformation of social inequality, too. The fact that divorce is more frequent but remarriage less so among better educated women (Roussel et al. 1975) confirms again that decisions about marriage and divorce cannot be explained without taking into account the complex interplay of individual aspirations and decisions with structural resources and constraints.

CONCLUSION

According to M. G. Wenger (1977), a new "nonegalitarian" but "classless" paradigm for the study of social inequality has emerged in postwar Western sociology. Central to this development is the putative Weberian concept of "status groups" as opposed to that of "social classes." Its essential feature is a view of advanced societies as characterized by high degrees of social mobility, by a growing "middle mass" and an embourgeoisement of the working class. Wenger shows, however, that such a conception in no way does justice to Weber's concept of "Stände," which denotes almost the opposite of "status groups" in the aforementioned sense; as such it refers to a process of social closure initiated by elites in order to preserve their privileges and thus fosters a retrograde, not a progressive, social outcome (see also Mayer 1977, pp. 159-62).

Recently the dominant research tradition in the area of stratification and mobility has been criticized also from a Marxist point of view (Poulantzas 1975; Kirchberger 1975). In this perspective, the idea of a societal hierarchy is nothing more than an ideology serving the interests of the ruling class. In a postscript to his impressive essay "Two and a Half Models of Social Structure," Bertaux (1973, p. 150) has argued that even in the study of the reproductive aspects of social life the class structure approach "proves far more efficient . . . than the far more refined and 'consistent' stratificationist approach." Indeed, several sociologists

have already taken some steps in this direction by developing "conflict theories" of social stratification (e.g., Lenski 1954; Collins 1971; Morgan 1975; Archibald 1976).

In this paper, I have proposed an alternative strategy. I have argued that the long-term reproduction of social inequality should be explained by distinguishing between two different kinds of mechanisms: social stratification and class formation. The essence of social stratification concerns the inter- and intragenerational reproduction of social inequality through homogamous marriage patterns and status inheritance over families and generations. In this approach, individual actors and their social relations, not social norms and roles, constitute the basic units of analysis. It is possible, therefore, to avoid many of the ideological implications of the traditional approach to stratification as far as structure and functions of marriage and family are concerned but at the same time to allow for the fact that conflict, deprivation, and even exploitation are present within types of intimate social relations, too. From this point of view, marriage and family have to be considered concrete, historically rooted institutions. As such, they are not only dependent on the outcome of class formation and class conflict; they themselves contribute to the maintenance and reproduction of social inequality. From this point of view, the deduction drawn by J. Kellerhals from his analysis of social inequality in family life is wholly justified: "Terminons en disant que, dans ce qui précède, la famille a toujours été envisagée comme variable dépendante du système de stratification. Une analyse plus complète des rapports entre ces deux sous-systèmes demanderait que l'on s'interroge sur la question de savoir dans quelle mesure la famille est elle-même un agent de reproduction sociale" (Kellerhals 1974, p. 484).¹⁵

Marriages and families doubtless are dependent on material and institutional support by society at large, and they are also deprived or privileged according to the position of their members in the macrosocial structures of inequality. But as systems of social relationships consisting of responsible individual actors they do not react mechanically to these and other macrosocial pressures and demands.

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¹⁵ In a similar vein, P. Donati (1978, pp. 27-34) has noted that functionalist theory and orthodox Marxism (as proposed by some scientists and policymakers in socialist countries) show a "strange convergence" insofar as they agree in their judgment of the modern family as a private, society-dependent, nuclear group whose functions are progressively declining.

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Kinship, Marriage, and the Family: An Operational Approach¹

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The concepts used in the study of social organization (such as kinship, marriage, and the family) are "secondary" concepts insofar as they are derived from a more implicit, more fundamental notion of group. The notions of group which underpin current models of kinship, marriage, and the family render groups "ontologically variable" because they are ultimately reducible to considerations of interpersonal behavior and normative representations. This, I contend, makes them unfit for rigorous comparative study of social organization (the very project of social anthropology). To achieve greater rigor, I suggest a new, "operational" notion of group disentangled from considerations of interpersonal behavior and normative representations, and I consequently derive new and, I hope, more "universal" definitions of kinship, marriage, and the family on the basis of this new notion of group.

The literature of the past two or three decades in social and cultural anthropology testifies to a growing conceptual malaise; such concepts as descent, kinship, and residence, to name but a few, are subject to regular reassessment, but to little avail, it seems, since these periodic introspections have failed to yield any consensus about our basic definitions. The "universal" concepts, without which our comparative project remains futile, are still wanting. In this paper, I wish to investigate the reasons for this failure and to propose a solution. The solution, which hinges on a new, "operational" notion of group, could in fact be applied to all the concepts used in the analysis of social organization. Elsewhere, I have applied it to the notions of residence and descent (Verdon 1980*a*, 1980*b*), and in this essay I seek to extend it to the ideas of kinship, marriage, and the family, which anthropologists have treated as an inseparable trinity.

I regard these "anthropological" concepts (residence, descent, kinship, and so on) as "secondary" or "derivative" concepts, for the simple reason that they are derived from a more implicit, more fundamental notion of group. This is not to deny that, prior to their utilization for anthropological analysis, these notions were rooted in empirical and cultural experience.

¹ I wish to thank Dr. Mihaly Sarkany, of the Hungarian Academy of Science, for comments on an earlier draft of this paper.

Certainly, "kinship" and "marriage" refer to something "out there" which has received diverse interpretations in different cultural surroundings. But the manner in which these commonsense notions, borrowed from the everyday language, have been redefined as "scientific concepts" for the comparative analysis of sociocultural systems has been, I believe, directly determined by the analysts' representation of what groups are.

I argue that the few notions of group which currently dominate social and cultural anthropology render groups "ontologically variable" and therefore unfit for rigorous comparative analysis, since they thwart the emergence of universal concepts necessary for our comparative discourse. This stultifying action, I believe, stems from the fact that anthropologists have implicitly or explicitly defined groups by mingling three separate layers of reality: the behavioral, the normative, and the group levels. In this confusion, social and cultural anthropology (the "science of man") resemble Aristotelian physics (the "science of change"), which conflated phenomena of kinematics, biology, and chemistry as various instances of "change." The scientific revolution in the 17th century could be achieved only through the initial separation of these separate levels of reality; similarly, I believe a more rigorous comparative study of society to be possible only if and when the level of "group" is divorced from the realms of interpersonal behavior and normative phenomena.² I consequently see the main task of a conceptual revision as beginning with a new definition of group, disentangled from considerations of interpersonal behavior and normative representations. This I endeavor to achieve by suggesting an operational notion of group, from which it is then possible to derive new and, I hope, more universal definitions of our main concepts, namely, descent, residence, kinship, and so on.

In this paper, I will limit myself to a redefinition of the concepts of kinship, marriage, and the family and open the investigation with a brief review of what I regard as the two dominant conceptual paradigms in the social anthropological study of these concepts—namely, the structural-functionalist and the structuralist.

THE DOMINANT CONTEMPORARY PARADIGMS

The structural-functionalist approach in anthropology is associated with the name of Radcliffe-Brown, whose notion of social structure underlies much of present-day theorizing. Radcliffe-Brown assumed that, by definition, individuals are unique, idiosyncratic, and autonomous, and that their relations, or the very existence of sociability, was problematic. For a group or a society to exist, these unique and independent individuals

² It has been the aim of most "positivist" social theorists to achieve a divorce of the social from the psychological, but this essay rests on the premise that they have failed.

must interact, and their interaction presupposes some standardization or patterning of behavior (Radcliffe-Brown 1957). Like most functionalists, Radcliffe-Brown postulated that individual behavior must be somewhat constrained or regulated (i.e., share a common element by reducing its idiosyncratic nature) for interpersonal relationships to take place and that this regulation is achieved through the action of "binding" or coercive mental representations—namely, norms, values, rights, duties, beliefs, and so on.

Against this undifferentiated background of interpersonal ties, Radcliffe-Brown also sought a formula to conceptualize the discreteness of the corporate groupings which constitute society's internal subdivisions. Individuals within a society interact, but they also form groups with diverging loyalties which unite subsets of individuals but divide them from other subsets. In order to reconcile these two levels of interpersonal and group ties, Radcliffe-Brown resorted to the Durkheimian distinction between solidarity and its jural expression. The standardization of behavior found in interpersonal relationships is concretely expressed in specific rights and duties (Radcliffe-Brown [1935] 1952), and the rights and duties pertaining to the reciprocal positions in a given relationship form identifiable "roles." A group may therefore be represented as a "bounded" and interconnected set of social relations, which are crystallized in terms of roles. It is further possible to represent the set of roles within a group as something which belongs to the group, as the group's "estate," and to picture groups accordingly as "corporations" owning estates comprised of both a territory and a set of roles. To perpetuate themselves groups must avoid disputes over membership and must therefore own their roles exclusively (and the members who occupy them), thereby delineating the boundaries which distinguish them from other groups.

Against this general notion of group, kinship is presented as a fact of nature, a physiological fact (the fact of consanguinity) selectively emphasized by society and endowed by the latter with a set of norms or jural sanctions which make it binding on individuals. Once made binding or normative through "social recognition" or "institutional arrangement," kinship then operates to constrain and regulate behavior in interpersonal relationships. By the same token, it also serves to define statuses, thereby functioning to give "kinship status"—bearers membership in the group. Because Radcliffe-Brown conceptualized groups in terms of corporations which are differentiated from others (are bounded and therefore only allow people in under certain conditions) and also internally "organized" insofar as the interaction of members is regulated, kinship came to be represented both (*a*) as a rule of group membership (often known as "descent" in this instance), and (*b*) as a regulator of behavior. And

because anthropologists believed groups in simpler societies to be based mostly on kinship, the family came to take a dominant position in structural-functional thinking as the very workshop where the raw, natural datum of consanguinity is processed to produce the social ties of kinship. The family operated to generate kinship, to manufacture the glue which organizes these simple societies; as a result, marriage was given a derivative importance, being completely subsumed under the family. For the structural-functionalists, marriage derives its significance from the unique position of the family, and it is defined in a teleological manner, with the family as its purpose. Since the latter institution serves for social placement, marriage appears as a means of legitimizing children.³

The main challenge to the structural-functionalist model came from Lévi-Strauss (1945, 1949). Lévi-Strauss elaborated his model from the initial intuition that kinship statuses ought to be treated like symbols, not to be defined "substantively" with reference to themselves, but only through their relation to other statuses (Lévi-Strauss 1945). A man, for instance, is not a "father" when studied with reference to himself, but only in relation to his child(ren). This notion was later extended to groups, and Lévi-Strauss postulated that groups gain their identity and reality only in relation to other groups, through exchange. Such an idea ran counter to all the established dogmas, if only because it appeared to do away with the elements of behavior and their jural expression so crucial to the structural-functionalist credo. It provided a new concept of group which strove to be operational, in that a knowledge of the rules specifying the relation between groups should allow the analyst to infer the type of social structure. This innovation reverberated throughout his conceptualization of kinship, marriage, and the family.

Marriage, to Lévi-Strauss, came to represent the main expression of the principle of exchange upon which social life rests because of the alliance that it creates between exchanging units, an alliance which actually defines the very identity of these units or groups. From this equation, Lévi-Strauss

³ Despite numerous debates on the definitions of kinship, marriage, and the family (esp. Gellner 1957, 1960, 1963; Needham 1960; Barnes 1961, 1964; and Beattie 1964 on kinship; Fischer 1952, 1956; Peter, Prince of Greece and Denmark 1956; Leach [1956] 1961; Gough [1959] 1968; Fortes 1962; Bohannan 1963; Marshall 1968; Winch 1968; Harris 1969; Goodenough 1970; Rivière 1971; and Krige 1974 on marriage; and Murdoch 1949; Goode 1959; Levy and Fallers 1959; Reiss 1965; Bender 1967; Smith 1968; Winch 1968; Harris 1969; Barnes 1974; Adams [1953] 1968; Christensen 1964; Buchler and Selby 1968; Mead 1931; Richards 1950; and Hsu 1965 on the family), the basic concepts have not been significantly altered, since the underlying notion of group has never been questioned. Whatever definition of these concepts one may find in the sociological and social anthropological literature, it always makes reference to social relationships, their behavioral content, and the regulatory power of norms, expressed in terms of rights and duties. This is the invariant substratum in all these discussions.

inverted the common assumption that the purpose of marriage is to found a family through the procreation of children, contending instead that the creation of families (or the production of children) is only accomplished for the purpose of marriage! If groups derive their reality and identity through exchange, they must keep the exchange going, and the only solution is to procreate. Consequently, groups reproduce themselves physically only with a mind to break even in the eternal game of exchange, which requires a bilateral flow of gifts. Kinship is therefore a simple by-product of alliance and is geared at the outset to the maintenance and furtherance of alliances. As a result, the structural-functionalist "elementary family" lost its unique status of irreducible kinship group, to be replaced by the structuralist "atom of kinship," which included the mother's brother as a necessary representation of the principle of exchange.⁴

Despite its brevity, this sketch illustrates how the concepts under investigation, insofar as they have been endowed with a specialized content to serve as anthropological or sociological tools, are derived from a more fundamental notion of group. In the following part, I wish to assess the usefulness of these notions of group in the light of social anthropology's avowed purpose, comparative analysis.

THE COMPARATIVE USEFULNESS OF CONTEMPORARY PARADIGMS

1. The structural-functionalist notion of group is predicated on the idea of "regulated behavior." Behavior can only be controlled through the mind (if one excludes brute physical coercion), and only mental representations which have acquired a "binding power" from a source external to the individual—such as "social recognition," "institutional arrangements," the "politico-jural domain," or simply "custom," according to the various authors—can achieve this control. If some mental representations become binding because of their recognition by "society at large" (i.e., because of their "institutionalization"), this can only be because there already exists a structured and organized society which recognizes that certain behaviors are appropriate in certain interpersonal relations, while others are not. This only begs the question of the source of this external society's own organi-

⁴ Since Lévi-Strauss, Schneider seems to be the only theorist who has suggested a new perspective on the problem of kinship, by arguing that it is an essentially cultural category and does not exist as a separate sociological category; hence its sociological study cannot be a legitimate endeavor (Schneider 1967, 1968, 1972). Furthermore, from what can be culled from the sociological literature on the family, most if not all the conceptual frameworks can be construed as variants of a functional approach. All of them postulate the relevance of interpersonal relationships, of interaction and behavior, whether in an ego- or a group-oriented perspective, whether arguing in favor of integration or demonstrating dysfunction (Hill and Hansen 1960; Christensen 1964; Mogey 1969; Klein et al. 1969; Burgess, Locke, and Thomas 1971; Nye and Berardo 1973; Morgan 1975; Hays 1977).

zation. How are the people forming this external, ordered, all-encompassing group themselves adjusted behaviorally before being empowered to recognize any norms as binding? In other words, that which we are trying to explain is itself used in the explanation, and the formulation is circular.

A more important shortcoming of the structural-functionalist model, however, is the ontological variability of statuses and groups which is attendant upon its application. Indeed, the first question which springs to mind in the jural definition of status is the number of rights required to define a status. If statuses are clusters of rights, it is legitimate to infer that all British citizens who are not eligible to the Crown suffer some politico-jural disability and are "less citizens" than the royal family. Convicts debarred from public office will also partake of a lesser degree of citizenship. Indeed, a definition of statuses in terms of rights and duties can yield only different degrees of the same status since rights and duties, like their cognate norms, are only binding mental representations, or regulators of behavior (Fortes 1949, p. 346).⁵ It is commonly acknowledged that normative mental representations are not equally binding on every individual and at different times in the life of the same individual. If there are varying degrees of conformity to normative mental representations, the same statuses (those of "agnate" or "citizen," for instance) will vary both in the number of their rights and duties and in conformity to the latter. This is the direct cause of the statuses' ontological variability (the fact that they appear in degrees). What applies to statuses also extends to groups. If the rights that a corporation wields over its members vary both in their number and in their degree of possessiveness (Schneider 1965), so do the groups they define.⁶ The jural model of group, like that of statuses, can generate only "degrees of groupness" (like the "strong" and "weak" descent groups, or the "more or less" patrilocal groups of anthropological literature).

In short, the jural notion of group is ontologically variable because, in this framework, groups are deprived of an independent ontological status. Groups, jurally defined, do not exist as a separate level of reality, because

⁵ The best illustration of this assertion will be found in the debate over the definition of marriage, where Leach opposed any attempt at devising a universal definition of marriage on the basis that the latter institution was a "bundle of rights" which varied from culture to culture (Leach [1956] 1961). This jural equation, he concluded rightly, implies an intrinsic variability of the statuses of "spouse" across and within cultures. From this correct assessment, it is only unfortunate that Leach failed to draw a more important inference, namely, that the jural definition ought to be abandoned!

⁶ This particular problem came very strongly into relief in the debate on the relative "strength" of patrilinearity and the acquisition of rights over the bride through bride-wealth (Gluckman 1950; Fallers 1957; Leach [1956] 1961; Lewis 1965), in which "strong agnatic descent" was read in opposite situations, namely, where women are "absorbed" into their father's group, or into their husband's. Barnes (1967) has already drawn the right conclusion: if radically opposite interpretations are derived from the same fact, is it not time that this type of formulation of the problem be altogether abandoned?

they are always reducible to the notion of "regulated interpersonal behavior." Murphy has best demonstrated this contention by showing how the "social" dimension is never extricated from the cultural in anthropological literature (1971). Despite more than a century of constant efforts, we have systematically failed to devise a notion of group which is not immediately translated in terms of psychology (interpersonal behavior) and culture (normative mental representations). The social level has never managed to acquire an independent ontological status, and this, in my opinion, is the ultimate cause of our conceptual malaise. There is only one alternative: either it is impossible to define groups as a distinct and autonomous level of reality, and our concepts will retain their intrinsic ontological variability, or it is possible, and we will be able to formulate the universal concepts so urgently needed.

2. In my view, Lévi-Strauss's greatest achievement was to catch a glimpse of what an operational definition of group (i.e., one in which the social is disentangled from the psychological and cultural) could be. His own notion came very close to being operational, because it almost obviated the two most dangerous pitfalls of structural-functionalism—namely, the behavioral and normative referents in the notion of group. By positing the primacy of the relation over the terms it relates, Lévi-Strauss attempted to give the concept of "relation" a purely logical or mathematical meaning, seemingly extracting groups from purely behavioral social relationships. His paradigm, however, did not completely elude the jural model's predicament. In the first instance, the logical relation by which groups are given their identity cannot be one of degrees; it has to be specified by a rule, since only a rule can define a logical or mathematical class.

The key relation between social groups in simple societies being one of marriage, it follows logically that marriage rules alone can give rise to groups. A class or group which acquires its very identity through a logical relation or axiom cannot tolerate mere preferences.⁷ Consequently, Lévi-Strauss's framework applies only to societies with marriage prescriptions. On this point I believe Needham (1971) to be right. Nothing short of a

⁷ The existence of "fuzzy sets" in geometry (or "fuzzy geometry") does not contradict this assertion in any way. Neither fuzzy geometry, nor probability theory, nor indeed any branch of mathematics has ever defined a subset in terms of "probable axioms." Subsets, of both definite and probable events, are always defined in terms of axioms which do not tolerate one single exception. There are nowhere in mathematics probable axioms (like our marriage "preferences") defining probable subsets; there are only axioms which define subsets of probable events. There is consequently a fundamental difference between a law of probability and a "probable law." What the structuralists and structural-functionalists alike are proposing is a rule (i.e., a "law," or an axiom) which would sometimes operate, but at other times would not. Such a rule would only operate with probability. A probability law, on the contrary, predicts with absolute certainty the probability of an event taking place. The law itself is not probable, only the events that it predicts.

prescription can define a group in terms of a relation. Degrees of "normativeness" in the rules (i.e., simple preferences) can only engender degrees of precision and identity of the group. The groups would therefore vary (as groups) with the changing bindingness of the relation. There is thus only one alternative: either the model yields the same ontological variability of groups, if one is to believe Lévi-Strauss's later statement (1965), or its use is limited to the study of a tiny fraction of the societies investigated by social anthropologists (if one is to believe Lévi-Strauss's original statement [1949]).

Moreover, it is logically unwarranted to postulate the anteriority of the relation over the terms it relates (the anteriority of the principle of exchange over the exchanging units). Groups cannot emerge *ex nihilo*, spontaneously and reciprocally defined through their relation. Groups must pre-exist the exchange, a fact which is implicitly acknowledged by Lévi-Strauss when he introduces "rules of descent and residence" before being able to deal with marriage prescriptions. If groups precede their relation and are above all defined by rules of descent and residence, Lévi-Strauss has not contributed anything new to the notion of group; if he claims that groups are completely defined through their relation, he is indeed saying something radically new, but unfortunately untenable.

But, above all, Lévi-Strauss's model has never completely severed its links with the behavioral and normative dimensions. His central notion, that of exchange, is defined in behavioral and normative terms from purely interpretative considerations about the behavior of individuals engaged in interpersonal relationships involving exchange (1949). His notion of relation also ambiguously straddles the logical and behavioral domains, here a logical operation and there a behavioral relationship. It could hardly have been otherwise, since Lévi-Strauss shares the structural-functional assumption about the problematic nature of solidarity. Positing that one (emotional) part of the individual is abhorrent to social life, Lévi-Strauss had to find a mechanism to generate the solidarity which makes society possible. Whereas Radcliffe-Brown located this mechanism in society at large, Lévi-Strauss placed it in man's mind. Not completely so, however, since the mind can only be operative, in the Lévi-Straussian model, if it generates a number of representations which are normative (the rule). Like Radcliffe-Brown, Lévi-Strauss needs the mind and rules shared by the group because he is addressing himself to the same fundamental problem, namely, the regulation of interpersonal behavior. Any theory which postulates that solidarity is a phenomenon to be explained is logically inseparable from a behavioral notion of group, since solidarity is essentially a psychological phenomenon.

In other words, all theories which posit the problematic nature of solidarity as a foundation for conceptualizing the social can breed only onto-

logically variable groups, because they unwittingly deprive the notion of group (or the social) of its specificity, its ontological autonomy. Conversely, any attempt to endow the social dimension with a separate reality will have to set aside the representation of groups in terms of social relationships. Indeed, if social is equated with social (meaning "interpersonal") relationships, I would also conclude the impossibility of divorcing the social from the cultural. What is needed, in fact, is a break with tradition and common wisdom, and if this were achieved by suggesting a new notion of group which were not ultimately reducible to the regulation of interpersonal behavior, it might then be possible analytically to isolate the social. Before putting forth such a notion, however, I will first identify a further source of ontological variability in the current notions of group.

The contemporary models also posit groups as being involved in many different activities (i.e., groups are said to have many functions—ritual, economic, judicial, and so on; they are defined as "multifunctional"). This only raises further problems, insofar as groups labeled in the same way (such as "lineages," let us say) may have radically different functions in various societies. If the group's solidarity is itself linked to the importance of these functions, and it is in the jural model, the groups will also vary in their "degree of groupness" according to the combination of functions that they perform. In other words, the groups' multifunctionality is a further source of ontological variability.

In the light of this assessment, it appears that, in order to define groups in a way that endows them with a full, separate ontological status (and does not reduce them to the regulation of interpersonal behavior) and that consequently rids them of any ontological variability, we will have (a) to divorce them from the problem of patterning or regulation of interpersonal behavior, that is, from the question of solidarity, and (b) to separate analytically the different activities in which groups are involved.

AN OPERATIONAL DEFINITION OF GROUP

To abide by our second precept, let us analytically define separate groups for every different type of "activities" or "sets of interconnected activities" (or for every different function, using a more familiar language; our groups are thus posited as unifunctional)⁸ and speak of "group overlapping" when

⁸ With this notion of activities or sets of interconnected activities, I have to make allowance to intuition, since even mathematicians ultimately rely upon intuitive sets which are rooted in common sense. Anthropologists have always been working with this intuitive notion (which they often translate as "function") whenever they write about production, ritual, adjudication, warfare, and so on. In some instances only one activity is performed (as in praying, for instance), whereas in other cases different activities are interconnected (as in production); hence the distinction between activities and sets of interconnected activities.

individuals form the same group in different types of activities. In the performance of a given type of activities, moreover, an individual may allow anybody to join him or her. The resulting collection of individuals, in this instance, would form a *crowd*. On the other hand, where the individual(s) involved in an activity use(s) specific criteria (such as sex, age, type of filiation, whatnot) to discriminate between those individuals who can join and those who cannot, we will call the resulting unit a *group* if the criteria of membership are defined with respect to the individual(s) already involved in the activity. However, where discriminating criteria are employed which are defined with respect to individual(s) not involved in the activity, the resulting assemblage will be dubbed a *social category*.⁹ In other words, a group presupposes both one type of activity (and only one) and one (or many) criteria of membership.

In this definition, there enters no notion of "corporateness" or ownership. The corporateness, if it refers to collective action and solidarity, is simply taken for granted and posited as unproblematic. If it refers to ownership of an estate, however, it suggests something quite different which, in my opinion, warrants a separate definition. Whenever individuals distinguish themselves from the rest of the world by a set of criteria which gives them exclusive access to specific natural or man-made resources (tangible or intangible, although statuses are not counted among resources), the unit thus formed will be designated a *corporation*. Analytically speaking, a corporation is not a group; the members of a corporation qua members of a corporation are not involved in any activity, but in ownership. Similarly, groups are not corporations, although the two may overlap.

For every separate type of activity, it is therefore the existence of criteria of membership, and the points of reference with respect to which they are defined, which distinguishes analytically between different collections of individuals (such as crowds, groups, and social categories).¹⁰ The structural-functional model also makes use of "rules of membership" which govern the entrance to groups. But because the structural-functionalists

⁹ This is the case, for instance, when Ego's mother's brothers get together to perform something for Ego (such as his burial). They form a collection of individuals distinguished from the rest of the world because of the fact of their relationship to an individual who is himself not involved in the performance, or involved in a complementary set of activities. Where a collection of individuals are distinguished by specific criteria without ever getting involved in any activities, they form a *pure category*.

¹⁰ Criteria of membership, incidentally, have nothing to do with "criteria of group categorization." The latter notion implies that individuals have in their heads an idea about their group, or the manner in which their group operates. This idea is then thought to influence their behavior, since social action is somewhat determined by the manner in which the individual categorizes his or her social universe. Since an operational perspective stands in diametrical opposition to any social action framework, there is nothing in common between criteria of group membership and criteria of group categorization.

have confused the problem of group entry (or boundary definition) with that of regulation of interpersonal behavior, their rules of membership are ultimately equated with normative mental representations, and groups are directly subsumed under the cultural.¹¹ If we separate the problem of group definition from that of behavior regulation, the criteria of membership may be represented like axioms defining subsets in mathematics. They simply function like a franchise which discriminates among the "world outside" and delineates a set of individuals who can gain membership in the group if they so wish. These facts intimate nothing about the behavior expected of the members.¹² Whether the eligible members activate their membership, and how they behave once members, are analytically separate problems.

Criteria of membership are often, if not always, chosen because of the value attached to them, but this, once again, does not influence the group's

¹¹ Rivers (1924) had made clear the analytical distinction between the definition of the group's boundary and the organization of the behavior of its members, but this distinction was blurred by Radcliffe-Brown and his followers.

¹² I wish to make it absolutely clear that beliefs can often be used as criteria of membership. Belief in a flat earth, for instance, may be used as the basis to form a club; in itself, it says nothing about the organization of interpersonal behavior within the group. Some may also wish to argue that any criterion of membership has to be supplemented with a certain code of conduct. I do not deny the ubiquitous presence of codes of conduct, but I question their relevance to the analytical task of isolating what is specific to groups. Let us take the example of a group of production, membership in which can be gained through patrification and male gender (i.e., sons may join their father's group of production). It could be tempting to list obedience and respect as further criteria of membership, since disobedient and disrespectful sons will be ejected from the group. In order to understand the implications of such a position, let us reexamine the root of the problem. The structural-functionalists have represented agnatic descent groups, for instance, in terms of "norms of agnatic descent" pulling agnates together, these norms, however, operating with a differential strength (or pull) in different societies. No one, however, has ever dreamt of writing of the "norms of respect and obedience" pulling respectful and obedient individuals together, for the simple reason that respect and obedience, unlike such criteria as filiation, male gender, and so on, are not found systematically present with some and absent with others. They cannot be used to discriminate among individuals, since everyone can, if he so wishes, be respectful and obedient. Moreover, one who is disobedient today may be reformed tomorrow, so that his ejection would not be lasting. Finally, the same code of conduct is not limited to a particular group, but applies to all the relationships that any Ego can have with people who are senior in age or status. In other words, it is quite obvious that all individuals who activate their membership in a group are going to accept its code of conduct, but this cannot be taken to generate ontological variability in the group. Anthropologists have written about descent groups varying in degrees, because they assume that descent is one of the elements that can pull individuals together in groups and contrast them to other groups. But nobody has yet written about "obedient groups" or "respectful groups," because no one has ever considered respect and obedience to be criteria of membership. If they were to enter the definition of groups, we ought to write of "patrilineal obedient groups," implying that one can also find "patrilineal disobedient groups"! If this appears like nonsense to everybody, it follows that the code of conduct cannot enter the definition of the group.

ontological status.¹³ A mathematician may select axioms because they highlight problems in which he is interested, but the value he attaches to these axioms does not in any way affect the ontological status of the subsets they define. Moreover, the number of axioms selected does not change anything in the set's ontological status; whether one or 10 axioms are applied, the subset is not more or less of a subset. The subset will certainly have different properties according to the axioms chosen, but it will not vary in degrees. In other words, this operational definition of group seems to rid groups of their ontological variability, because it endows them with a separate ontological status. Operationally defined, groups have a reality of their own and cannot be reduced to behavior or culture. Ontologically speaking, all activities or sets of interconnected activities, as well as all criteria, are equal. There are no "degrees" of activities or criteria, so that collections of individuals defined for every separate type of activities, in terms of criteria of membership, cannot vary in degrees.¹⁴

¹³ It is also quite obvious that, in every population, individuals attach specific symbols to the various features which they select as criteria of membership, but I contend that this level of "meaning," this cultural coefficient, is also irrelevant in the definition of groups themselves. To illustrate my point, let us imagine two basketball team leaders who have to choose from a large number of individuals to form their teams. Let us assume that the two leaders decide upon height as the main criterion in their selection. It is beyond doubt that the notion of height is associated with a number of symbols and values (masculinity, power, endurance, good feeding habits, perhaps wealth, special blessing of the god, and so on). The team leaders will also have a certain purpose in choosing height as the main criterion, since tall people will presumably be better basketball players. Insofar as the teams are defined as groups, however, these values and symbols are not pertinent (i.e., they do not enter the definition of the group). Operationally speaking, height is simply an element, a feature exclusive to some and which may be used to discriminate among a large number of individuals in the formation of a team. It does not intimate anything about the behavior expected of the members of the team (such as aggressiveness, cooperation, etc.), or about their idea of what the group is.

¹⁴ Groups thus defined resemble mathematical subsets; they can be fuzzy because the axiom allows for some probability in the occurrence of events, but the axioms themselves remain axiomatic and are not fuzzy. Also, the definition of groups as multifunctional automatically raises the question of a "hierarchy of functions." If one defines a "domestic group" as united through sexual intercourse, reproduction, coresidence, biological maintenance, socialization of the children, production, and distribution (among other things), it is legitimate to inquire whether the individuals which form this group have started to mate, produce, and raise children simply because they happened to coreside, or if they perform all these activities because they have come together to raise children. In my view, there have been three dominant opinions about this hierarchy of functions: (1) the Marxist one, which views all functions or activities as derived from the essential need of biological maintenance and reproduction; (2) the structural-functional one, which regards physical reproduction and the socialization of children (altogether, social reproduction) as the key function from which all others stem; and (3) the structuralist one, which conceives all functions as springing from the universal tendency to exchange: people produce and reproduce only to keep the exchange going. These privileged functions, in turn, delineate privileged frames of reference: (1) groups of production in Marxist theory; (2) groups of social reproduction, or families, in structural-functional theory; and (3) groups of matrimonial exchange in Lévi-Strauss's theory. Such hierarchies, incidentally, are permanent

Since the question of interpersonal adjustment is set aside, the notion of "group structure" takes on a different meaning. In an operational perspective, a group is structured only when different interconnected activities (as in production, for instance) are performed by different individuals of the same group, that is, when there is a division of labor. Since some groups do not have any division of labor, insofar as all their members perform the same activity, the notion of structure is consequently not intrinsic to the notion of group.

By obviating the question of interpersonal behavior and its patterning through the agency of normative mental representations, and by postulating a group for every different type of activities, I believe this operational notion of group to solve the problem of ontological variability. If this is so, it should now be possible to derive operational definitions of kinship, marriage, and the family which ought to be more universal and therefore more suited to the task of comparative analysis.¹⁵

AN OPERATIONAL DEFINITION OF KINSHIP

Before broaching the topic of kinship, it may be apposite to say something about descent, since I hold kinship and descent to represent radically different phenomena. I have elaborated an operational notion of descent elsewhere (Verdon 1980*b*), and I will be satisfied here with repeating its most important features.

It seems to me imperative to make a further analytical distinction between the notion of group and that of *aggregated group*. With respect to certain types of activities (mostly religious and political), certain units emerge which are not groups, but "groups of groups." For the adjudication of certain offenses, for instance, a Canadian province is not a group of individuals but a conglomeration of counties. A *territorial group* is consequently not defined in terms of membership of individuals, but in terms of the fact which is used to aggregate (or merge) smaller groups. A province is

features of prescientific paradigms. In an operational framework, however, there is a separate group for every one of these activities, and since all activities are ontologically equal, there is no room for privileged frames of reference and hierarchy of functions.

¹⁵ An operational notion of group should not be confused with Malinowski's "theory of institutions." Malinowski was among the first to define social anthropology as the study of "organized behavior." In order to account for the latter, he was forced to excogitate a "charter," defined as a set of norms ordering the relationships of individuals, and to find out the purpose of such collections of individuals by seeking some needs that individuals try to satisfy by coming together. In other words, Malinowski was dealing with interpersonal behavior, its purpose, its regulation and organization by norms, and its lack of conformity with the ideal charter. An operational model, on the contrary, ignores interpersonal behavior, its purpose, its regulation and organization by norms, and its lack of conformity with an ideal charter.

therefore an aggregated group which makes use of territoriality as the element to achieve its aggregation. Descent groups belong to the same category; they are groups of groups which utilize the fact of descent (putative or real) to merge smaller groups. In such instances, there is "individual membership" only in the elementary or simple groups merged into the descent group; above that level we are not dealing with group membership, but with group aggregation.

Kinship therefore appears as a criterion of group membership, whereas descent is a criterion of group aggregation. Operationally speaking, kinship is thus simply the fact of consanguinity used as a discriminating criterion in the formation of groups, social categories, and corporations. As such, it is not held to regulate behavior, or to carry with it specific rights and duties, defining statuses. It is simply like an axiom defining a subset. From this simple definition, there nevertheless follow a number of important corollaries.

First of all, kinship is an all-embracing concept. It implies that two or more individuals can trace links of consanguinity either to a common ancestor, or lineally to one another, through single or cumulative links of filiation. Criteria of membership, however, are usually much more specific than that and usually bear on precise links, such as matrification, patrification, filiation, agnation, and so on. In other words, kinship can be said to be a criterion of membership in a given group only when it is specified that cognatic links give membership. Such a criterion, however, is not commonly found, and the notion of "kinship group" has been indiscriminately applied to (*a*) descent groups, (*b*) groups where specific links of consanguinity other than cognation have been used, and (*c*) groups where cognation is specified as the criterion of membership. Our usage dispels such confusion but seriously restricts the application of "kinship groups" to category *c*.

Second, it is also very uncommon to find only one criterion of membership operating in the formation of groups. In New Guinea, for instance, recruitment to some groups can be achieved either through patrification or through residence. Among the Abutia Ewe of Ghana, membership in elementary political groups is activated through patrification, but also through matrification in specific circumstances. In every society there are circumstances (such as illegitimacy, divorce of the parents, death of both parents, and so on) in which additional criteria come into play. Consequently, to characterize the Melanesian or Abutia groups by reference to only one of their several criteria would be misleading. They cannot be labeled "patrifiative groups" (and still less "kinship groups"), because they recognize other criteria in addition to patrification. This unfortunate tendency to conceptualize groups with reference to one dominant rule (usually kinship) has only yielded paradoxes: when other rules are dis-

covered, which operate together with kinship, anthropologists have tended to represent the characteristic rule as "weak." In my view, these problems need not be; they are the direct outcome of the manner in which groups are conceptualized. If the conceptualization is changed, the problems will disappear.

Furthermore, social anthropologists have indiscriminately characterized groups by their activities (such as "political groups" or "groups of production"), or by one (and only one) of their criteria of membership (such as kinship groups). In a comparative perspective, it seems that the activities represent the invariant part, whereas the criteria vary widely from one society to the next. I would therefore deem it more useful to characterize groups with respect to the activities in which their members are involved. It would only be warranted to label groups with reference to their criteria of membership when only one criterion is used (as in the case of age groups). Such instances, however, are rare.

In other words, the locution "kinship group" ought to be restricted to groups which select cognation as their only criterion of membership. Since these groups could adequately be described as "cognatic" and are not infrequently found, the whole notion of kinship group would more or less vanish from an operational model. This disappearance would also precipitate the demise of the family as a privileged group of reference, thereby severing the implicit connection between kinship, on the one hand, and marriage and the family, on the other. Kinship, operationally defined, is simply a criterion of group membership and not a general idiom of social relationships.

AN OPERATIONAL DEFINITION OF MARRIAGE

In an operational perspective, marriage must be defined with reference to group formation, a task which requires first the identification of the activities in which "married groups" are involved. The activity which has been almost universally acknowledged is the sexual one. In the performance of sexual activities, however, the criteria of membership in the group are far from evident. There are many negative criteria which discriminate between those with whom sex is permitted, or even desirable, and those with whom it is forbidden. However, these criteria in themselves only delineate categories; they do not serve for the formation of any group.

In fact, it seems that the only criterion which can be found in the performance of sexual activities is the performance of the activity itself (a somewhat cognate notion has already been derived by Harris [1969]). Individuals form a group in the performance of sexual activities when the very performance of this activity is used to discriminate between members and nonmembers. Such a group will then be called a "mating group."

If the sexual involvement is not used as a discriminating criterion, the individuals concerned do not form a mating group (as with involvement with prostitutes, or collective sex, or generally promiscuous relations). It follows from this definition that the sexual involvement of one of the members of the mating group with someone outside the group blurs the discrimination between member and nonmember and is likely to result in the disruption of the original group if this involvement is known to the other member (unless this outside involvement is with another spouse; as we shall see, conjugal relations are not purely sexual). Sex outside the mating group jeopardizes its existence since it contradicts its criterion of membership.

A second corollary of this approach is that mating groups can only be "mating pairs." Empirically, it is obvious that the original pair does not necessarily break up if one member forms another pair outside, but this situation would have to be accounted for in terms of personal and individual decisions (because money is involved, for instance); it does not affect the analytical postulate. The resulting "triangle" would be analytically described as two mating groups overlapping in one of their members, and not as a mating group of three individuals. This overlap would be purely circumstantial.

A mating group, however, is not necessarily a conjugal group, and I do not believe that conjugal groups can be defined with reference to sexual activities only. There are good reasons, which have been reiterated many times in the literature, for wanting to separate sex and marriage analytically, since there are conjugal groups in which no sex is involved between the partners (ghost-marriage, woman-marriage, or simply sexless marriage) and there are mating groups which are manifestly not married (such as instances of Nigerian Plateau sigisbeism, or the lover-mistress institution of Western Europe), but there are no mating groups without any sex. The conjugal group (as distinct from the mating group) seems indeed to be formed around two distinct sets of activities, namely, sex and reproduction, with the emphasis on the latter. Reproduction, however, is a natural activity (an activity performed by nature, and not by people; only copulation is performed by people) which is also internal to women.¹⁶ Individ-

¹⁶ Some may wish to argue that it is also external to women insofar as other individuals think they can influence reproduction by the use of special foods, special massages, or special rituals. This position is unfortunately untenable. The growth of plants, for instance, is another natural activity internal to a natural environment. People may form groups in the activities of planting, weeding, harvesting, and may also perform rituals to influence the land's fertility. They thus form groups of production and ritual groups to assist the growth of plants, but do not form groups in the process of growth itself, since they are not direct agents in it. Furthermore, there is strictly no scientific value in trying to conflate activities or groups. Otherwise, one ends up calling a group of production the group of individuals associated in the performance of a fertility ritual.

uals cannot therefore form groups in the activity of reproduction itself (they can in copulation, in child care, or in the rituals surrounding pregnancy, but not in reproduction itself), because they are not agents in it (it is achieved without their direct intervention, only as a result of indirect action). There is nevertheless a universal connection between sexual activities and reproduction, whether based on spiritual or physical postulates. Genitors are indeed recognized universally (Scheffler 1974).

This connection between the two activities is associated with the woman only, since reproduction is internal to women. Conjugal groups thus seem to be formed around the sexual activities of women, insofar as sex is linked to reproduction, and "marriage" would then be the criterion of membership of conjugal groups. I would suggest the following operational definition of marriage. First of all, conjugal groups are formed between one or many persons of any sex, on the one hand, and one woman, on the other, around the sexual activities of that woman insofar as they are connected with reproduction. Second, the group needs a criterion of membership. In fact, we find a conjugal group when two conditions are met: (a) children born to the woman around whose reproduction the group has been formed will not intrinsically transform or disrupt the group, and (b) these children will gain membership in the main religious and/or political groups of their mother, or of the person(s) (spouse) who has (have) formed a group around the reproduction of that woman. Any fact or set of facts (such as a ceremony, night visitations made public, cohabitation, and so on [see Goodenough 1970]) which serve in the formation of groups which meet these two conditions (i.e., conjugal groups) thus constitute the conjugal group's criteria of membership, and I define them as "marriage."¹⁷

In many societies, the child is assured of membership in a political group (as well as all other groups) whether or not his mother is married to his genitor. Marriage, consequently, is not everywhere necessary for social placement; nevertheless, where a woman's relationship to a man is not disrupted by the birth of a child, they form a conjugal group only if their child can find social placement through them. If it cannot, there is no marriage.¹⁸ Furthermore, a child born to an "unmarried pair" will trigger off a change in that pair. They may be requested to stop their involve-

¹⁷ This "fact" equated with marriage may itself be a set of activities, such as a wedding or a marriage ceremony, in which special groups are formed.

¹⁸ Among many populations of Sudan, Ethiopia, or the Nigerian plateau, for instance, a woman's first marriage (often established with the payment of bridewealth) can never be severed, although the women can leave their husbands and found stable relationships with other men, from whom they may have many children. According to my definition, such couples are not married, since their children cannot gain membership in the main political corporations or groups through either their genitor or genitrix, but only through the man who first paid bridewealth for the woman.

ment, or it may be terminated by itself, or the child may be killed, or the man may be asked to marry the girl, or they may be punished, and so on. If, after the birth of a child, a group organized around the sexual activities of the woman (whether it is mating or not) remains intrinsically unchanged and the child can trace political group membership through any member of that group, the group can be treated analytically as a conjugal group, and the fact which gave it these two properties can be identified as marriage.¹⁹ Operationally speaking, marriage is somehow defined like the notion of force in physics; it is not defined substantively, but through its effects. If the effects are observed, we can then infer the existence of marriage and attempt to identify it. By removing the need to list the bundle of rights and duties which enters the definition of marriage, this approach seems to have wider applicability. It easily extends to include woman-marriage, leviratic marriage, Caribbean marriage, Nayar marriage, and even marriage between two ghosts as practiced by the Singapore Chinese (Topley 1955, p. 35), with only one exception known to me, namely, homosexual unions. Where "married" homosexuals are allowed to adopt children, there is a fiction of reproduction, and the resulting group could by extension be considered a conjugal one. Where "married" homosexuals are barred from adopting, however, they do not form a conjugal group, and, whatever the actors say, their union is not to be treated analytically as a marriage.²⁰

Within the same population, different facts are sometimes used in the formation of conjugal groups (i.e., there are different types of marriage). These differences, moreover, appear to be associated with concomitant divergences in the women's (and children's) general social status. This stems from the fact that marriage (or, more accurately, membership in a conjugal group) is itself a criterion of membership in other groups. Where different facts are used in the formation of conjugal groups within the same society (i.e., where there are different types of marriage), one usually finds that they are used differentially in giving access to other groups or corporations. This is the operational equivalent of Leach's notion of bundle of rights. The manner in which different types of marriage in-

¹⁹ Where marriage is the only way to have access to sex, the birth of children may lead to divorce for people who did not want children. This event, however, results from personal inclinations and is not built into the group's situation.

²⁰ This raises the interesting problem of the emic definition of marriage. Although homosexuals are ritually "married" in some societies, this is far from meaning that their union should be treated analytically as marriage. Nuns may claim to be married to Jesus Christ, but no one would analyze a convent as a polygynous family! If a given people fancy claiming that their men are married to cows, this is not a sufficient ground to analyze the man-cow alliance as a conjugal group, even if sex is present. If a definition of marriage were based on the symbolic extensions that one finds in emic definitions, we would have to include the Kwakiutl case where, Boas informs us, chiefs could marry their leg!

fluence a woman's membership in other groups in a society does not in any way affect the general operational definition of marriage.²¹

AN OPERATIONAL DEFINITION OF THE FAMILY

The family consists of a conjugal group and a child born or adopted to them. In my opinion, there is not a single set of activities in which this assemblage of individuals is universally involved. Sex only takes place between the parents, and socialization and biological maintenance can be and often are performed by the mother alone or by other agents. Family members are often scattered over many residential groups and often form different groups of production and distribution of products. The only activity associated with the family, once more, seems to be reproduction. Reproduction takes place within the woman and stops at birth. After that we are dealing with biological maintenance, child care, socialization, but not reproduction. The family is thus a set of individuals, membership in which is defined with respect to an activity which is internal to one of them, but in which none of them has been directly involved. Analytically speaking, therefore, the family is not involved in any activity (Bender [1967] has already concluded that the family has no function) and thus is not a group as defined operationally.

Consequently, the collection of individuals delineated through the reproduction of a mating group will be designated a "biological set," and the collection of individuals delineated through the reproduction of a conjugal group will be termed a "family." A first corollary of this definition is that the family has neither function nor structure. Since its members are not involved in any activity, it has no "definitional function";²² and without activities, there can be no division of labor. Hence the absence of structure. At the phenomenological level the family may appear structured because of its overlap with other groups involved in specific activities and internally structured, such as groups of production, of so-

²¹ In other words, there are many different ways in which conjugal groups may overlap with other groups. Where the conjugal group coincides entirely with the mating group, for instance (i.e., where there is "no sex outside marriage"), the man himself cannot be involved in sex outside the conjugal group without the woman's consent (or without the risk of disrupting the group).

²² "Function" is the concept employed in functional theory to designate what I have described as activities or set of interconnected activities. The concept of function, however, is of dubious utility, because of its polysemic usages. There are indeed three sets of meanings which anthropologists have given the term: (a) a "definitional" notion of function, denoting a group's activities—in this case, the function of a group of production is said to be production itself; (b) a "sociological" referent of function, in which a group's function is described as the role of this group in the general process of social integration; and (c) a "mathematical" function, in the sense that *A* is said to be a function of *B*.

cialization, and so on. When the family is isolated analytically, however, it cannot logically be structured. Among some of the implications of this definition, we will note only the following.

1. Adoption is an inverted way in which individuals can gain membership in a family. It presupposes that the actual links of consanguinity of a person are either unknown, completely ignored, or rendered useless, so that he or she can then be "grafted" onto a conjugal group as if he or she had been born to this group through reproduction. Consequently, if a woman's husband raises those children of his wife who are not born to him, but the children can still activate their links to their genitor to gain group membership, the situation will be defined as a case of fostering of one's wife's child(ren), and the resulting set is not a family. If, however, the links of consanguinity to the genitor (when the genitor is different from the mother's husband) are either unknown or completely ignored, and the child gains membership in most groups through its mother's husband only, this represents a case of semiadoption, and the resulting unit is a family.

2. Since the family is formed through the birth or adoption of children to a conjugal group, it can break in only two ways: (*a*) by the death of all children or the repudiation of any kinship links with them (as when they are given in adoption or sold into slavery), or (*b*) by the termination of the conjugal group itself (through death or divorce). If the death of the father does not terminate the conjugal group, leviratic children will belong to the family of their late father, if their links to their father are used to gain membership in some groups.²³

CONCLUSION

No analysis is possible without a set of clearly defined concepts, but the concepts themselves must be defined in such a way as to render the analysis possible. Both social and cultural anthropologists are overtly dedicated to comparison, but their conceptual models do not provide support for this particular project. This has often been recognized, and the general dissatisfaction with notions of kinship, marriage, and the family (as they were sociologically or anthropologically defined) reached its peak with Goodenough's and Needham's publications (Goodenough 1970; Needham 1971). The alternatives which dissatisfied authors have suggested, however, seem to have fallen victim to the same fate as the notions against which they inveighed because, I presume, their questioning was not radical enough. Indeed, none of the "rethinking" in anthropology has ever sought

²³ Incidentally, this operational definition of marriage is also consistent with the fact that the death of a wife always terminates marriage, whereas in some societies (such as the Nuer) that of a husband does not necessarily.

to question our reliance on the intuitive, commonsense ideas of social relationships, behavioral adjustment, and solidarity.

All social theorists who hoped that their discipline would eventually share some of the rigor of the natural sciences have attempted to isolate the social level, but have in fact failed to achieve this, because they all assumed sociability, or solidarity, to be problematic. This premise prompted them to define the social in terms of the regulation that norms, beliefs, rules, rights, and duties exert on the behavior of interacting individuals. By doing so, they reduced groups to behavior, or subsumed them under culture, and denied them any separate ontological reality. This is what I identified as the main source of the conceptual malaise which is now, more than ever, bedeviling social and cultural anthropology, as well as comparative sociology.

As an attempt to solve this conceptual predicament, I have proposed an operational approach. I am eminently aware that, at the theoretical level, one set of definitions equals another and that this approach's analytical validity will only be proved if it stands the test of reality. This has already been done successfully with a number of ethnographic problems, the results of which are not yet in print. But if only the operational notion of marriage, for instance, can serve to clarify the question of marriage among the Nayars or in the Caribbeans, as I believe it does, this can serve as a first demonstration that it takes us a bit further. More exhaustive comparative analyses, unfortunately, remain a task which lies ahead.

Many, if not all, will doubt the possibility of extricating the social from the psychological and the cultural and thereby laying the foundations for a more rigorous study of social organization. For almost two centuries, if not more, this objective has remained the constant aspiration of sociologists and anthropologists alike; that they have failed, however, should not be construed as an indication that the project itself is intrinsically doomed and that we ought to steer into phenomenology. It is rather evidence that we ought to keep trying!

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Status and Status Strain in the Professions¹

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Professionals and nonprofessionals assign different status to the sub-specialties of the various professions. This follows from their different bases for prestige assignment. Intraprofessional status rests on the exclusion of nonprofessional issues or of professional issues irrelevant in a particular case. Public status for professionals rests on effective contact with disorder or nonorder. In the pursuit of intraprofessional status, professions and professionals thus tend to withdraw from precisely those problems for which the public gives them status. This phenomenon of professional regression partly accounts for many otherwise incomprehensible aspects of professional life, such as the prominence of inappropriate training, the rapid dispersal of esoteric professional knowledge to the public, and the lack of systematic approaches to such important problems as death.

A paradox confronts the student of relative status within professions. Publicly venerated professional roles are often those least respected by professionals themselves. The public admires front-line professionals like Kildare and Welby while it distrusts the "political doctors" of the American Medical Association (AMA) and ignores the researchers who "don't care about people." Yet these leaders and researchers enjoy status before their colleagues that general and family practitioners do not. The publicly admired Bailey and Belli follow the professionally disparaged specialty of criminal defense. Intraprofessionally admired tax and securities lawyers, in contrast, suffer a public reputation as willing agents of the corporate aristocracy.

At first glance, this paradox seems to reflect the public's dated images of professional life. Professionals are admired, but for the wrong reasons. Yet the evident public distrust of the professionally admired researchers, specialists, and academics suggests an important divergence between the bases of intraprofessional and public status for professionals. In this

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paper I shall outline those bases and indicate how their conflict creates some deep dilemmas for professional groups.²

By "status" I mean a quality entailing deference and precedence in interaction, a quality of professional or public honor. While status differences imply hierarchical order, one need not assume an exact ranking, but only a loose order of individuals that structures social relations. Status systems are generated by bases or dimensions of honor—power, wealth, knowledge.

INTRAPROFESSIONAL STATUS

The search for these bases of status begins with the status rankings themselves. Detailed data on intraprofessional status—status assigned groups within a profession by professionals themselves—are surprisingly scarce. Laumann and Heinz (1977) report a complete ranking of legal specialties, headed by securities, tax, and antitrust defense. Various other types of commercial law follow, with personal injury, civil rights, and criminal prosecution below them. Debtor, landlord-tenant, and divorce fall at the bottom of the scale. In medicine, Merton, Bloom, and Rogoff (1956), Becker et al. (1961), Schartzbaum, McGrath, and Rothman (1973), and Shortell (1974) have reported roughly similar hierarchies headed by specialty surgery, followed by general surgery and the various divisions of internal medicine. General practice, allergy, dermatology, and psychiatry bring up the rear. Several studies (Coker et al. 1966) have shown public health to be of low status. The general preeminence of specialists over generalists and of surgeons over all is well known. Within psychiatry, a clear hierarchy runs from psychoanalysts through outpatient psychiatrists to the foreign medical graduates at the state hospitals (Henry, Sims, and Spray 1973). In social work, psychiatric social work proved its status over general casework in the twenties (Lubove 1969). England presents

² Since this paper focuses on a particular problem in professional status relations, I shall not begin with a rigorous definition of "profession" as a concept. That would keep us from ever reaching the matter of interest. Readers interested in definitions of "profession" should consult Millerson's (1964) tabular summary before venturing further into the staggering current literature. For purposes of this paper a profession is an exclusive occupational group marketing a specialized skill based in some way on esoteric knowledge. While the examples used here will inevitably reflect the literature's concentration on the familiar cases of law and medicine, the arguments can be generalized to other types of professions as well. Thus, the opening paradox is equally evident among the managerial professions, where the public heroes are fighters and entrepreneurs as opposed to the professionally respected managers (e.g., Patton vs. Bradley). The same divergence appears in the technical professions. The public's charismatic natural scientist is the experimentalist grappling with nature at first hand. In contrast, scientists themselves accord higher status to theoreticians who work with mathematical expressions alone.

the hierarchy of barristers over solicitors and a (current) pattern among medical specialists rather similar to that in America (Abel-Smith and Stevens 1967; Stevens 1966). Many underlying dimensions have been suggested to account for these intraprofessional hierarchies. The most important are income, power, client status, and substantive difficulty (non-routineness).

Income bases its claim on the general empirical correlation of income with intraprofessional prestige. Most data on specialty incomes seem to follow prestige lines at least in outline (see, e.g., Stevens 1966; Carlin 1962). It is therefore surprising that Laumann and Heinz (1977) found no significant correlation between the two among their lawyers. In fact, income fails in a number of particular cases. English barristers have often earned no more than solicitors, yet nearly always enjoy higher status (Abel-Smith and Stevens 1967). Employed professionals in general have lower status than free-lance and associated professionals with comparable incomes. Within a given profession, academic professionals receive salaries notoriously incommensurate with their high status. Also, interacting professionals often establish status relations without knowing each others' incomes. Income must be inferred from other qualities, such as specialty, which may convey status independently.

As an alternative basis of intraprofessional status, power has a similar aggregate success rate undercut by numerous detailed failures. The alliances of professional status and power that wrought the Flexner and Reed reports are well known (Berlant 1975; Auerbach 1976). Yet while the urban district attorney lacks a judge's status, for example, his effective power is often considerably greater. Administrative physicians similarly increase their power while losing status. Domination of professional associations is only loosely associated with status. The American Bar Association (ABA) may be a positive example of such a relation, while the AMA seems a mixed case (Garceau 1961) and the American Psychiatric Association, at certain points in its history, a possible counterexample (Abbott 1980). Even within professional work groups, positions of power—managing partnerships, department chairs—are hardly regarded as status conferring, but as the reverse.³

³ One might contend that different sorts of power are involved. Thus, administrative physicians gain only extraprofessional power. Yet while the managing partnership and the departmental chair are certainly intraprofessionally powerful positions, they do not confer status. Similarly, the power of the district attorney to dispose of cases is surely an intraprofessional power, and one that effectively exceeds that of the judge; yet that power does not give the district attorney higher status than the judge. It is possible to argue that specialized skill is often a source of intraprofessional power and thereby of prestige, but in that case one would not attribute the status to power, but to the specialized skill that creates the power. Such an argument is thus a variant of the intellectual difficulty thesis considered below.

Another commonly suggested basis for intraprofessional status has been status of client. Thus, 57% of American psychoanalytic patients are professionals (Marmor 1975). Similarly, in 19th-century British medicine, physicians, surgeons, apothecaries, and chemists treated patients standing in status ranks parallel to their own (Reader 1966). Laumann and Heinz (1977) make such an argument for American lawyers today. Yet the client status argument fails with employed professionals. Governments, corporations, and armed forces are all corporate agents of great income and power, if not always of high status. Yet the professionals they employ are normally depreciated by free-lance professionals on the same technical level (see, e.g., Ben-David 1958; Harries-Jenkins 1970). Also, status-mobile professional groups do not necessarily seek high status clients. British apothecaries and surgeons emphasized their control of practice in the 19th century, not their ability to treat gentlemen (Reader 1966; Stevens 1966).

A related argument holds that, since high status clients can usually commit more resources to professional problem solving, they create problems of greater intrinsic professional interest. Money buys complexity and confers its status upon that complexity. Based on such examples as anti-trust law, this argument can be extended into such fields as accounting, architecture, and possibly psychiatry. Although it seems silly when applied to the routine areas of medicine, it does well with esoteric medicine, where excess utilization by high status clients is well known (Mechanic 1968). Again, however, this argument fails in crucial cases. While manufacturing organizations and cities are corporations of roughly equivalent power and magnitude, the former command the highest status legal services available, while the latter can attract such services only for bond issues or other matters of corporate law. Similarly, while such private mental hospitals as McLean and Bloomingdale served extremely high status clientele from the 1850s onward, they eventually lost their elite physicians to government-based research hospitals of higher intraprofessional status—the Boston and New York Psychopathic Hospitals—although the latter organizations worked nearly exclusively with charity and welfare cases (Abbott 1980).

The issue of complexity suggests an alternative basis for intraprofessional status, one asserted by functional theorists of the professions. Here, intraprofessional status is attributed to nonroutine work. (See Becker et al. [1961] for an observational study of such a basis of status.) General practitioners refer difficult (i.e., nonroutine) cases to specialists, who handle them or pass them on in turn. Indeed, it is true that exceedingly routine aspects of professional practice are often ultimately degraded to the paraprofessional level (Freidson 1970). Yet the issue of routine is complicated. Although the low status solo lawyer does mostly routine legal work in probate, real estate, divorce, and personal injury, in fact he spends

much of his time cultivating the referral network that brings him cases (Carlin 1962). While the legal aspects of his work are routine, his daily tasks include other aspects that are not. Similarly, mental hospital psychiatry is hardly routine work, although university psychiatrists hardly consider most of that work psychiatry at all. In such low status contexts, professional routine is a defense against nonprofessional intrusions (Belknap 1956). Insofar as a real level of routine work exists in professions, its status is well above that of such pariah specialties as mental hospital psychiatry, "ambulance chasing" law, and "street preaching" clergy.⁴

These hypothesized bases for intraprofessional status have much in common. They share a strong general correlation with intraprofessional status, and most assume that intraprofessional status ultimately reflects the same material variables as does general social status. They also share a generic weakness: the strong general correlations break down on detailed inspection. There are two strategies for dealing with this breakdown. The first would combine all these bases with a theory determining the conditions of their precedence in status conferral.⁵ I shall take the second course of suggesting an underlying variable.

Intraprofessional status is in reality a function of professional purity. By professional purity I mean the ability to exclude nonprofessional issues or irrelevant professional issues from practice. Within a given profession, the highest status professionals are those who deal with issues predigested and predefined by a number of colleagues. These colleagues have removed human complexity and difficulty to leave a problem at least professionally

⁴ Two different types of routineness are at issue here. One is routineness of professional work, the obverse of intellectual difficulty. The other is routineness of tasks in general, which includes routineness of professional work as a subcategory. One might thus rescue the intellectual difficulty theory of intraprofessional status by stating that the complexity involved must be purely professional. Much of the complexity of low status practice is, in reality, extraprofessional. There are two problems with this procedure. First, it does not explain why some professionally routine specialties (e.g., banking, probate) should stand above nonroutine specialties (e.g., civil rights, environment) in intraprofessional status hierarchies (Laumann and Heinz 1977). The criminal responsibility of the insane is an intellectually taxing issue for both law and psychiatry; yet as a subject of practice it commands low status in both. Second, this view imports a new variable, pure professionalism, into the analysis as a qualification on complexity. One cannot adduce such a variable without considering it independently. I shall argue below that this second variable is, in fact, the more important of the two.

⁵ There is not room here to investigate the various ways one might accomplish such a combination theory. The standard approach is to let one variable be the major determinant, with others causing fluctuations about the means it determines. Thus, Laumann and Heinz (1977) close their discussion of prestige allocation in the legal profession with an analysis suggesting that client characteristics determine the overall structure of that prestige system, with other variables providing a "multifaceted structure" to prestige. The virtue of such a conditional and hierarchical analysis lies in its ability to handle the odd cases that vary around the basic pattern, given in this case by the precedence of specialties serving corporate clients. Its danger, of course, lies in its lack of parsimony.



defined, although possibly still very difficult to solve. Conversely, the lowest status professionals are those who deal with problems from which the human complexities are not or cannot be removed.

The theoretical origins of this argument lie in anthropology. As Douglas (1970) argues, purity and contagion taboos are an extension of cultural systems. The impure is that which violates the categories and classifications of a given cultural system. Through amorphousness or ambiguity it brings together things that the cultural system wishes to separate. Nearly all writers agree that the application of esoteric knowledge to particular cases is characteristic of professions. Since this knowledge is a cultural system, it must, by Douglas's argument, define certain problems as taboo because of their threat to its principles and classifications. Thus, problems that fundamentally challenge basic professional categories are impure and professionally defiling. It is at once clear why Laumann and Heinz (1977) find that legal practice involving corporations in nearly all cases stands above that involving private individuals. The corporation is the lawyers' creation. The muck of feelings and will is omitted from it *ab initio*. Where feelings are highest and clients most legally irrational—in divorce—intra-professional status is lowest.

Over time, professional knowledge develops a system of such relative judgments of purity and impurity. All these judgments follow the same pattern. The professionally defined or definable is more pure than the undefined or undefinable. The clear is more pure than the amorphous, the definite more pure than the ambiguous. As Dumont (1972) has shown, the accretion of such judgments produces a social structure in which these judgments are loosely associated with positions in a division of labor (in this case, a division of professional labor). The resultant hierarchy is, in fact, the status hierarchy that we observe within the professions.

Defining status in terms of professional purity, of exclusion of the professionally irrelevant, immediately explains the counterexamples to the other bases of status. The barrister stands above the solicitor because he works in a purely legal context with purely legal concepts; the solicitor links the law to immediate human concerns. The free-lance or associated professional stands above the employed professional because his work is not conditioned by employer policy. The academic professional's high status reflects his exclusively intraprofessional work. Unlike income, purity of practice is easily ascertained in a short conversation as well as easily inferred from such characteristics as specialty. It is a ready currency for status.

In the case of power, the counterexamples can be explained in the same way. The judge stands above the district attorney because he may keep his professional purity through the rituals of the courtroom. The public defender and the district attorney must purify issues for his consideration.

When the judge directly enters the process of case disposition and thereby forgoes his ritual protection, he gains power but loses status (Skolnick 1967; Blumberg 1967). The administrative physician and the AMA official lose status because their professional practice is tainted by organizational and corporate ends. The managing partner and department chair, despite their positions within purely professional organizations, suffer impurity in the same way (Smigel 1964).

The counterexamples to the other theories can also be accounted for. The upward thrust of British surgeons and apothecaries was founded upon two claims of purity beyond that of the physicians, the first, purity of knowledge (rationality), the second, purity of practice (ethicality). Since higher status clients conferred no additional professional purity, they were unnecessary. Similarly, the great exception to the routine theory—the low status of nonroutine, front-line professional work—is explained by the extreme professional impurity of that work. Even though generalist practice may be more routine, it is less likely to be tainted by nonprofessional issues. The professional who enters the emergency room, the criminal court, the receiving ward, the client's home, is professionally defiled. Such defilement may follow from issues that professionals of other kinds might handle. But the act of extricating one's own particular set of problems is professionally defiling, precisely because the interwoven complexity of social life always threatens any specific set of professional judgments. Such defilement explains the inability of cities to command high status legal resources for anything other than corporate work. While such matters as bond issues involve only the professionally pure world of corporate law, in most of a city's other legal business—criminal prosecution, condemnation, eviction—the squalid realities of poverty, racism, and crime play out their drama in real human lives, mocking the pristine abstraction of the law. For the professional, the invention of tax loopholes within the exclusively legal world of corporate law is a much cleaner business.⁶

⁶ Laumann and Heinz's study (1977) provides striking confirmation of the purity thesis. The best correlate of prestige in their legal data is the degree to which a specialty implies work for altruistic or reformist motives (*pro bono* work). Such service for nonlegal ends is a strong negative correlate of specialty status, while a subfield's legal (intellectual) challenge and reputation for professional ethicality are strong positive correlates. It is clear that professional ethicality, at least, is an aspect of professional purity. The low ethicality ratings of personal injury, criminal defense, and divorce lawyers reflect the profession's fear that their judgment will be corrupted by client concerns. The low status of *pro bono* work is a similar disparagement of extralegal motives. It is interesting that Laumann and Heinz operationalize the other major correlate, intellectual challenge, as purely legal challenge, thus introducing the variable of purity inadvertently. Their exact wording is important here: "The legal doctrines, cases, statutes, and regulations involved in some types of practice are characteristically more difficult, complex, and intellectually challenging than are those in others. Would you say that the degree of intellectual challenge presented in the *substance* (as opposed to the strategic considerations) of this type of work is: very great,

Professional practice proceeds by applying abstract knowledge to a particular situation. The situation is first defined as a case, rendered into professional terms. Inferences about the professionally defined case are then made within the professional knowledge system itself. From these inferences come directives which are translated into prescriptions or answers in the client's own world. So far the discussion of impurities has focused on impurity in the first and last of these three processes, the processes of diagnosis and treatment. Thus, the depressed, unemployed welfare mother with 10 dependents and no skills has a problem too amorphous for the psychiatric system to diagnose or treat. In contrast, the so-called criminally insane present a problem whose professional diagnosis is not amorphous but rather ambiguous. A crucial variable in both diagnostic and therapeutic impurity is the degree to which the client translates for himself. Much of the correlation between the statuses of client and professional comes about because higher status clients are more likely to translate their problems into professional terms themselves (Lochner 1975) and are equally adept at translating professional prescriptions into their own world of action.

These diagnostic and therapeutic impurities may be roughly measured. The number of different professions claiming jurisdiction over a problem measures its ambiguity. The number of interprofessional contacts in referral measures its amorphousness. Predefinition can be measured directly in clients' presentations of their problems (Mechanic 1968; Freidson 1970). But not all impurities occur within diagnosis and treatment—there are problems in professional inference as well.

Some of these are caused by the use of residual terms in diagnosis. Thus, the history of dementia, dementia praecox, and schizophrenia is the history of a residual term from which other psychoses—paresis, pellagra psychosis, etc.—have gradually been removed. Since dementia was largely a residual term, few effective inferences could be made from it. It was amorphousness successfully defined. Ultimately, it proved so intraprofessionally recalcitrant that high status physicians ceased dealing with it altogether.

Other professional impurities are more complex. Some well-defined professional problems prove insoluble at the first level of professional inference and require consultation with individuals at a second level. The higher status of these consultants does not lie in their specialization, for not all

higher than average, average, lower than average, or very little?" (pp. 167–68). Since only legal complexity is measured, the variable of professional purity is implicitly included. Given that issues of the exclusion of the nonprofessional seem to sustain the other two correlates of prestige, parsimony suggests their importance in this correlation as well. Given that these correlates account for 87% of the variance in intraprofessional prestige rankings, Laumann and Heinz's study seems strong evidence indeed for the place of purity in determining that status.

of them are specialists in the common medical pattern of referral from general practitioner to internist to cardiologist to mitral valve surgeon. The barrister, the immunologist, the psychoanalyst, the senior architect are not more subject specialized than are those professionals beneath them in referral chains. What they share (with the mitral valve surgeon) is an ability to work chiefly with problems from which not only general impurities but also a number of professional side issues have been ruled out. The process of referral or consultation is thus one of successive, iterative purification. The cardiologist does not worry about liver problems; the internist has defined the patient into specialized terms for him. Such successive purification is, in fact, the norm of professional life. The intern prepares the body, the resident opens it, the cardiac surgeon works with the heart alone. Clearly, such refinement can be measured by patterns of referral, lengths of referral chains, and the like. Such studies are surprisingly scarce (but see Stevens 1966; Shortell 1972).

To the problems that successfully traverse the entire status hierarchy of purifications a profession has two responses. They may be degraded to some residual status—as functional neuroses were in medicine in the early 20th century—and referred to an outcast, low status specialty—in that case, to psychiatry. Or they may be given special status as anomalies and made the focus of high status, academic (purely intraprofessional) work in the profession. Anomalies are problems whose unresolvability symbolizes the fundamental ambiguities of internal professional inference. Since their resolution is anticipated by reduction or simplification to some “deeper level,” they symbolize for the profession the possibility of yet further purification, the possibility of stilling the dangerous conflict of present alternatives. Therein lies their charisma. As Douglas argues, “We recognize that [disorder] is destructive to existing patterns; also it has potentiality. It symbolizes both power and danger” (1970, p. 114).

Professional purity, then, is indeed the basis of intraprofessional status. It accounts for both the successes of the other bases and their failures. It is based directly on the cultural structure of professional work. It is a readily available, common currency for status. Its only problem as an account of professional status is that the public seems to ignore it completely.

EXTRAPROFESSIONAL STATUS

I noted at the outset that publicly venerated professional roles differ from intraprofessionally respected ones. Thus, media fictional professionals occupy the front-line specialties disparaged within the professions (Gitlin 1977). Professionally high status doctors no longer conduct advice services and write newspaper columns. Studies comparing specialty status percep-

tions of patients, medical students, physicians, and the public show general practitioners considerably higher on the lay scale (Shortell 1974). These differences again suggest that the basis of a profession's general status in society is not purity at all.

The high public status of the professions is unquestioned in nearly all data on occupational prestige (Inkeles and Rossi 1956; Hodge, Siegel, and Rossi 1966; Hodge, Treiman, and Rossi 1966). The literature analyzing the bases or dimensions of this public status is extensive (see, e.g., Gusfield and Schwarz 1963; Laumann 1966; Marsh 1971). The common theories are that general social status is based on power, income, and/or education. Of these, power is probably the least relevant to the high status of the professions. While doctors and lawyers may be publicly perceived as powerful, such power can hardly be attributed to scientists, college professors, dentists, and ministers—all professions high on standard occupational prestige scales.

Income is a more likely candidate. Professionals in general have large incomes. *Ceteris paribus*, income does command status in American society. Yet income again fails in detail. The public status ordering of the professions does not reproduce that of income (see, e.g., Friedman and Kuznets 1939). More important, clergy and teachers enjoy status above that of farm owners, factory owners, and numerous other occupational groups that usually stand above them in income. Small town professionals, in particular, often enjoy higher status than their incomes imply (Vidich and Bensman 1960). Certainly the subspecialties relatively more admired by the public—general medical practice and courtroom law—are not admired for their higher income (Bruhn and Parsons 1964, 1965). Indeed the whole public image of the professions has generally deemphasized income altogether (Parsons 1964). Income's problems as a basis for the general social status of the professions suggest that professions have somehow used status to achieve income rather than the reverse. This would explain the rough but inexact correlation of the two.

Education is the most serious possibility. All upwardly mobile professional groups have emphasized education as a means of collective mobility (Larson 1977; Bledstein 1976). The overall correlation of education with social status is undeniable. Certainly the importance of knowledge in the professions requires that education be closely associated with all aspects of professional life. Yet again there are nagging difficulties. On the one hand, the public often admires relatively uneducated professionals and heterodox professionals. The debates over Laetrile and Krebiozen illustrate the public antipathy to overeducated professional elites, as does the "Velikovsky affair" in another context. Moreover, American clergy, despite widely varying educational requirements across denominations, have always commanded rather high status. On the other hand, professionally educated in-

dividuals who are not active professionals do not necessarily continue to enjoy high status, as many married female professionals have found on withdrawing semipermanently from the labor force (Epstein 1970). Furthermore, given that television presents the professionals America wants to see and teaches American children about professional status, it seems surprising, if education is the basis of professional status, that professional education is almost totally neglected on television (Gitlin 1977; DeFleur 1964; DeFleur and DeFleur 1967). Its only relevance is as a source of professional efficacy. These various difficulties together imply that, if education confers status on professionals, it does so only in action.

Shils (1965) has argued that professionals enjoy high status because of the order-giving power that proceeds from their application of esoteric knowledge. While this power over disorder is most obvious in medicine, psychiatry, law, and the clergy, in fact all the professions attempt to tame disorder or to create new order. This ordering has two major properties. First, its outcome is not guaranteed. Second, it is accomplished by means of esoteric knowledge, usually acquired by extensive training. The professional, then, confronts disorder or nonorder with a system that enables him, most of the time, to control or order it. Even if he does not necessarily succeed, at least he makes effective contact with disorder via his knowledge.

It is this effective contact with the disorderly that is the basis of professional status in society. I have already discussed the charisma of disorder itself. The impure or polluting holds the possibility of change, of renewal, of reconciliation. As many deviance theorists have argued, the impure symbolizes the principles of social and cultural structure (Erikson 1966). While these properties make the various disorders and nonorders confronted by professionals charismatic, they are nonetheless still impure. To see a crime, to converse with an insane person, to nurse a sick relative—these are defiling acts for an everyday conscience, mind, or body. Only if he can control them can their charisma be transferred to one who undertakes them. In our society, as in any other, those who possess the cultural apparatus sufficient to touch and possibly control these social impurities assume their charismatic status. With his deep knowledge of cultural categories, indeed his partial membership in the group that creates them, the professional possesses that apparatus. He touches the problems and difficulties of our world without personal defilement. His reward is to assume their status. The risk of failure heightens the charisma of the disorder he confronts and hence heightens the status he draws from the confrontation, as Fox's work shows (Fox and Swazey 1974).

It is thus clear why contact seems as necessary to general professional status in society as does education. The academic professional does not contact real disorder, but only the disorder of the profession's own cate-

gories. His colleagues grant him status, therefore, but the larger public does not. The public's professional, as Gitlin (1977) shows, is an authoritative, effective idealist whose warm character and timeless knowledge stand between the passive viewer and active social disorder (see also Shortell [1974] on passivity). Only active contact enables the professional to take on the full charisma of disorder itself. Education that is not used thus cannot confer equivalent status. Conversely, effective contact itself can confer status in the absence of elaborate education or allegiance to accepted categories, as the history of charlatans and religious reformers shows. Viewing effective contact with charismatic disorder as crucial likewise accounts for the failures of income as a basis of the generally high social status of the professions. In contact with the postsputnik unknown, scientists stand above lawyers in status if not in income. The admired specialties are not referral specialties with their high incomes, but front-line, lower income specialties in immediate contact with disorder. Even the poorest country doctor can look death in the eye.⁷

CONSEQUENCES OF STATUS STRAIN

The original paradox of this paper thus gives way to a new one. Professionals themselves confer status on the exclusion of the nonprofessional. The public confers status on effective contact with the disorderly. In practice, the public prizes precisely those contacts which professionals want to escape. As professionals seek the admiration of their peers, they gradually withdraw from front-line practice. As a result, the whole profession gradually shifts toward purer practice until, as in the case of psychiatry, the original disorder is the shadow of a shade. This withdrawal from publicly charismatic disorders, both within individual careers and within professions over time, is a kind of drawing back into purity, a regression. Such professional regression is a fundamental feature of professional life.

Some of its consequences are internal to the profession itself. As the status hierarchy grows away from the basic disorder, a massive problem of

⁷ In commenting on this paper, Michael Moffatt has suggested that an important element in the public's apparent reversal of intraprofessional hierarchies is the public's perception of a given profession as a uniform body rather than a differentiated one. On this argument, the general practitioner is the most familiar, and hence the best, example of a professional. Deviations from this exemplar—researchers, specialists, and the like—are deviations from the perfect type and hence lose status. To the degree that the public really knows the profession's own system of knowledge, it should itself adopt the typical intraprofessional status ranking, for then it knows the professionally perceived bases of effective contact. This may account for the less than perfect inversion of the two rankings in the medical case (Shortell 1974). For the most part, however, since the public views any member of a given profession as possessing the cultural apparatus necessary to efficacious contact with disorder, the public views those with the greatest appearance of contact as being of highest status.

integration emerges. This problem is addressed by a number of strategies. One is to force all careers to start in the charismatic heartland. Thus arise the anachronistic rituals of moot court, cadaver dissection, and missionary internships. Corporate strategies for reunification include the hegemony of general over specialty associations (American law has steadfastly refused to acknowledge the latter) and the creation of heartland renewals for the elite, such as community mental health and legal aid. Strategies for maintaining unity in knowledge and skills include continuing education, codification, recertification, and elite "reform" of lower status practice in the heartland. All of these seek to redress the increasing disunity caused by professional regression.

The consequences of professional regression reach beyond the profession itself to the interprofessional world. As regression occurs, new professions and old rivals are waiting to take over the charismatic dirty work and earn its status. The interprofessional consequence of regression is conflict and competition. (For a brief case study, see Pettigrew [1973].) Jurisdiction over the heartland is maintained in several ways. Despite their low status, front-line professionals are seldom totally excluded. Interns and other trainees also serve as *locum tenentes*. At the corporate level, joint committees are founded to settle boundary disputes. Thus, the criminal responsibility of the insane, despite the minimal status of psychiatric practice with the insane and of legal practice of criminal defense, commands extensive cooperative elite attention from both professions lest important dimensions of jurisdiction be lost. In reality, at the front line, such issues are handled by extremely dirty (in professional terms) collaboration and working consensus among psychiatrists, judges, district attorneys, and mental health officials.

In the area of skills, competition couples with the need for intraprofessional unity to produce a characteristically defensive systematization designed to sharpen professional boundaries. Thus lawyers wish to abolish the incompetency plea on grounds of systematization (Burt and Morris 1972), while psychiatric researchers are seeking the true nature of responsibility in the brain. Each profession rationalizes to defend its jurisdiction over the "criminally insane." This defensive systematization and the consequent sharpening of professional boundaries lessens the ability of the professions as a group to deal with problems not amenable to the highly rationalized mappings of the professional world. Thus, death, surely a charismatically disorderly event, involves clerical, medical, legal, and possibly psychiatric professionals; yet as a general problem it is largely in the hands of funeral directors, outcast pseudoprofessionals (Habenstein 1954). The conflict engendered by professional regression has the functional consequence of making the professions better at what they do do and worse at what they do not.

In addition to these intra- and interprofessional effects, professional regression has consequences in the general social context as well. There the great problem created for the profession is the maintenance of public prestige despite withdrawal from its real basis. Individual careers can partly accomplish this—hence the highly visible location of medical training in the emergency room. High status professionals often serve on the boards (and usually only the boards) of charity and other organizations dealing with their heartland disorders. Another career strategy, part-time work in legal aid, has maintained charismatic, public visibility for lawyers. More important than career visibility, however, is a systematic campaign of public relations aimed at persuading the public of the active contact of profession and professionals with publicly charismatic disorder. The AMA founded its Physicians Advisory Committee to Television, Radio, and Motion Pictures in 1955, and its successes are evident. The relative public invisibility of intraprofessionally high status, regressed professionals is also noteworthy.

In the general social context, regressing professions face a difficult choice about knowledge and skills. The threats of internal disunity and interprofessional competition lead to defensive systematization, but this may make professional paradigms visibly removed from common sense. The *Miranda* decision preserved legal hegemony over police discretion, but proved publicly costly. Medical paradigms decreed that Laetrile need not be tested, but public outcry has forced a test. Such dangerous external challenges must be either silenced or ingested. But even ingestion, along with impression management and career linkages, often does not suffice to maintain proper levels of professional charisma in the context of professional regression.

Professions may therefore also follow a final strategy, that of slowly dispersing professional charisma to the public. Both by accident and by design, the "profession shows" on television have helped in this. More important are the professionals whose public pronouncements interpret the profession's function and its vision of order to the public. These are the public speakers, the testifiers, the professionals quoted in the media. Important also are the popularizers, whose books dole out professional knowledge and thereby some of the charisma implicit in the effective contact with disorder enabled by that knowledge. Yet ultimately this dispersal of charisma puts the public in the professional fellowship, in the company of the order-givers (Shils 1961). Thus, the slow dispersal of Freudian psychiatry to the American public has ultimately resulted in widespread threats to psychoanalytic supremacy in the area of emotional problems.⁸

⁸ These threats are being met with professional countermoves. One may interpret the concept of narcissism, first promulgated in its current form in Kohut's opaque monograph (1971), as an attempt at the decisive obfuscation necessary to recoup profes-

There are thus numerous developments in the American professions that can be viewed as consequences of the conflict between internal and external status rankings. Such a status conflict argument can also be generalized more effectively than traditional approaches to status within professions. For example, the different dynamics of purity among German lawyers lead to a higher intraprofessional ranking for generalists, although the income hierarchy among specialties resembles that in America (Rueschmeyer 1973). A more interesting case is that of Brahmins in traditional Indian society, who can be shown to have undergone similar processes of regression. Temple and family Brahmins suffer a loss of status for performing precisely those priestly functions on which their premier place in the social hierarchy rests. The highest status is reserved for "the learned Brahmins who do not serve at all" (Dumont 1972, p. 109; see also Mandelbaum 1972, p. 188). The comparison of status bases proves essential in understanding professional dynamics in earlier Western societies as well. Precisely because the public status of 18th-century English professionals rested on an ancient purity hierarchy (that of sciences, liberal arts, mechanical arts, and labor, a hierarchy more fully expressed in France), there was little conflict-induced change in that period. The public and intraprofessional hierarchies were similarly based. It is the general shift from a public status based on gentlemanliness to the more rationalized status based on effective contact that begets the status dynamics discussed here. Of course, status dynamics are but one of many processes in professional change. But independent appraisal of them seems essential to the history and sociology of the professions.

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sional charisma. By remystifying crucial areas of professional jurisdiction, this concept hopes to remove them from public purview and, indeed, from the purview of other professions as well.

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Latent Structure Models of Mobility¹

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This paper proposes several latent structure models for the analysis of mobility tables and examines the relationship of these to some earlier mobility models (e.g., the "perfect" and the "quasi-perfect" mobility models). Data from the classic Danish (5×5) and British (5×5 and 8×8) mobility tables are used to illustrate the utility of these methods in comparative analysis. A model designated as a quasi-latent structure is suggested as a plausible rendering of the structure of mobility for each set of data, and this model is used to derive various kinds of substantive inferences.

On several occasions, Boudon (1972, 1973, 1975) has proposed that latent structure models be developed for the analysis of mobility tables. Others have alluded to a latent structure approach for the analysis of mobility without actually bringing the general latent structure model into the center of their mode of analysis (e.g., Bartholomew 1973; Goodman 1969; Hauser 1978; Pullum 1975). This paper is a systematic attempt to model mobility processes from the latent structure point of view. It is based for the most part on methods developed in very different contexts by Goodman (1974a, 1974b; cf. Lazarsfeld and Henry 1968; Clogg 1977, 1979a, 1980b). These methods will be applied to classical data on intergenerational mobility in Denmark and Great Britain to illustrate their utility in various kinds of comparative analysis.

Section I defines the latent structure model, describes important deductions which can be made from it, and elaborates the substantive meaning of model parameters in the context of mobility-table analysis. Analogies are drawn between the latent structure approach and some approaches based on other kinds of multiplicative models (e.g., models of quasi independence or "quasi-perfect" mobility), between the latent structure approach and certain regression approaches to analyzing the dependence of destination on origin, and between the latent structure approach and certain Markov-type models often used to examine the time dependence exhibited in the transitions among statuses across time.

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Section II applies unrestricted latent structure models to three sets of data: the Danish 5×5 table and the British 5×5 and 8×8 tables. Section III applies some restricted latent structure models to the same data. A particular type of quasi-latent structure is suggested as an alternative to the quasi-perfect mobility model. This new model is similar in many respects to the "mover-stayer" model which has been influential in mobility research for over two decades, but differs from it by positing two latent classes of movers instead of a single class of latent movers. This section also presents new measures of immobility, the assignment of mobile and nonmobile persons into latent classes, and the Markov-type transition matrices derived from the latent structure models.

I. THE LATENT STRUCTURE APPROACH TO THE ANALYSIS OF MOBILITY TABLES

The Latent Structure Model

Let π_{ij} refer to the expected proportion in the (i, j) cell of an $I \times I$ mobility table, where $i = 1, \dots, I; j = 1, \dots, I$. The row variable will refer to the origin statuses (e.g., father's occupational category) and will be denoted by the symbol O . The column variable will refer to the destination statuses (e.g., son's occupational category) and will be denoted by the symbol D . With $\pi_i^O = \sum_j \pi_{ij}$ defining the marginal distribution of the statuses at the time of origin, the mobility rates can be written as $r_{ij} = \pi_{ij}/\pi_i^O$. Models explicitly devised for the π_{ij} are also implicit models for the r_{ij} , and vice versa, a fact to which I shall often refer. The categories of O and D are deliberately referred to as statuses, that is, positions in social structure broadly defined, although clearly in some cases this designation may not be appropriate.

Since the origin status variable O is logically prior to the destination status variable D , it is natural to attempt to characterize the variable that intervenes between O and D . This intervening variable may be denoted as X , and I hasten to point out that X might actually be composed of several variables, each one of which might be important in its own right as a (partial) intervening variable. For the present argument, X will refer to the composite representation of all intervening variables, a single variable formed in an appropriate way by considering the joint variable with components equal to each of the intervening variables. Standard methods imply that, if X can be adequately specified and properly measured, the association between O and D will disappear when X is "controlled." This means that the association between O and D would be nil in each conditional table formed by taking the cross-classification of O and D at each level of X .

If X were observable and discrete, standard contingency-table methods could be used to test whether X has been properly specified; an elementary

log-linear model ($O \times D|X$) would be of interest. If O , D , and X were quantitative and X were observable, standard methods appropriate for linear models could be used instead. This type of analysis would focus on the partial regression coefficient $\beta_{DO \cdot X}$ and would lead to the conclusion that X is properly specified whenever $\beta_{DO \cdot X} = 0$. Yet another approach to specifying the X variable which intervenes between O and D is the latent structure approach on which this paper is based. Variables O , D , and X will be regarded as discrete (note that O and D are always discrete in the mobility-table context), but X will be conceived as "latent," or inherently unobservable. The latent structure approach is distinguished from a linear model approach only by (a) positing discreteness for all variables and (b) positing that the intervening variable X is latent or incapable of being infallibly measured in a direct way. The latent structure approach allows a characterization of the intervening variable X without bringing additional information into the analysis, and characterizing this variable can be viewed as the primary objective of this research.

Let us suppose that X has T categories; the categories of X will be indexed by t , with t ranging from 1 to T , and these categories are referred to as the classes of X . The use of the term "class" is deliberate, just as the term "status" was deliberately chosen to refer to the categories of O and D . It will be seen later that the classes of X are, in general, mixtures of the statuses of O and D . Certain sets of statuses will serve to define these classes, though, strictly speaking, only in probabilistic terms. A major objective of the analysis will be to determine precisely which sets of statuses serve to define the respective classes of X . What is being suggested is that the pattern of mobility observed in the $O \times D$ mobility table can serve to define classes, or groups of statuses, within which the structure of mobility takes on a special character. In most respects, this approach will define a class of persons as a group which possesses random mobility chances with respect to the statuses which together constitute the particular class. That is, the absence of structured barriers to mobility among statuses within a class is the defining characteristic of a latent class of individuals. The close isomorphism between this statistical definition of a latent class and at least some sociological concepts of social class is perhaps the principal justification for the approach presented herein.

Let π_{ijt}^{ODX} refer to the expected proportion in the (i, j, t) cell of the indirectly observed $O \times D \times X$ cross-classification. We assume that

$$\pi_{ij} = \sum_{t=1}^T \pi_{ijt}^{ODX}, \quad (1)$$

which expresses the idea that the observable proportions π_{ij} are obtained by collapsing, or taking marginals over, the latent classes of X . If X is the variable intervening between origin status (O) and destination status (D),

then the following relationship will also hold:

$$\pi_{ijt}^{ODX} = \pi_t^X \pi_{it}^{\bar{O}X} \pi_{jt}^{\bar{D}X}, \quad (2)$$

for all i, j , and t . In equation (2), π_t^X refers to the probability that a randomly chosen member of the population will be a member of the t th latent class, that is, the proportion of the population in latent class t . The parameter $\pi_{it}^{\bar{O}X}$ refers to the conditional probability that a member of the t th latent class will have origin status i (for $i = 1, \dots, I$), and the parameter $\pi_{jt}^{\bar{D}X}$ refers to the conditional probability that a member of the t th latent class will have destination status j (for $j = 1, \dots, I$). Equation (2) states that O and D are conditionally independent, given the levels of X , as should be the case if X intervenes between O and D . Note should be taken of the meaning of the parameters on the right-hand side of (2) in the mobility-table context: the π_t^X provide the distribution of the classes indirectly observed in the mobility table, and the $\pi_{it}^{\bar{O}X}$, $\pi_{jt}^{\bar{D}X}$ describe the distribution of the statuses within each class. Substituting (2) into (1) produces the fundamental equation of latent structure analysis:

$$\pi_{ij} = \sum_{t=1}^T \pi_t^X \pi_{it}^{\bar{O}X} \pi_{jt}^{\bar{D}X} \quad (3)$$

(Goodman 1974a; Lazarsfeld and Henry 1968).

Some Deductions from the Model

Let $\pi_{ijt}^{OD\bar{X}}$ denote the conditional probability that a person who has the (i, j) mobility pattern will be a member of the t th latent class. Applying the definition of conditional probability gives

$$\pi_{ijt}^{OD\bar{X}} = \pi_{ijt}^{ODX} / \pi_{ij}. \quad (4)$$

In general, $\pi_{ijt}^{OD\bar{X}}$ could be greater than zero for more than one value of t , implying that persons with a given mobility pattern (i, j) might be drawn from more than one latent class. A rule must be devised which provides an "optimal" prediction of latent class membership for any particular mobility pattern. Let t' refer to the class of X for which $\pi_{ijt}^{OD\bar{X}}$ is at its maximum (i.e., t' is the modal class of X for the $[i, j]$ mobility pattern). One prediction rule suited for the problem at hand is to assign all individuals with the (i, j) mobility pattern to the t' th class. When this is done, the expected proportion misallocated for the (i, j) mobility pattern is

$$E_{ij} = 1 - \pi_{ijt'}^{OD\bar{X}}, \quad (5)$$

and the expected proportion misallocated into the latent classes over the whole table would be

$$E_2 = \sum_{i,j} E_{ij} \pi_{ij}. \quad (6)$$

The proportion correctly allocated into the latent classes given a model for the π_{ij} is merely $1 - E_2$. If the distribution of X were known in advance, an unconditional assignment of individuals into the classes of X could also be made. If t^* were the class of X where π_{it^*} is at a maximum, an assignment rule similar to that discussed above would place all individuals in the t^* th latent class, irrespective of their mobility patterns. The expected proportion of errors with this assignment would be

$$E_1 = 1 - \pi_{t^*}^X. \quad (7)$$

A measure of association seems appropriate as an index of the degree to which errors in predicting class membership are reduced when mobility patterns are taken into account. A proportional-reduction-in-error measure consistent with the definition of E_1 and E_2 above is the asymmetric λ ,

$$\lambda_{X \cdot OD} = (E_1 - E_2)/E_1 \quad (8)$$

(see Goodman and Kruskal 1954). As suggested by Clogg (1979b), these measures can be of use in assessing the quality of a latent structure model in terms different from usual criteria based on goodness-of-fit of the model to data.

Returning now to the model parameters in (2), we note that $\pi_{it}^{\bar{O}X}$ appears to be defined in a way inconsistent with the specification of X as an intervening variable consequent to the origin status variable O , since it defines the conditional probability that the antecedent variable O takes on status i given that the consequent variable X is at level t (i.e., class t). We must quickly point out that X might actually be conceived as coexistent in time with O , in which case this logical difficulty would be less important. Or, perhaps more realistically, X could be conceived as containing component variables, one or more of which are coexistent in time with O and one or more of which are consequent to O . Whatever the interpretation given to X (i.e., as coexistent in time with O , partly coexistent in time with O , or entirely consequent to O), the important idea is that the $\pi_{it}^{\bar{O}X}$ cannot be interpreted as simple "recruitment" probabilities, as is customary with the general latent structure model in many substantive contexts (cf. Lazarsfeld and Henry 1968).

Let $\pi_{it}^{O\bar{X}}$ denote the conditional probability that a member of the i th origin status will be in the t th latent class. Clearly, this can be written as

$$\pi_{it}^{O\bar{X}} = \pi_{it}^{OX}/\pi_i^O = \pi_{it}^{\bar{O}X}\pi_i^X/\pi_i^O. \quad (9)$$

Now the mobility rates $r_{ij} = \pi_{ij}/\pi_i^O$ can be written as

$$r_{ij} = \sum_{t=1}^T \pi_{it}^{O\bar{X}}\pi_{jt}^{\bar{D}X}, \quad (10)$$

using (9) and (3). Equation (10) says that the probability of moving from origin status i to destination status j is the sum of the T probabilities of

moving from origin status i to latent class t and from latent class t to destination status j , where the moves (status $i \rightarrow$ class t) and (class $t \rightarrow$ status j) are independent of each other, for $t = 1, \dots, T$. Equation (10) is similar to the Chapman-Kolmogorov identity for Markov chains (Feller 1968, p. 383), and it is clear that equations (10) and (3) imply each other. The π_{it}^{OX} can be interpreted as recruitment probabilities (i.e., they are the probabilities that a member of origin status i is recruited into the t th latent class), and the π_{jt}^{DX} can also be so interpreted (i.e., they are the probabilities that a member of class t will be recruited into the j th destination status). The latent structure model thus bears some resemblance to a Markov-type model, with the important specification that the intervening states in the transition from status i to status j are unobservable or latent. The observable mobility rates π_{ij} are mixtures of the transition rates π_{it}^{OX} , π_{jt}^{DX} derived from the latent structure model.

Some Special Cases

Let us now consider some special cases of the latent structure model defined in equations (1)–(3). First consider a model H_1 where $T = 1$, implying that $\pi_{i1}^{OX} = \pi_i^O$, $\pi_{j1}^{DX} = \pi_j^D$. In this case, (3) can be written as

$$\pi_{ij} = \pi_i^O \pi_j^D, \quad (11)$$

demonstrating that H_1 is the hypothesis of independence between O and D , usually described as a model of “perfect mobility.” It could be said that H_1 posits the nonexistence of a latent class structure, since the “class variable” X is degenerate, possessing only one class. Mobility is completely random with H_1 , depending only on the distribution of statuses at origin and destination, and it is precisely this feature which demands that the occupational statuses be interpreted as designating only one class. Random mobility patterns imply that no structured barriers to mobility exist, apart from those constraints implied by differences in the two relevant status distributions. When the number of classes T is greater than one, a class structure is posited to exist, and these cases are of most interest to us here. If $T = 2$, for example, (3) would imply that within each latent class mobility would be random, depending only on the marginal distributions of the statuses at origin and destination within each class (i.e., π_{it}^{OX} , π_{jt}^{DX}), and a “class barrier” between two classes would be said to exist.

Consider next a model where $T = I + 1$ and where $\pi_{ii}^{OX} = \pi_{ii}^{DX} = 1$, for $i = 1, \dots, I$. Then (3) reduces to

$$\pi_{ij} = \begin{cases} \pi_i^X + \pi_{I+1}^X \pi_{i,I+1}^{OX} \pi_{j,I+1}^{DX}, & \text{for } i = j, \\ \pi_{I+1}^X \pi_{i,I+1}^{OX} \pi_{j,I+1}^{DX}, & \text{for } i \neq j, \end{cases} \quad (12)$$

using the fact that $\pi_{it}^{OX} = \pi_{it}^{DX} = 0$ when $i \neq t$. The first I classes of X are actually deterministic “status classes,” since the π_i^X , for $i = 1, \dots, I$,

define the proportions in particular latent classes who stay in status i with probability one. We can let

$$\alpha_i = \pi_{I+1}^X \pi_{i,I+1} \bar{O}^X,$$

$$\beta_j = \pi_{j,I+1} \bar{D}^X,$$

and

$$\gamma_i = 1 + \pi_i^X / \alpha_i \beta_i, \quad \text{for } i = 1, \dots, I,$$

and then (12) reduces to

$$\pi_{ij} = \begin{cases} \alpha_i \beta_j \gamma_i, & \text{for } i = j, \\ \alpha_i \beta_j, & \text{for } i \neq j. \end{cases} \quad (13)$$

When the γ_i are all nonnegative, this $(T+1)$ -class restricted latent structure is equivalent to the model of quasi-perfect mobility, and the parameters γ_i are the "new indexes of immobility" presented by Goodman (1969, 1972) to measure status inheritance.

Another model similar to the quasi-perfect mobility model occurs when $T = I + 2$ and when the same 0-1 restrictions on the conditional probabilities are imposed. In this case, (3) would imply

$$\pi_{ij} = \begin{cases} \pi_i^X + \sum_{t=I+1}^{I+2} \pi_t^X \pi_{it} \bar{O}^X \pi_{jt} \bar{D}^X, & \text{for } i = j, \\ \sum_{t=I+1}^{I+2} \pi_t^X \pi_{it} \bar{O}^X \pi_{jt} \bar{D}^X, & \text{for } i \neq j. \end{cases} \quad (14)$$

Such a model was introduced by Goodman (1974a) in a very different context and was designated a quasi-latent structure. For the model described by (14), there are I deterministic status classes, where status inheritance (or immobility) occurs with probability one. However, two classes of "latent movers" are posited to exist (instead of just one class of latent movers), and that is all that distinguishes this model from the quasi-perfect mobility model. When considering the quasi-perfect mobility model as applied to various sets of mobility data, Pullum (1975) found it necessary to modify the model by allowing for deterministic classes of persons having a characteristic form of mobility (e.g., mobility from status 1 to status 2) in order to explain the clustering adjacent to the main diagonal in the mobility table. The generalization of the quasi-perfect mobility model suggested in the quasi-latent structure of (14) is to posit the existence of two probabilistic classes of latent movers. It will be seen later that this generalization of the quasi-perfect mobility model fits the data very well, providing an adequate accounting for the clustering that is typically observed on or adjacent to the main diagonal.

To draw out more fully the relationships between the quasi-perfect mobility model and the quasi-latent structure model, let

$$\alpha_{i1} = \pi_{I+1}^X \pi_{i,I+1} \bar{\alpha}^X,$$

$$\alpha_{i2} = \pi_{I+2}^X \pi_{i,I+2} \bar{\alpha}^X,$$

and

$$\beta_{j1} = \pi_{j,I+1} \bar{\beta}^X,$$

$$\beta_{j2} = \pi_{j,I+2} \bar{\beta}^X.$$

With these quantities, (14) can be written as

$$\pi_{ij} = \begin{cases} \pi_i^X + \sum_{k=1}^2 \alpha_{ik} \beta_{jk}, & \text{for } i = j, \\ \sum_{k=1}^2 \alpha_{ik} \beta_{jk}, & \text{for } i \neq j. \end{cases} \quad (15)$$

Letting $s_{ij} = \sum_k \alpha_{ik} \beta_{jk}$, we can write

$$\gamma_i^* = 1 + \pi_i^X / s_{ii}, \quad (16)$$

and (14) and (15) can be rewritten as

$$\pi_{ij} = \begin{cases} s_{ij} \gamma_i^*, & \text{for } i = j, \\ s_{ij}, & \text{for } i \neq j. \end{cases} \quad (17)$$

Equations (15)–(17) show that γ_i^* is a measure of the status inheritance in status i (for $i = 1, \dots, I$), relative to the quasi-latent structure model. That is, γ_i^* (which will be greater than or equal to one, since $\pi_i^X \geq 0$) measures the surplus of stayers in status i which cannot be accounted for by the expected immobility under a model positing two latent classes of movers.

II. UNRESTRICTED LATENT STRUCTURES

To my knowledge, latent structure models have not been previously applied to mobility tables in published research (except for Clogg 1980b), nor have they been applied to analyze other kinds of two-way cross-classifications. However, no peculiar difficulties are encountered in applying latent structure models to two-way cross-classifications like mobility tables, as this section will demonstrate. Attention will be devoted here to unrestricted latent structures for mobility tables with the objectives of demonstrating the general approach just discussed, clarifying some identification problems which arise, and presenting some of the deductions which can be made from these relatively simple models.

The Data

Table 1 presents three mobility tables pertaining to intergenerational occupational mobility in Denmark and Great Britain. The Danish 5×5

table is derived from Svalastoga (1959), and the British 5×5 table is derived from the published 7×7 table in Glass (1954, p. 183). The British 8×8 table is taken essentially from Miller (1960, p. 71), who obtained the data personally from Glass.² These tables, or various condensed versions of

TABLE 1
THREE SETS OF DATA PERTAINING TO INTERGENERATIONAL SOCIAL
MOBILITY IN DENMARK AND GREAT BRITAIN
A. DANISH 5×5 TABLE ($N = 2,391$)

FATHER'S STATUS	SUBJECT'S STATUS				
	1	2	3	4	5
1.....	18	17	16	4	2
2.....	24	105	109	59	21
3.....	23	84	289	217	95
4.....	8	49	175	348	198
5.....	6	8	69	201	246

B. BRITISH 5×5 TABLE ($N = 3,497$)

FATHER'S STATUS	SUBJECT'S STATUS				
	1	2	3	4	5
1.....	50	45	8	18	8
2.....	28	174	84	154	55
3.....	11	78	110	223	96
4.....	14	150	185	741	447
5.....	0	42	72	320	411

C. BRITISH 8×8 TABLE ($N = 3,497$)

FATHER'S STATUS	SUBJECT'S STATUS							
	1	2	3	4	5	6	7	8
1.....	50	19	26	8	7	11	6	2
2.....	16	40	34	18	11	20	8	3
3.....	12	35	65	66	35	88	23	21
4.....	11	20	58	110	40	183	64	32
5.....	2	8	12	23	25	46	28	12
6.....	12	28	102	162	90	553	230	177
7.....	0	6	19	40	21	158	143	71
8.....	0	3	14	32	15	126	91	106

NOTE.—See text for derivation of these sets of data; see n. 2 concerning correction of an error in the 8×8 table.

² The British 8×8 table presented by Miller (1960) and used by Duncan (1979) is apparently in error, since the numbers in Miller's cells (5, 5), (5, 6), (6, 5), (6, 6) do not agree with the percentage reported for the sum of these cells in the 7×7 table presented by Glass (1954). I have changed the count in Miller's (6, 6) cell from 554 to 553 to make the sum of these four entries agree with the count for the corresponding single entry in the published 7×7 table.

them, have been analyzed so extensively that little comment about their makeup is required.

The statuses in the 8×8 British table correspond to the following occupational categories: (1) professional and high administrative; (2) managerial and executive; (3) inspectional, supervisory, and other non-manual (high grade); (4) inspectional, supervisory, and other nonmanual (low grade); (5) routine grades of nonmanual; (6) skilled manual; (7) semi-skilled manual; (8) unskilled manual (Miller 1960, pp. 70-72). For the 7×7 table published in Glass, status categories 5 and 6 were combined; and in the 5×5 table given in table 1, status categories 2 and 3, status categories 5 and 6, and status categories 7 and 8 were combined. Svalastoga (1959) is responsible for collapsing the British table into its 5×5 form; he did so to make it more comparable to the Danish 5×5 table also reported in table 1. These data thus allow two distinct types of comparative analysis: a direct comparison of mobility structures in Denmark and Great Britain based on the two 5×5 tables (assuming that the occupational statuses are comparable between the two tables) and a comparison of mobility structures in a relatively coarse and in a relatively fine categorization of occupational statuses for the same society based on the British 5×5 and 8×8 tables. Special note should be taken of the zero count for the (5, 1) cell in the British 5×5 table and of the zero counts in the (7, 1) and (8, 1) cells in the British 8×8 table. These zero counts will require simple modification of some of the latent structure models proposed herein.

Two-Class Models

Table 2 presents several summary statistics and indexes pertaining to unrestricted models for the three sets of mobility data. The usual χ^2 statistics are reported, with X^2 referring to the Pearson goodness-of-fit statistic and L^2 to the χ^2 statistic based on the likelihood-ratio criterion. Indexes of fit not directly based on the magnitude of X^2 or L^2 are required because the data have been obtained by rather complicated sampling arrangements, and the descriptive levels of significance associated with the χ^2 statistics should not be taken at face value. The index of dissimilarity between the observed and expected frequencies is presented as a simple index of fit and will be used often to compare models. Its use is suggested here because it does not depend in a direct way on sample size, and it can therefore be used to make comparisons between models applied to the Danish table (where $N = 2,391$) and models applied to the British tables (where $N = 3,497$). Other indexes presented in table 2 allow assessment of the fit from different points of view. One is the likelihood-ratio χ^2 statistic divided by its degrees of freedom, $F = L^2/\text{df}$, a quantity analogous to the F -statistic of regression analysis (cf. Haberman 1978, p. 17). A final index of fit is the R^2 , which is

TABLE 2
UNRESTRICTED LATENT STRUCTURES APPLIED TO THE THREE
MOBILITY TABLES: INDEXES OF FIT

Model	df	L^2	χ^2	Δ (%)	$F = L^2/df$	R^2
Danish 5 × 5 Table						
H_1	16	654.2	754.1	19.4	40.9	...
H_2	9	116.2	133.6	8.8	12.9	.82
H_3	2	32.7	33.3	5.0	16.4	.95
British 5 × 5 Table						
H_1	16	811.0	1,199.4	15.4	50.7	...
H_2	9	182.3	206.7	8.0	20.3	.73
H_3	2	55.1	49.8	4.6	27.6	.93
British 8 × 8 Table						
H_1	49	954.1	1,415.3	16.6	19.5	...
H_2	36	253.5	282.4	9.3	7.0	.73
H_3	21	96.7	99.3	6.5	4.6	.90

calculated by using the L^2 of the baseline model of independence (namely, the model of perfect mobility) as a measure of overall variability to be accounted for and the L^2 of successive models as measures of residual variability.

The one-class model presented in (12), equivalent to the hypothesis of perfect mobility or independence, is denoted as H_1 in table 2. The model clearly does not fit well for any table, and judging from $L^2(H_1)$ the British data appear to depart most seriously from the prediction of H_1 . But the British sample is 46% larger than the Danish sample, and upon multiplying $L^2(H_1)$ for the Danish sample by 1.46 (yielding 956.4) it is easy to see that, contrary to the ordering of the unadjusted χ^2 statistics, the Danish table exhibits a more marked departure from the one-class model. It can be noted that this feature of the data is exhibited clearly in the comparison of Δ 's. Model H_1 and the summary indexes associated with it are presented here solely for purposes of gauging the improvement obtained by considering other models to be considered next.

The two-class latent structure model denoted by H_2 provides a moderate improvement over H_1 for each set of data. The Δ 's range from 8.0% to 9.3%, and the R^2 s range from .73 to .82. (Here it can be noted that the R^2 and Δ lead to slightly different interpretations of the degree of fit, since Δ is the lowest [8.0%] for the British 5 × 5 table, but R^2 is the highest for the Danish 5 × 5 table, even though neither R^2 nor Δ depends in a direct way on sample size.)

A comment on the degrees of freedom associated with H_2 is appropriate, owing to the fact that the parameter estimates are not locally identifiable

(cf. Goodman 1974b). In the 5×5 table there are 25 expected proportions π_{ij} , but one of these can be ignored, since $\sum_{i,j} \pi_{ij} = 1$, giving a total of 24 nonredundant π_{ij} 's. There are two latent class proportions π_1^X , π_2^X , but one of these can clearly be ignored, since $\sum_i \pi_i^X = 1$. There are $5 \times 2 = 10$ of the $\pi_{it}^{\bar{O}X}$, but since $\sum_i \pi_{it}^{\bar{O}X} = 1$ we can ignore two of these (one for each class $t = 1, 2$); similar logic shows that two of the $\pi_{jt}^{\bar{D}X}$ can be ignored. These results seem to suggest that there are $24 - (1 + 8 + 8) = 7$ df for H_2 , but actually there are 9 df, and really only 15 (not 17) nonredundant model parameters. Certain dependencies exist among the model parameters other than those noted above, and unless two restrictions are imposed the parameter estimates will be unidentifiable. The unrestricted model for the 5×5 table has 9 (not 7) df, and although the unrestricted two-class model can be tested by comparing either X^2 or L^2 with the tabulated percentiles of the χ^2 distribution on 9 df, empirical deductions from the model such as those presented in the formulae of Section I cannot be made unless restrictions are imposed to identify the parameter estimates.

The identification problem is depicted in the two sets of parameter estimates appearing in table 3. Each set of estimates produces an identical value of L^2 , but the estimates can be seen to differ somewhat from each other. The set of estimates in part A corresponds to a solution obtained by using one set of start values in the computer program described by Clogg (1977), which employs the algorithm presented by Goodman (1974b).³

TABLE 3
DIFFERENT PARAMETER ESTIMATES FOR THE TWO-CLASS
MODEL APPLIED TO THE DANISH DATA AND
YIELDING IDENTICAL VALUES OF L^2

	A		B	
	LATENT CLASS t		LATENT CLASS t	
	$t = 1$	$t = 2$	$t = 1$	$t = 2$
$\hat{\pi}_{it}^{\bar{O}X}$:				
$i = 1$0619	.0016	.0620	.0000*
23080	.0305	.3083	.0234
34616	.1992	.4619	.1924
41681	.4175	.1678	.4239
50004	.3512	.0000*	.3603
$\hat{\pi}_{jt}^{\bar{D}X}$:				
$j = 1$0785	.0065	.0755	.0065
22753	.0132	.2645	.0134
34876	.1508	.4738	.1510
41545	.4593	.1670	.4591
50042	.3702	.0192	.3700
$\hat{\pi}_t^X$3693	.6307	.3847	.6153
	$L^2 = 116.232$		$L^2 = 116.232$	

* Denotes a restriction; see text for details.

³ This computer program is also briefly described in Clogg (1979a). It is available on request from the author.

Different sets of start values will lead to different parameter estimates, but the underidentification of parameters implies that there can be no rational means of choosing among different sets of estimates so obtained. The set of parameter estimates in part *B* was obtained by imposing two restrictions, $\hat{\pi}_{61}^{\bar{O}X} = \hat{\pi}_{12}^{\bar{O}X} = 0$, and with these two restrictions the parameter estimates are identifiable. An additional restriction can be enforced in a new model, but such a restriction will lead to an increase in the value of L^2 , and so only two restrictions are necessary for a "just-identified" solution.

In a situation where parameters are unidentifiable, strictly a priori considerations must be used to construct plausible restrictions which render the estimates interpretable. A rationale for circumventing the identification dilemma is presented here; it exploits the fact that status 1 is the highest and status 5 the lowest status in the table.⁴ Consider the effect of restricting $\hat{\pi}_{61}^{\bar{O}X} = 0$, which is equivalent to assuming a priori that $\pi_{61}^{\bar{O}X} = 0$. This restriction says that the first latent class (where $t = 1$) cannot contain members from the fifth (the lowest) origin status category. Such a restriction serves in part to characterize the first latent class; since persons in that class cannot have been drawn from the lowest origin status, it can be said that class 1 of X corresponds to an "upper" class. Consider next the effect of restricting $\hat{\pi}_{12}^{\bar{O}X} = 0$, equivalent to assuming a priori that $\pi_{12}^{\bar{O}X} = 0$. This says that the second latent class cannot contain members from the first (the highest) origin status, and so this type of restriction can aid in characterizing the second latent class. Persons in that class can have been drawn only from the four lower origin statuses, and this leads in part to designation of the second latent class as the "lower" class. The model using both of these restrictions produces the set of identified parameter estimates in part *B* of table 3, and inspection of those quantities indicates the validity of characterizing the classes as upper versus lower. The conditional probabilities $\hat{\pi}_{it}^{\bar{O}D}$ are such that $\hat{\pi}_{i1}^{\bar{O}X} > \hat{\pi}_{i2}^{\bar{O}X}$ for $i = 1, 2, 3$ and $\hat{\pi}_{i1}^{\bar{O}X} < \hat{\pi}_{i2}^{\bar{O}X}$ for $i = 4, 5$, and an identical ordering is evidenced for the conditional probabilities $\hat{\pi}_{jt}^{\bar{O}X}$. Since the two restrictions imposed in part *B* of table 3 could have been based on an a priori conception of how the classes of X relate to the origin status of the individual, and since these restrictions also serve to identify the parameters, we are led to these estimates as a plausible description of the structure of mobility as rendered by the two-class model. It can be noted that 38% of the persons are estimated to be in the upper class for the Danish data, with 62% in the lower class. The relationship of the class variable to both the origin and the destination status variables can be studied by inspecting the corresponding sets of conditional probabilities reported in table 3.

Although the two-class model H_2 has not fitted the data to an acceptable

⁴ If we were not willing to assume that the highest and the lowest status in the table were known a priori, different sets of restrictions would have to be used.

degree as judged by various criteria of fit, to simplify later discussion I now go on to describe briefly some of the deductions that can be made from it. Again the results of H_2 for the Danish data will serve for purposes of exposition. The set of identified parameter estimates provided in part B of table 3 will be used throughout to derive results.

Equations (4)–(8) showed how latent class membership could be predicted from knowledge of a person's mobility pattern and also how the quality of that prediction could be assessed. Table 4 summarizes results for the Danish table. The symmetry of the assignment is noteworthy, since the table of predictions is almost in a triangular form. Persons with origin status 1 are assigned to latent class 1 (the upper class) and those with origin status 5 are assigned to latent class 2 (the lower class), and there are zero error rates for these predictions as a direct consequence of the two zero restrictions used to identify the parameter estimates. Those who move from

Table 4. Assignment of Mobile and Nonmobile Persons into Latent Classes (Danish 5x5 Table, 2-Class Model with Identifiable Parameter Estimates)

D=		1	2	3	4	5
O =	1	1 .00	1 .00	1 .00	1 .00	1 .00
	2	1 .01	1 .01	1 .04	1 .25	2 .30
	3	1 .06	1 .03	1 .16	2 .35	2 .07
	4	1 .26	1 .17	2 .44	2 .08	2 .01
	5	2 .00	2 .00	2 .00	2 .00	2 .00

% correctly allocated into latent classes = 88.2%

Lambda = .693

Note.— The first entry in each cell is the class assignment under the 2-class model with $\pi_{51}^{\bar{O}X} = \pi_{12}^{\bar{O}X} = 0$. Class 1 is "upper" ; class 2 is "lower." The second entry is the probability that a person with the given mobility pattern is drawn from a latent class different from the one assigned under the model, i.e., the E_{ij} of (5).

status 2 to 1, 2, 3, or 4; from status 3 to 1, 2, or 3; or from status 4 to 1 or 2 are all predicted to be members of the upper class. All others are predicted to derive from the lower class. The indexes of the quality of this prediction indicate that a reasonably good prediction is offered by the model (e.g., 88.2% of the persons are correctly allocated into the latent classes under the model).

Also presented in table 4 are the error rates associated with the assignments for all 25 mobility patterns. It can be noted that these error rates behave as could be expected if the model were substantively plausible, with low error rates in the extreme upper left and lower right parts of the table and higher error rates in the center of the table where latent class membership is more difficult to determine. Within the mobility patterns which are predicted to define the upper class, there is a nondecreasing trend in the error rate associated with the prediction as one considers mobility patterns downward from or to the right of the (1, 1) cell, with a similar comment pertaining to mobility patterns above or to the left of the (5, 5) cell.

In equations (9)–(10), the mobility rates r_{ij} were decomposed into a sum representing the Markov character of the latent class model. A display like that in figure 1 can summarize the results effectively, with the transition rates entered on the arrows in a kind of flow graph or path diagram. A further consequence of the identifying restrictions used for the two-class model can be seen in figure 1; the transitions from origin statuses 1 and 5

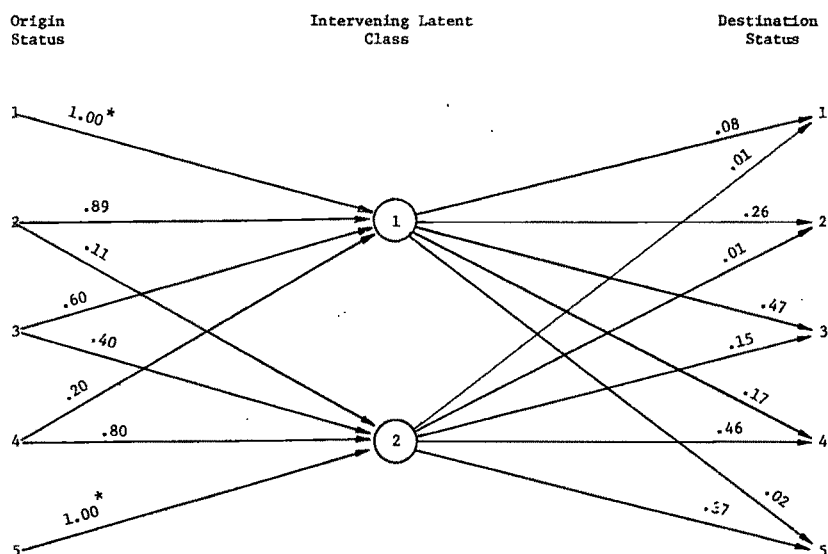


FIG. 1.—Transition rates for the mobility table, with the latent classes viewed as intervening states: two-class model H_2 for Danish table (identified parameter estimates). Asterisks denote quantities that are restricted in order to identify the model.

are deterministic in character, since a person in origin status 1 moves to latent class 1 with certainty, and a person in origin status 5 moves to latent class 2 with certainty.

The two-class model H_2 was also applied to the British data. For the British 5×5 table, it has been noted that there is a zero count in the (5, 1) cell, and in the British 8×8 table it has been noted that there are zero counts in both the (7, 1) and (8, 1) cells. Upon applying to the British 5×5 table the same zero restrictions used to identify the parameters of H_2 for the Danish table (i.e., $\pi_{51}\bar{\theta}^x = \pi_{12}\bar{\theta}^x = 0$), it was found that a terminal solution for this restricted model was obtained, where $\hat{\pi}_{12}\bar{\theta}^x = 0$. This result, when coupled with the two a priori zero restrictions, produced a zero expected count in the (5, 1) cell. This implies that a modification of the model's df would be required. To make H_2 for the British data comparable to H_2 for the Danish data, an expedient was adopted to circumvent this difficulty. The observed count of zero for the (5, 1) cell was replaced by .1, and the two-class model was then reestimated. While this strategy is admittedly ad hoc in character, there can be little question that the modification just described can have little effect on the interpretation of results.⁵ Model H_2 applied to this modified British 5×5 table produces $L^2(H_2) = 182.3$, $\Delta = 8.0\%$, $R^2 = .78$. When sample size is taken into account, the fit of H_2 is thus comparable for both the British and the Danish 5×5 tables. The estimated proportion in the upper class is .246 (compared with .385 for the Danish table). Later analysis will show that this apparent difference in the size of the upper class is due almost entirely to status inheritance unaccounted for by the two-class model H_2 .

Upon applying the two-class model to the British 8×8 table, the same kind of difficulty was encountered as when it was applied to the 5×5 table. A value of .1 was added to both of the cell zeroes; a proper nonterminal solution to the likelihood function was then obtained, yielding positive expected frequencies for all cells. Model H_2 produces an L^2 of 253.5, $\Delta = 9.3\%$, $R^2 = .73$, for the 8×8 table, results that are nearly comparable to the results for both the British 5×5 table and the Danish 5×5 table. Under H_2 for the British 8×8 table, the estimated proportion in the upper class was .228, compared with .246 for this model applied to the 5×5 British table.

Three-Class Models

The three-class latent structure is designated as H_3 ; it corresponds to an assumption that an upper, a middle, and a lower class are indirectly observed in the mobility table. Since the two-class model H_2 did not fit the

⁵ This modification changes the χ^2 statistics for the independence model (H_1), e.g., by a very small amount (less than two).

data, H_3 is one natural modification of that model to consider. However, there are difficulties in the application of the three-class model to mobility tables as well, and these will now be discussed, first with reference to the Danish 5×5 table. As noted previously, there are 24 nonredundant π_{ij} , and equation (3) thus defines a transformation from the model parameters to these 24 π_{ij} . For the three-class model, there are apparently two nonredundant π_i^X , 12 nonredundant $\pi_{it}^{\bar{O}X}$, and 12 nonredundant $\pi_{jt}^{\bar{D}X}$, producing an apparent total of 26 parameters. Since the number of parameters is two greater than the number of nonredundant π_{ij} , the parameters of the three-class model are clearly underidentified unless some restrictions are imposed. It is easy to see that at least two restrictions are required, and when two restrictions of the kind used in connection with H_2 are imposed (see table 3) the fit of the model is equivalent to that obtained for the completely unrestricted model, $L^2(H_3) = 32.7$, $\Delta = 5.0\%$. However, it can be shown that, even with two restrictions, the parameter estimates are still unidentifiable, and it can be further shown that two additional restrictions are required. In other words, the three-class model has 2 df when applied to the 5×5 mobility table, and this demonstrates why the fit of the model is not perfect. The three-class model does improve the fit, but it is clearly not parsimonious. Moreover, it still does not fit the data to an acceptable degree as judged by any of the indexes of fit. Because of these facts, we are led to consider various kinds of restricted latent structures, some of which are surveyed in the next section.

Results from H_3 applied to the other tables also appear in table 2. While a moderate improvement in fit was obtained with each set of mobility data, there is clearly room for improvement. One must conclude that general latent structures of the kind which would initially be considered in an analysis of mobility have failed to fit the data to an acceptable degree. Inspection of standardized residuals for each of the models considered here shows that "clustering" along the main diagonal is the main reason for the failure of these models. The tendency for at least some individuals to stay in their origin status is thus not adequately taken into account by either the perfect mobility model H_1 or the two- and three-class models H_2 and H_3 . (See Singer and Spilerman [1978] for a recent discussion of this problem and for references cited there.)

III. SOME RESTRICTED LATENT STRUCTURES

All the models of the preceding section were unrestricted in the sense that no a priori restrictions were applied in order to specify the model and none were necessary to test the model or gauge its fit. Certain restrictions were imposed in order to identify parameter estimates (see, e.g., table 3), but this is actually a separate issue from that of testing the model in question.

This section applies some restricted latent structures to the data with the objectives of both testing the models and arriving at the substantive implications of them for the structure of mobility. All of these models can be viewed as generalizations of either or both of the latent structures H_1 and H_2 . We will find that it is not necessary to consider generalizations of H_3 , the three-class latent structure.

The quasi-perfect mobility model considered by Goodman (1969, 1972) is a restricted latent structure, as indicated by the discussion surrounding (12)–(13). This model is an $(I + 1)$ -class restricted latent structure, with I -deterministic status classes, or categories of “latent stayers,” and an additional single class of latent movers. Within the latent mover class, mobility is posited to be perfectly random, depending only on the distribution of the origin and destination statuses within this class. For each of the I statuses, a group is posited to exist which stays in the status with certainty, and it is this feature of the model which leads us to characterize the I groups of latent stayers as constituting distinct status classes. Mobility across statuses is prohibited for members of each status class, giving the justification for the term “class,” and each of these groups consists of members of only one status, giving the reason why this is referred to as a status class. This model is designated as M_1 in table 5, and we see clearly that it does not fit any of the sets of data. (The Δ 's, e.g., range from 6.6% to 10.1%.) It can be noted that this model is roughly comparable in terms of its fit with the two-class model H_2 considered in the previous section; M_1 fits less well but is slightly more parsimonious. A variety of modifications of M_1 have been considered in the literature (e.g., Goodman 1969, 1972, 1979; Haberman 1974; Pullum 1975; Hauser 1978; Duncan 1979), but, to my knowledge, none of the various generalizations have considered the type of extension to which we now turn.

A Two-Class Quasi-latent Structure: Indexes of Fit

The quasi-perfect mobility model is a direct generalization of the perfect mobility model H_1 , a model that has been interpreted in this paper as a one-class latent structure. Models M_2 – M_6 and M'_4 , M'_5 can be viewed as generalizations of the two-class latent structure H_2 which contain features of M_1 as well. Let us consider a two-class latent structure in which two classes of latent movers (namely, an upper and a lower class) are posited to exist, in an assumption similar to that made with the two-class model H_2 . In addition, let us posit the existence of K status classes, or latent stayers, corresponding to K ($\leq I$) of the statuses in the table. Such a model modifies H_2 in a way analogous to the way in which M_1 (the quasi-perfect mobility model) modifies H_1 . From a purely statistical point of view, these models are constructed in order to account for the clustering on the main diagonal,

and this is done by introducing the concept of deterministic status classes of individuals.

For the 5×5 tables, the same kind of identification problem arises with these models as was encountered with H_2 . Accordingly, the restrictions used above to identify parameter estimates will be employed here. That is, the restrictions $\pi_{61}\bar{0}^X = \pi_{12}\bar{0}^X = 0$, where class 1 of X refers to the upper class and class 2 of X refers to the lower class, are imposed a priori. The zero count in the (5, 1) cell of the British table also has the same consequence for applying the methods as was encountered earlier, and so this count was replaced by .1 in order to apply the models directly.

Models M_2 – M_6 introduce deterministic status classes into the two-class models for the 5×5 table, and it is evident that these greatly improve the fit. Model M_2 introduces two status classes, one for each of the statuses 1 and 5; there are accordingly two fewer degrees of freedom for M_2 relative to H_2 . Model M_3 introduces a third status class corresponding to latent stayers in status 3 and produces a most compelling fit indeed for both sets of data. Note that $\Delta = 1.7\%$ for M_3 applied to each set of data, and $R^2 = .99, .98$, $F = 1.4, 2.2$, respectively, for the Danish and British tables. If

TABLE 5
SOME RESTRICTED LATENT STRUCTURES (Quasi-Latent
Structures) APPLIED TO THREE MOBILITY TABLES:
INDEXES OF FIT

	df	L^2	X^2	Δ (%)	$F = L^2/\text{df}$	R^2
Danish 5×5 Table						
H_1	16	654.2	754.1	19.4	40.9	...
M_1	11	248.7	270.3	9.5	22.6	.62
M_2	7	43.2	43.0	4.5	6.2	.93
M_3	6	8.2	8.1	1.7	1.4	.99
M_4	5	8.2	8.1	1.7	1.6	.99
M_5	4	8.2	8.1	1.7	1.6	.99
M_6	7	27.9	33.4	2.6	4.0	.96
British 5×5 Table						
H_1	16	811.0	1,199.4	15.4	50.7	...
M_1	11	249.5	327.3	6.6	22.7	.69
M_2	7	33.2	33.8	2.5	4.7	.96
M_3	6	12.9	12.5	1.7	2.2	.98
M_4	5	12.9	12.5	1.7	2.6	.98
M_5	4	12.9	12.5	1.7	3.2	.98
M_6	7	71.4	76.2	3.7	10.2	.91
British 8×8 Table						
H_1	49	954.1	1,415.3	16.6	19.5	...
M_1	41	446.8	555.1	10.1	10.9	.53
M'_4	29	60.3	61.1	4.1	2.1	.94
M'_5	28	60.3	61.1	4.1	2.2	.94

the χ^2 statistics were taken as pertaining to simple random samples (i.e., multinomial samples) or to stratified random samples with the destination statuses as the stratifying criterion (i.e., product-multinomial samples), the descriptive level of significance for M_3 would be greater than .20 for the Danish table and only slightly less than .05 for the British table.

Models M_4 and M_5 posit four and five status classes, respectively, and contribute nothing to fit. The estimated proportions of latent stayers in statuses 2 and 4 turn out to be nearly zero for each set of data, given that latent stayers are admitted for statuses 1, 3, and 5. Model M_6 considers the possibility that latent stayers in status 1 can be deleted from model M_3 , but for each set of data it is clear that this cannot be considered tenable. Model M_3 thus appears as a plausible model for both 5×5 tables, and there appear to be no serious competitors involving either more or fewer status classes than the three considered with M_3 .

We next consider similar models for the British 8×8 table, namely, models M'_4 and M'_5 . Model M'_4 modifies the two-class model by positing the existence of seven status classes (a status class corresponding to status 3 was omitted). Model M'_5 is the model with all eight status classes considered, and we see that this does not improve the fit to any discernible extent. Model M'_4 is thus the quasi-latent structure suggested for the 8×8 table. It fits the data less well than M_3 applied to the 5×5 tables and would still be judged inadequate if the χ^2 statistics were taken at face value. However, M'_4 accounts for 94% of the variability in the table, has a Δ of 4.1% and an F of only 2.1, implying that the model performs very well indeed when compared with any of the other models considered in this paper. It can be noted that Duncan (1979) obtained a somewhat better fit for the 8×8 table with his row-effects and his uniform-association model, with the diagonal "deleted" ($L^2 = 45.9, 58.4$ on 34 and 40 df, respectively), but his model is developed from an entirely different point of view than the models considered here (also see Goodman 1979). Duncan examined the dependence of destination status on origin status and formulated coefficients somewhat analogous to the simple regression coefficient β_{DO} in the fashion of status attainment research (cf. Blau and Duncan 1967). I have focused analysis directly on the class structure of mobility processes, taking the variable X as the intervening variable which explains the dependence of destination on origin. While the fit of M'_4 can certainly be improved, inspection of the R^2 or Δ indexes indicate that only a little improvement is necessary.

Parameter Estimates from the Quasi-latent Structure

The parameter estimates associated with M_3 for the two 5×5 tables are presented in table 6. The estimated proportions in the latent upper class

are .251 for the Danish data, .220 for the British data. After status inheritance is introduced by considering three status classes, the proportion in the upper class in the two societies is virtually identical (differing only by .03). Similarly, the estimated proportions in the lower class are .624 for Denmark and .691 for Britain, implying that the British data exhibit a slightly heavier concentration in the lower class. What is striking about the differences between the two sets of data is the proportions in the three status classes. The Danish data have one-half the proportion of latent stayers in status 1 (.006/.012), slightly over four times the proportion of

TABLE 6
PARAMETER ESTIMATES OBTAINED WITH THE
QUASI-LATENT STRUCTURE MODEL M_3
A. LATENT CLASS PROPORTIONS, $\hat{\pi}_i^{X*}$

	Danish 5×5	British 5×5
$\hat{\pi}_1^X$251	.220
$\hat{\pi}_2^X$624	.691
$\hat{\pi}_3^X$006	.012
$\hat{\pi}_4^X$059	.014
$\hat{\pi}_5^X$059	.062

B. CONDITIONAL PROBABILITY OF ORIGIN STATUS i ,
GIVEN LATENT CLASS l ($\hat{\pi}_{il}^{\bar{D}X}$)

i	DANISH 5×5		BRITISH 5×5	
	$l = 1$	$l = 2$	$l = 1$	$l = 2$
1.....	.071	.000†	.111	.000†
2.....	.450	.032	.509	.043
3.....	.342	.242	.178	.137
4.....	.137	.466	.202	.560
5.....	.000†	.260	.000†	.260

C. CONDITIONAL PROBABILITY OF DESTINATION
STATUS j , GIVEN LATENT CLASS l ($\hat{\pi}_{jl}^{\bar{D}X}$)

j	DANISH 5×5		BRITISH 5×5	
	$l = 1$	$l = 2$	$l = 1$	$l = 2$
1.....	.084	.010	.077	.000
2.....	.383	.022	.438	.063
3.....	.375	.195	.165	.117
4.....	.125	.505	.254	.510
5.....	.032	.269	.067	.310

* The meaning of the latent classes is as follows: classes 1 and 2 refer to latent upper and latent lower classes, respectively; classes 3, 4, and 5 refer to latent status classes corresponding to latent stayers in observed statuses 1, 3, and 5, respectively.

† Denotes a restriction.

latent stayers in status 3 (.059/.014), and roughly equal numbers who are latent stayers in the fifth status. These results correspond to 25%, 20%, and 27% of those in origin statuses 1, 3, and 5 being latent stayers in Denmark, and 32%, 10%, and 26% in Britain. In both absolute and relative terms, the differences observed between the two sets of data with respect to the latent class distribution arise because of differing tendencies for status inheritance in the first and third statuses.

The conditional probabilities $\hat{\pi}_{it}^{\bar{O}X}$, $\hat{\pi}_{jt}^{\bar{O}X}$ corresponding to M_3 are also presented in table 6 and allow comparison of the status distributions within classes between the two sets of data. A salient difference is that the third status is much more likely to depict membership in the upper class for Denmark (since $\hat{\pi}_{31}^{\bar{O}X} = .342$ for Denmark, as compared with .178 for Britain). The reader can note that $\hat{\pi}_{i1}^{\bar{O}X} > \hat{\pi}_{i2}^{\bar{O}X}$ for $i = 1, 2, 3$, whereas $\hat{\pi}_{i1}^{\bar{O}X} < \hat{\pi}_{i2}^{\bar{O}X}$ for $i = 4, 5$, with a similar ordering for the $\hat{\pi}_{jt}^{\bar{O}X}$, for each set of estimates. Latent class 1, the upper class, is therefore essentially a mixture of statuses 1, 2, 3, and latent class 2, the lower class, is essentially a mixture of statuses 4 and 5 for each society.

Tables 7 and 8 present the parameter estimates for model M'_4 for the British 8×8 table. Alongside these are the corresponding estimates for model M_3 applied to the 5×5 table. One question to be raised is how the descriptions offered by the models change when a less coarse summary of the mobility data is used instead of a coarse summary. In table 7 the latent class proportions are compared. The proportion in the latent upper class is estimated at .236 in the 8×8 table, compared with .220 in the 5×5 table; the proportion in the latent lower class is estimated at .680 in the

TABLE 7
ESTIMATED LATENT CLASS PROPORTIONS OBTAINED
WITH THE QUASI-LATENT STRUCTURE
MODEL M'_4 , BRITISH 8×8 TABLE

	LATENT CLASS PROPORTIONS, $\hat{\pi}_t^X$	
	Model M'_4 for 8×8 Table	Model M_3 for 5×5 Table
Latent upper.....	.236	.220
Latent lower.....	.680	.691
Latent stayers in status:		
1.....	.013	.012
2.....	.006	{ ...
3.....	...	{ ...
4.....	.013	.014
5.....	.004	{ ...
6.....	.014	{ ...
7.....	.017	.062
8.....	.016	{ ...

8×8 table, compared with .691 in the 5×5 table. These differences are relatively slight. The estimated proportions in the status classes differ somewhat for the two tables. Introducing the finer detail changes the inference, for example, for each of the status classes associated with observed statuses 5-8. In the 8×8 table, .018 of the persons (compared with .000 for the 5×5 table) are estimated to stay in statuses 5 or 6, and .033 (compared with .062) are estimated to stay in statuses 7 or 8. In spite of these differences, I do not believe that these results should be used in an ad hoc way to determine whether the 5×5 or the 8×8 table should be the focus of the analysis. The question which table should be used is largely a non-statistical question and should be resolved by the researcher when he decides which forms of mobility or immobility are important to consider. Table 8 presents the estimates $\hat{\pi}_{ii}^{\bar{D}X}$, $\hat{\pi}_{ji}^{\bar{D}X}$ for the 8×8 table and compares them with the corresponding quantities obtained from model M_3 applied to the 5×5 table. Some differences in interpretation are again observed, but on balance I believe the reader will find that both sets of estimates

TABLE 8
ESTIMATED CONDITIONAL PROBABILITIES OBTAINED WITH THE QUASI-LATENT STRUCTURE MODEL M'_4 , BRITISH 8×8 TABLE, COMPARED WITH ESTIMATES FROM MODEL M_3 FOR BRITISH 5×5 TABLE

CONDITIONAL PROBABILITY OF ORIGIN STATUS i , GIVEN LATENT CLASS i , $\hat{\pi}_{ii}^{\bar{D}X}$				
i	$i = 1$		$i = 2$	
	M'_4	M_3	M'_4	M_3
1.....	.103	.111	.000*	.000*
2.....	.152	.509	.001	.043
3.....	.298042	...
4.....	.196	.178	.130	.137
5.....	.047	.202	.043	.560
6.....	.202479	...
7.....	.003	.000*	.166	.260
8.....	.000*139	...

CONDITIONAL PROBABILITY OF DESTINATION STATUS j , GIVEN LATENT CLASS i , $\hat{\pi}_{ji}^{\bar{D}X}$				
j	M'_4	M_3	M'_4	M_3
1.....	.072	.077	.000	.000
2.....	.140	.438	.009	.063
3.....	.266047	...
4.....	.178	.165	.112	.117
5.....	.109	.254	.058	.510
6.....	.172419	...
7.....	.045	.067	.208	.310
8.....	.019148	...

* Denotes a restriction used to identify model parameters.

produce nearly the same characterization of the latent class variable and the relationship of it to the status variables O and D .

In sum, the two-class quasi-latent structure has provided compelling summaries of all three sets of mobility data, and the parameter estimates lend themselves easily to substantive interpretation. The logical relationships among these models can be depicted as in figure 2. The perfect mobility model, which states that there is only one class in the table within which there are random mobility chances, can be generalized into both the quasi-perfect mobility model M_1 , by adding concepts of status classes, and the fundamentally different model H_2 , a two-class latent structure. The models considered by Goodman (1972), Haberman (1974), Pullum (1975), Duncan (1979), and Hauser (1978), for example, can all be viewed as generalizations of H_1 and/or M_1 . However, the quasi-latent structures M_2 - M_6 (as well as M'_4 , M'_6) are all direct generalizations of H_2 , where two classes of latent movers are posited to exist. The results presented here certainly suggest that models combining the features of H_2 and M_1 should be given serious consideration.

Indexes of Immobility

A measure of immobility was devised by Goodman (1969, 1972) to measure the surplus of observed stayers in various statuses of the mobility table under the quasi-perfect mobility model. Attempts to measure immobility and/or status persistence go back at least as far as Rogoff (1953), and the methodological difficulties associated with such attempts are surveyed by Blau and Duncan (1967), Tyree (1973), Boudon (1973), and Hauser (1978),

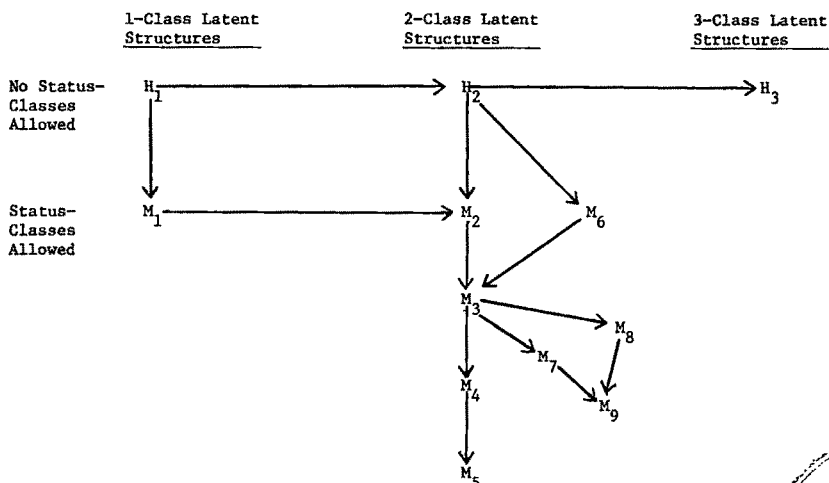


FIG. 2.—The logical implications of the various models

as well as by Goodman (1969). One difficulty in the earlier literature was that comparisons of the observed numbers staying in a status over time were implicitly made with the expected numbers staying in that status given the perfect mobility model, a model which on the whole did not fit the data to an acceptable degree wherever it was applied. Goodman proposed that immobility be measured with the quasi-perfect mobility model M_1 , where explicit parameters measuring immobility were incorporated as a feature of the model (see eqq. [12]–[13] herein). Model M_1 does not fit these data, but the quasi-latent structure did fit the data to an acceptable degree, and so it seems altogether appropriate to base measures of immobility upon it. The indexes of immobility γ_i^* presented in (16)–(17) can be used to measure the surplus of stayers in status i , relative to that predicted to stay in status i given the operation of random mobility chances within both the latent upper and the latent lower class of “movers.” In table 9, Goodman’s indexes of immobility $\hat{\gamma}_i$ obtained from M_1 are compared with the indexes of immobility $\hat{\gamma}_i^*$ obtained from M_3 for the 5×5 tables and M'_4 for the 8×8 table.

In virtually every case, the amount of immobility in the statuses of the mobility tables is greatly reduced when the γ_i^* of (16) are used as measures. The comparisons are dramatic in some instances; for example, Goodman’s index $\hat{\gamma}_1 = 11.26$ and the revised index $\hat{\gamma}_1^* = 5.05$ for the Danish table. That is, under the quasi-perfect mobility model the observed number of stayers in status 1 is 11.26 times greater than the expected immobility in this status predicted from the marginals for the single latent mover class. On the other hand, the revised index says that the observed number of stayers in status 1 is 5.05 times greater than would be expected from the prediction obtained from the marginals for both of two latent classes of movers. With the two-class quasi-latent structure, an accounting must be

TABLE 9
A COMPARISON OF GOODMAN’S INDEXES OF IMMOBILITY WITH THE
REVISED INDEXES OF IMMOBILITY

STATUSES IN 5 × 5 TABLE	DANISH 5 × 5*		BRITISH 5 × 5		STATUSES IN 8 × 8 TABLE	BRITISH 8 × 8	
	M_1	M_3	M_1	M_3		M_1	M'_4
1.....	11.26	5.05	29.62	7.62	1.....	30.38	8.22
2.....	4.03	1.00	3.58	1.00	2.....	7.60	2.28
3.....	1.66	2.36	1.59	2.11	3.....	2.01	1.60
4.....	1.18	1.00	1.00	1.00	4.....	1.62	1.74
5.....	2.95	1.96	2.59	1.79	5.....	2.17	2.44
					6.....	1.26	1.10
					7.....	2.07	1.74
					8.....	2.62	2.18

* Model M_1 is the quasi-perfect mobility model; models M_3 , M'_4 are two-class quasi-latent structures.

made for the observed immobility in fewer statuses (1, 3, 5 in the 5×5 tables; 1, 2, 4-8 in the 8×8 table) than with the quasi-perfect mobility model, and the amount by which observed immobility exceeds that predicted by considering two classes of movers is usually reduced. The exceptions are for the immobility in status 3 for the 5×5 tables, where the index $\hat{\gamma}_3^*$ is 2.36 for the Danish table and 2.11 for the British 5×5 table, compared with Goodman's index $\hat{\gamma}_3$ of 1.66 and 1.59, respectively. The revised index also shows slightly more immobility for statuses 4 and 5 of the 8×8 table than does Goodman's index, but on balance the indexes of immobility based on the two-class, quasi-latent structure produce an impression that considerably more immobility is accounted for by the model that allows for two classes of latent movers. I view this as an advantage of the quasi-latent structures considered herein: while concepts of "status persistence," latent stayers, or status classes still appear to be required with the latent structure approach, these concepts are necessary to describe the immobility of relatively fewer persons. It may be noted in passing that the index of immobility $\hat{\gamma}_1^*$, depicting the excess immobility in status 1, is greater for Britain than for Denmark by a nontrivial amount, but the immobility in the other statuses is virtually identical.

The Prediction of Membership in Latent Classes and Latent Status Classes

When the two-class quasi-latent structure is used to describe the data, the model parameters can be manipulated to provide the prediction of latent class membership for any mobility pattern in the table. This technique was discussed in equations (4)-(8) and was illustrated with the two-class model H_2 for the Danish data in table 4. I shall now describe the latent class predictions for all three sets of data based on models that fit. The only difference between these predictions and those presented earlier is that now status classes must be taken into account.

Table 10 summarizes the predictions for the two 5×5 tables. The most salient feature of this comparison is the striking degree of similarity in results, as can be seen in several ways. First, the predictions are identical for each table, with the single exception of the prediction for the (4, 2) mobility pattern. With the Danish data (table 10A) the (4, 2) pattern of mobility is sufficient to produce a prediction of membership in the upper class (with an error rate of .33), while for the British data (table 10B) this same pattern of mobility produces a prediction of lower-class membership (with an error rate of .45). Interpreted in another way this says that, under the model, 67% of the persons with this pattern of mobility would be predicted to be members of the upper class in Denmark, while only 45% would be predicted to be in the upper class in Britain. I remarked earlier that Britain appears to have a slightly heavier concentration in the lower

class, and this characteristic shows up in the prediction for this particular mobility pattern. As a whole, the quality of the prediction is comparable for each table: 82.9% and 83.7% of the persons would be correctly allocated to the latent classes and latent status classes for Denmark and Britain, respectively, and the λ measures of the quality of the prediction are also comparable. (From [8] it is clear that λ takes into account the distribution of the latent classes, i.e., the relative magnitudes of the $\hat{\pi}_i^X$, whereas the "percent correctly allocated" is based only on the quantity E_2 of eq. [6], and this accounts for why the two indexes give slightly different interpretations of the quality of the predictions.) The upper- versus lower-class division in both tables is comparable, with the division occurring approximately on the diagonal of the table that runs from the lower left to the upper right.

Table 10 also provides us with the error rates associated with the predictions of membership in the three status classes associated with M_3 . For example, the error rate of .20 for the (1, 1) cell for the Danish table signifies that 80% of the observed stayers in status 1 can be regarded as latent stayers or members of a status class associated with the first observed status. A similar argument shows that 87% of the stayers in status 1 for the British table can be regarded as latent stayers. As a direct consequence of the model, it is easy to see that all other members of the (1, 1) mobility pattern besides those in the associated status class are members of the latent upper class, and this is a consequence of the fact that the restriction $\pi_{12}^{\bar{0}X} = 0$ was applied. Since this holds, it appears appropriate further to characterize the status class associated with the first origin status as a status class embedded within the upper class. To corroborate this interpretation, merely note that all mobility patterns adjacent to the (1, 1) cell produce predictions of membership in the upper class, and note in addition that membership in the first origin status denotes either membership in the status class just discussed or in the upper class. Because of these considerations, it seems justified to speak of the first latent status class as a special category within the upper class, an interpretation that certainly accords well with intuition. Similar comments apply directly to the characterization of the status class corresponding to latent stayers in the fifth status. We are led to characterize this status class (denoted as class 5 in table 10) as a status class within the lower class, and this designation is a consequence of the fact that all members of origin status 5 are predicted to be in either the lower class or the latent status class for this status, and/or the fact that all mobility patterns adjacent to the (5, 5) cell are predicted to be in the lower class. A similar interpretation cannot be made for the status class corresponding to the third observed status, since (a) those not in the relevant status class (denoted as class 4 in table 10) might be in either the upper or the lower class, and (b) the mobility patterns adjacent to the (3, 3) cell produce predictions

of both upper- and lower-class membership, depending on the direction in which one moves from this cell. The status class corresponding to latent stayers in observed status 3 is thus intermediate between the upper and lower classes indirectly observed in the table. In sum, there is an appealing simplicity in the predictions of table 10, which may be summarized in the following way: (1) two latent classes exist, an upper and a lower class; (2)

Table 10. Assignment of Mobile and Nonmobile Persons into Latent Classes, Based on Model M_3 for Danish and British 5x5 Tables

		Destination Status				
		1	2	3	4	5
Origin Status	A.	1	2	3	4	5
	1	3*	1	1	1	1
		.20	.00	.00	.00	.00
	2	1	1	1	1	2
		.02	.01	.18	.42	.40
	3	1	1	4*	2	2
		.17	.19	.51	.12	.06
	4	1	1	2	2	2
		.49	.33	.18	.13	.01
	5	2	2	2	2	5*
		.00	.00	.00	.00	.42

% correctly allocated into latent classes = 82.9
Lambda = .545

		Destination Status				
		1	2	3	4	5
Origin Status	B.	1	2	3	4	5
	1	3*	1	1	1	1
		.13	.00	.00	.00	.00
	2	1	1	1	1	2
		.00	.04	.16	.35	.45
	3	1	1	4*	2	2
		.01	.26	.56	.17	.08
	4	1	2	2	2	2
		.03	.45	.14	.15	.02
	5	2	2	2	2	5*
		.00	.00	.00	.00	.47

% correctly allocated into latent classes = 83.7%
Lambda = .472

*--Asterisked numbers denote a prediction of membership in a particular status-class.

three status classes exist; and (3) two status classes are embedded within the two latent classes of the model, and a third status class straddles the two latent classes.

For purposes of comparison, table 11 presents the latent class predictions for the British 8×8 table. In overall quality, the predictions are similar to those made with the corresponding 5×5 table, although some differences in interpretation do arise. Note, for example, that the (7, 1) and the (2, 7), (2, 8) patterns produce a prediction of upper-class membership with the model M'_4 applied to the 8×8 table, whereas earlier results based on the 5×5 table would have indicated that these would produce a prediction of lower-class membership. Again, the reader should be cautioned that inference about the 8×8 table is qualitatively different from inference about the 5×5 table, and the choice of which is to be preferred is largely a nonstatistical question. On balance, however, the two predictions are

Table 11. Assignment of Mobile and Nonmobile Persons into Latent Classes, Based on Model M'_4 for British 8×8 Table

		Destination Status							
		1	2	3	4	5	6	7	8
Origin Status	1	3*	1	1	1	1	1	1	1
	2	.12	.00	.00	.00	.00	.00	.00	.00
	3	1	4*	1	1	1	1	1	1
	4	.00	.44	.00	.01	.01	.05	.09	.14
	5	1	1	1	1	1	1	2	2
	6	.00	.03	.07	.20	.18	.50	.35	.24
	7	1	1	1	5*	2	2	2	2
	8	.00	.11	.25	.58	.49	.18	.10	.06
Origin Status	1	1	1	1	2	6*	2	2	2
	2	.01	.14	.32	.38	.41	.13	.08	.05
	3	1	1	2	2	2	2	2	2
	4	.01	.30	.45	.19	.21	.14	.03	.02
	5	1	2	2	2	2	2	2	2
	6	.25	.09	.04	.01	.01	.00	.43	.00
	7	2	2	2	2	2	2	2	2
	8	.00	.00	.00	.00	.00	.00	.00	.46

% correctly allocated into latent classes = 83.3%

Lambda = .477

--Denotes that a "status-class" is the prediction for members of this cell. Classes 3, 4*, 5*, and 6*, correspond to status-classes for the observed statuses 1, 2, 4, and 5, respectively.

very comparable, with only minor differences occurring along the negative diagonal of the table where latent class membership is difficult to determine.

Markov-Type Transition Rates Based on the Quasi-latent Structure

In equation (10), it was shown how the general latent class model for the π_{ij} could be translated into a model for the mobility rates r_{ij} , and this corresponded to a Markov-type assumption about the transition from status i to latent class t , and from latent class t to destination status j . Let R denote the matrix of mobility rates, let U denote the $I \times T$ matrix of transition probabilities π_{it}^{OX} (see [9]), and let V denote the $T \times J$ matrix of transition probabilities π_{jt}^{DX} . Then (10) implies that $R = UV$, and for a particular model (e.g., M_3), the estimated expected mobility rates can be written as $\hat{R} = \hat{U}\hat{V}$. The transition matrices \hat{U} and \hat{V} appear

Table 12. Transition Matrices for the Danish Mobility Table, Model M_3

		$\hat{\pi}_{it}^{OX}$				
		t=1	2	3	4	5
Origin Status	i=1	.75	.00	.25	.00	.00
	2	.85	.15	.00	.00	.00
	3	.29	.51	.00	.20	.00
	4	.11	.89	.00	.00	.00
	5	.00	.73	.00	.00	.27

		$\hat{\pi}_{jt}^{DX}$				
		j=1	2	3	4	5
Intervening t=1 Latent Class		.08	.38	.38	.13	.03
	2	.01	.02	.20	.50	.27
	3	1.00	.00	.00	.00	.00
	4	.00	.00	1.00	.00	.00
	5	.00	.00	.00	.00	1.00

Note--All zero entries result from restrictions on the quasi-latent structure.

in table 12 for the Danish data; to save space, the results for the British data are omitted. Many zeroes occur in these matrices as a result of the restrictions used to develop M_3 . These transition rates can be summarized by means of a flow graph similar in kind to figure 1. It is my conjecture that an eigenanalysis of the matrix R of the kind considered by McFarland (1978) could be approached from the point of view of this paper by noting that $R = UV$ and by taking account of the simplicity of structure of both the U and V matrices.

IV. CONCLUSION

The argument throughout this paper has been that the general latent structure model can be used to considerable advantage in the analysis of mobility tables. These methods are consistent with a variety of important conceptual paradigms that have been developed for the mathematical and statistical analysis of mobility. Similarities between the framework presented here and a regression framework are obvious: the latent structure method seeks to characterize the variable that intervenes between origin and destination in the mobility table, and this is somewhat analogous to the objectives of status attainment research generally. It was also pointed out how the latent structure framework leads to the development of a quasi-latent structure, a model similar to the influential quasi-perfect mobility model considered at length by Goodman. Finally, the Markov character of the latent structure model was discussed, demonstrating the conceptual similarity between the approach used here and certain approaches based on discrete-state stochastic processes. It was found that three classic sets of mobility data, which have been analyzed extensively over the past 20 years, could be adequately described by a quasi-latent structure. This model says that (a) two latent classes exist, an upper and a lower class, each of which is a mixture of the observed statuses in the table; (b) these latent classes derive their meaning as classes because within them mobility is "perfect" or random, structured only by the status distribution at origin and destination within each class; and (c) status classes corresponding to latent stayers in some of the statuses could also be posited to exist, and these can account for the excessive immobility typically found in some diagonal cells of the mobility table. It is my view that the latent structure framework allows a more direct integration of traditional sociological concepts of class into mobility analysis than any approach hitherto directed to the analysis of mobility tables. For practical purposes, the statistical characterization of the classes of the latent variable X might be taken as a summary of the (sociological) class variable that explains the observed dependence of destination on origin. These are certainly bold claims, but

I should be hard pressed to devise another framework depicting class structure as well as the approach advocated herein.⁶

However, certain technical issues must be resolved if the exploratory analysis reported on here is to be used in more theoretically enlightening "confirmatory" analyses, or in comparative mobility analyses with current data. Estimation of parameters in the latent structure model has long been a difficult problem (cf. Lazarsfeld and Henry 1968), but the methods described by Goodman (1974a) circumvent these difficulties. The computer program used in all of the calculations (Clogg 1977) is suggested as one approach to efficient computation, but different methods surveyed by Haberman (1979) or Clogg (1980b) could be used with equal justification. A more difficult problem arises from the fact that parameters are unidentifiable for many of the models considered here; all of the empirical deductions from the models depend on the proper resolution of the identification problem. To simplify the present exposition, very simple identifying restrictions were imposed, but no amount of ad hoc specification of the models can circumvent this problem in a satisfactory way. I believe that the zero restrictions on two conditional probabilities used herein, which effectively define the highest origin status as exclusively upper class in character and the lowest origin status as exclusively lower class in character, represent plausible strategies for researchers to follow.

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⁶ This point of view is also taken in Clogg (1979a, chap. 5; 1980a), although certain labor force statuses (not occupational statuses) were used there to construct the latent classes of interest.

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Detecting Sentencing Disparity: Some Problems and Evidence¹

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Research has yet to establish convincingly either the existence or the absence of sentencing discrimination. This paper identifies major methodological and conceptual problems inherent in sentencing disparity research and presents an empirical analysis of the racial disparity argument for the years 1969, 1973, and 1977. Evidence is presented that indicates (1) sentencing patterns fluctuate with contextual variation, (2) racial discrimination at the sentencing stage exists, and (3) the detection of sentencing discrimination can easily be obscured by a number of pitfalls.

The past decade has provided us with an extensive and methodologically sophisticated array of empirical studies assessing the degree to which extra-legal variables influence decision making in the criminal court (Hogarth 1971; Chiricos, Jackson, and Waldo 1972; Wolfgang and Riedel 1973; Pope 1975; Bernstein, Kelly, and Doyle 1977; Hagan and O'Donnel 1978; Myers 1979; La Free 1980). These analyses attempted to compensate for the generally unsophisticated research conducted in previous decades (see Hagan 1974).

These studies, however, have failed to provide consistent findings concerning the existence of discrimination in criminal sentencing. Some studies have offered evidence to suggest that sentencing outcomes are directly related to the race of the offender and/or victim (Chiricos et al. 1972; Wolfgang and Riedel 1973; Pope 1975; Hagan 1977; Gibson 1978; Myers 1979; La Free 1980), while others have determined that the effect of race on sentencing outcomes is indirect (Burke and Turk 1975; Swigert and Farrell 1977; Lizotte 1978), mediated through such variables as prior record, offense seriousness, and access to bail. Swigert and Farrell (1977) argue that class has an independent and direct effect on sentence outcomes. Chiricos and Waldo (1975) suggest that sentence outcomes are independent of the social class of the defendant. Finally, Bernstein et al. (1977)

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report that whites and persons employed for a relatively long period of time receive more severe punishments.

The range of contradictory findings calls for closer examination of the sentencing disparity issue. Inconsistent findings might well be the result of contextual and methodological peculiarities. This paper has two major goals: (1) to identify five contextual factors which might account for dissimilar findings, and (2) to provide an empirical test of racial disparity after accounting for some of these factors.

CONTEXTUAL FACTORS

Variation as a Function of Changes over Time

Combining data sets from different years is a popular practice (Wolfgang, Kelley, and Nolde 1962; Bensing and Schroeder 1960; Wolfgang and Riedel 1973; Chiricos and Waldo 1975; Swigert and Farrell 1977) which results in large data sets, but it assumes a static conception of criminal processing. Aggregating over years suggests that sentencing operates in a social vacuum, unaffected by the political, economic, and social climate. We argue that such an assumption is not merely a methodological problem that may be remedied by simple statistical controls, but a basic conceptual problem that may affect both one's findings and one's general view of the legal system.

Research should account for changes in public attitudes toward crime, in judicial legislation, and in the incidence of interstatus crime in order to construct a more dynamic depiction of sentencing. Our data, for example, span a period during which sentence lengths were increased by legislative action. These increases covaried with societal members' perceptions of increasing amount of crime, greater risk of victimization, and increasing leniency of the courts.² The influence of such changes is obscured when sentences given during different years are combined to form one data set. Accordingly, it is necessary to treat year of sentencing as an important explanatory variable.

² A 1975 Harris Survey (as cited in Gottfredson, Hindelang, and Parisi [1978, p. 260]) reports that the public perception of an increase in crime in one's own area has risen from 46% in 1967 to 70% in 1975. In addition, reported uneasiness concerning one's personal safety has increased from 49% in 1966 to 55% in 1975. At the same time, criminal courts are being accused of not performing their duties adequately. Data indicate that the proportion of the public who thought courts were not dealing with criminals harshly enough rose steadily from 66% in 1972 to 84% in 1977 (National Data Program for the Social Sciences 1977, p. 79). Such changes may in fact have an important impact on sentence outcomes as representatives of the court respond to the demands and expectations of the public and to the increased latitude given to them by legislation.

Variation as a Function of Jurisdiction

One of the earliest criticisms of those studies which reported racial sentencing bias was that they were conducted in the South (Hindelang 1969). In the light of more recent studies (Farrell and Swigert 1978), charges of regional influence may be overstated. Other forms of jurisdiction, however, evidently do have an influence. Research suggests that lower courts and rural courts discriminate more than superior courts and urban courts, respectively (Pope 1975; Hagan 1977). If, as the research suggests, discrimination is concentrated in particular areas and courts, data which are not disaggregated by these factors will tend to suppress the detection of disparity.

Variation as a Function of Judge

The majority of sentencing studies have used data which do not take into account the potential impact of individual judges. Several scholars have previously addressed this problem (Gaylin 1974; Nagel 1969). Gibson (1978) demonstrates that, when data are disaggregated by individual judge, discrimination which was not previously observed is revealed. Failure to control for this contextual factor will therefore tend to suppress the detection of sentencing disparity.

Variation as a Function of Type of Decision

With few exceptions (see Bernstein et al. 1977), the majority of disparity research has not analyzed the processual nature of criminal justice decision making. Most studies have analyzed just one decision point. Unfortunately, the selected decision point varies among studies, thereby limiting comparability and possibly accounting for the dissimilar findings.

Pope (1975) suggests that disparity might exist at the time of disposition but not at later decision points, such as the determination of sentence length. In addition, populations in the later stages of the judicial process may be homogeneous in terms of status. If so, findings of no relationship between social class and sentence length should not be surprising.

If, as the research indicates, discrimination is concentrated in the earlier decision-making stages, research which does not account for the processual nature of decision making or which analyzes populations at just the later decision points will tend to produce findings of no discrimination.

Variation as a Function of Victim-Defendant Relationship

Crime has typically been an intraclass and intraracial phenomenon. Persons are victimized by offenders of similar status. The degree to which the

victim-defendant relationship does not conform to this pattern is related to sentence outcome disparity (Garfinkel 1949; Farrell and Swigert 1978; Myers 1979; La Free 1980).

Not accounting for this contextual factor will tend to obscure, because of the overwhelming incidence of intrastatus crime, the detection of disparity. As crime becomes more interstatus, sentencing disparity at the aggregate level should become more readily apparent, and time of sentencing should become a more crucial consideration.

In sum, contextual and methodological factors may account for some of the inconsistency in reported research findings. As we have attempted to show, inattention to contextual factors will generally produce findings of no disparity. In the following example we demonstrate that a finding of no disparity is possible even when racial sentencing disparity is widespread.

Example

Assumption: sentence length is a product of status differentials between the offender and the victim in a manner suggested by the research of Johnson (1941), Garfinkel (1949), and Farrell and Swigert (1978) and the theoretical arguments of Black (1976).³ Given: 100 white robbery offenders and 100 black robbery offenders sentenced in 1973 and matched on prior record and total number of charges; intraracial incidence of robbery in 1973: 93% of black offender robberies and 52% of white offender robberies (National Crime Survey 1973, p. 86); sentence lengths (*a*) black offender/white victim, 20 years; (*b*) white offender/white victim, 16 years; (*c*) black offender/black victim, 10 years; (*d*) white offender/black victim, 5 years.⁴ The average sentence lengths are computed as follows:

$$\text{black offenders} = \frac{(93 \times 10) + (7 \times 20)}{100} = 10.7 \text{ years,}$$

$$\text{white offenders} = \frac{(52 \times 16) + (48 \times 5)}{100} = 10.7 \text{ years.}$$

The example shows that even when 100% of the judges discriminate, researchers who do not account for victim characteristics may conclude that no discrimination exists.⁵ Most research which has found no evidence

³ Sentence severity varies with status differentials in the following manner, from most severe to least severe: (1) low status offender/high status victim, (2) high status offender/high status victim, (3) low status offender/low status victim, and (4) high status offender/low status victim.

⁴ While we specifically selected these four sentence lengths to emphasize our point, any other four sentence lengths also would have produced misleading results.

⁵ The large amount of existing white offender intraracial robbery relative to the small amount of black offender interracial robbery tends to produce a finding of discrim-

of sentencing disparity has acknowledged that discrimination may have occurred earlier in the judicial process⁶ or that it may have occurred in isolated cases, but few if any such studies acknowledge that undetected discrimination may be widespread.

AN EMPIRICAL TEST

Our data consist of all males⁷ sentenced in one southeastern state to active sentences for armed robbery during 1969 ($N = 251$), 1973 ($N = 441$), and 1977 ($N = 502$). Such a data set allows us to address three of the five contextual factors (time, jurisdiction, and type of decision). The absence of two contextual factors should suppress the detection of disparity, as noted earlier, so a finding of no disparity should be treated cautiously. We hypothesize that the detection of racial sentencing disparity at the aggregate level is a relatively recent possibility owing indirectly to changes over time in public opinion, sentencing legislation, and the incidence of inter-racial armed robbery.

We include both legal and extralegal variables in a multivariate analysis of sentence length, employing log-linear analysis (Goodman 1978) to obtain the simplest model of interrelationships which fits the data. The advantages of log-linear analysis over other methodologies available for analyzing categorical data lie in its ability to test for higher order relationships and in its ability to reduce the complexity of interpreting results. Its utility in dealing with sentencing studies has been established by a number of researchers (Burke and Turk 1975; Hagan and O'Donnel 1978; Cohen and Kluegel 1978).

The legal variables traditionally found in studies of sentencing are seriousness of offense and seriousness of offender. The underlying rationale for including these two variables is that the more serious the offense or

ination against whites, if any is detectable at all. However, as the character of robbery becomes less intraracial (and, according to the National Crime Surveys of 1973, 1974, and 1975, it is), disparity should become more easily detectable.

⁶ If, in fact, as some researchers have suggested (Pope 1975; Hagan 1975), discrimination occurs at earlier stages of the judicial process, another problem emerges. If almost all of the low status offenders and only the "worst" of the higher status offenders (e.g., only those who robbed other high status persons, had prior records, injured the victim) are incarcerated, a comparison of the average sentence lengths might produce faulty conclusions. The average sentence length for low status offenders would be based on a heterogeneous group, while the average sentence length for the higher status offenders would be based on a homogeneous group of the "worst" offenders. As the composition of the sentenced higher status group becomes more like that of the lower status group (i.e., more heterogeneous, perhaps as the result of "get tough" legislation), detection of disparity should become more likely.

⁷ Females constitute an extremely small number of armed robbery cases in this state for each of the years under investigation. We therefore limited our analyses to males.

offender, the more severe the sentence. We control for seriousness of offense by limiting our analysis to armed robbery. Our choice of armed robbery is based on two considerations: (1) the relatively large number of cases of this crime, and (2) the fact, as noted earlier, that armed robbery is becoming less intraracial in character. We include two measures of seriousness of offender in our analysis: (1) previous incarcerations, and (2) the total number of sentences received.⁸ Our dependent variable, sentence length, is trichotomized into least severe, moderately severe, and most severe. The cutoff points were determined by inspecting the frequency distributions for each year and taking one-half of one standard deviation above and below the mean as the moderate category.

Typical extralegal variables include class and race. As noted previously, the usefulness of social class in an analysis of this sort is highly questionable. Table 1 presents selected descriptive data which illustrate this point. The population of sentenced armed robbers from this one state is quite homogeneous in terms of education and occupation, and any attempt to treat it otherwise would result in drawing interclass conclusions from intraclass data (see Greenberg 1977). Race, however, does not pose the same problems for analysis. Thus for this type of research it is clearly the better indicator of status. Race is dichotomized into nonwhite and white categories.⁹

We first conduct the analyses with all years combined in order to be consistent with most other research. Finally, we run the analyses separately for 1969, 1973, and 1977 to control for the effects of year of sentence.

Findings

When the four-way cross-tabulations of sentence length, race, prior incarcerations, and total sentences were subjected to log-linear analysis, the best-fitting model for all years combined is the one in which race and total sentences has no effect on sentence length. As shown in table 2, prior incarceration is the only variable which has a relationship with sentence length. When we disaggregate the data by year, however, we find three best-fitting models which are quite different from the best-fitting model with all years combined. None of the variables significantly influence sentence length in 1969 or 1973. In 1977, however, the best-fitting model is one in which prior incarcerations and race significantly influence sentence length. When

⁸ A better measure of offender seriousness would have been prior record, but our present data limit us to prior incarcerations and total number of sentences received. While the findings, therefore, should be treated with caution, Burke and Turk (1975) have demonstrated the utility of prior incarceration as a measure of offender seriousness.

⁹ Blacks constitute 97.5% of the entire nonwhite incarcerated population.

the legal variables are controlled, nonwhites differ from whites in sentence severity in the most recent year, 1977.

The effect parameters (λ 's) presented in table 3 indicate the direction and relative strengths of the relationships involving sentence length. In 1977 whites incarcerated for armed robbery had a greater than average chance of receiving the least severe sentence, while nonwhites had a greater than average chance of receiving a moderately severe sentence. Members of each racial group had average chances of receiving the most severe sentence. Persons with a prior incarceration had a greater than average chance of receiving the most severe sentence and a less than average chance of receiving either the least severe or a moderate sentence. Prior incarceration is evidently more influential than race in the determination of sentence length, as evidenced by the size of the coefficients and by the fact that with all years combined prior incarceration alone remained significant. However, the fact that race did emerge as a significant influence on sentence length should not be underemphasized. This finding indicates that in 1977 racial sentencing disparity appears to have existed in at least one

TABLE 1

NUMBER AND PERCENTAGE DISTRIBUTION OF RACE, PRIOR INCARCERATIONS, TOTAL NUMBER OF SENTENCES, AND DESCRIPTIVE STATISTICS OF EDUCATION, OCCUPATION, AND SENTENCE LENGTHS, 1969, 1973, 1977, AND ALL YEARS COMBINED*

	YEAR			
	1969	1973	1977	ALL YEARS
Race:				
Nonwhite.....	186 (74)	318 (72)	296 (59)	800 (67)
White.....	65 (26)	123 (28)	206 (41)	394 (33)
Prior incarcerations:†				
No.....	91 (39)	233 (57)	349 (72)	673 (60)
Yes.....	144 (61)	174 (43)	136 (28)	454 (40)
Total sentences:				
One.....	142 (57)	256 (58)	398 (79)	796 (67)
Two+.....	109 (43)	185 (42)	104 (21)	398 (33)
Education:				
Mean, years.....	9.22	9.84	9.97	9.75
Median.....	9.29	10.02	9.97	...
Mode.....	9.00	10.00	12.00	...
SD.....	2.32	2.19	1.95	...
Occupation:‡				
Mean.....	6.07	6.10	6.31	6.20
Median.....	6.22	6.37	6.59	...
Mode.....	7.00	7.00	7.00	...
SD.....	.99	1.14	1.16	...
Sentence length:				
Mean, days.....	3,210	3,280	4,672	3,851
Total.....	251 (100)	441 (100)	502 (100)	1,194 (100)

* Percentages are shown in parentheses.

† Sums not equal to total because of missing data.

‡ Hollingshead and Redlich (1958) status rankings (6.0 is semiskilled level).

TABLE 2
 χ^2 VALUES FOR SOME MODELS FITTED TO FOUR-WAY CROSS-TABULATION OF SEVERITY OF SENTENCE LENGTH (S),
 RACE (R), PRIOR INCARCERATIONS (P), AND TOTAL SENTENCES (T), ALL YEARS, 1969, 1973, AND 1977

MODEL	FITTED MARGINALS	df	LIKELIHOOD-RATIO χ^2					P VALUES				
			All Years	1969	1973	1977	All Years	1969	1973	1977		
1.....	(RTP) (S)	14	47.6	22.9*	20.8†	35.2	.001	.06	.11	.01		
2.....	(RTP) (SR) (ST) (SP)	8	13.6	9.8	.0928		
3.....	(RTP) (ST) (SR)	10	32.2	23.5	.0101		
4.....	(RTP) (SP) (ST)	10	14.8	18.1	.1406		
5.....	(RTP) (SR) (SP)	10	18.5	11.3†	.0534		
6.....	(RTP) (ST)	12	33.5	32.2	.0101		
7.....	(RTP) (SP)	12	19.7§	19.2	.0708		
8.....	(RTP) (SR)	12	46.4	26.8	.00101		
9.....	(RTP) (STP)	8	11.1	17.6	.2005		
10.....	(RTP) (SRP)	8	11.4	8.1	.1842		
11.....	(RTP) (SRT)	8	29.3	21.7	.00101		

* Best-fitting model, 1969.
 † Best-fitting model, 1973.
 ‡ Best-fitting model, 1977.
 § Best-fitting model, all years.

TABLE 3
EFFECT PARAMETERS (λ 's) FOR SEVERITY OF
SENTENCE LENGTH, 1977

MAIN EFFECTS	TYPE OF SENTENCE LENGTH		
	Least Severe	Moderately Severe	Most Severe
Race (RS):			
Nonwhite.....	-.17	+.14	+.03
White.....	+.17	-.14	-.03
Prior incarcerations (PS):			
No.....	+.13	+.15	-.28
Yes.....	-.13	-.15	+.28

southeastern state. Our findings, however, must be accepted as tentative, pending the availability of data sets complete enough to account for all the contextual factors.

DISCUSSION

Disparity in sentence length is difficult to detect for both methodological and conceptual reasons. Evidence has been presented in this paper that (1) the public's fear of crime and its negative evaluation of court performance have increased, (2) judicial legislation geared to making the offender more accountable has emerged, and (3) robbery is becoming less intraracial. Racial disparity which does exist, therefore, should be more pronounced at the aggregate level in later years. The importance of these factors will be obscured, however, if one analyzes the data in the typical fashion of combining data sets from different years.

This is clearly the case when our data from 1969, 1973, and 1977 are combined: we find that only the prior incarcerations variable has an effect on sentence length. This finding is certainly consistent with the majority of recent studies, which argue that the effects of status characteristics are indirect at best, mediated through their association with offender and offense seriousness (Burke and Turk 1975; Cohen and Kluegel 1978). However, our analyses show that such a conclusion may be premature.

When the "all years" data set is disaggregated by year of sentence we obtain quite different results. The absence of a relationship between any of the independent variables and sentence length in 1969 and 1973 is clearly interpretable in a classic administration of justice framework—let the punishment fit the crime and not the criminal. Legalists would point to the fact that even the offender seriousness variables have no effect as *prima facie* evidence of a nondiscriminating criminal justice system. But the lack of a finding of disparity in 1969 and 1973 should not automati-

cally be construed to imply that disparity did not exist. Disparity may have been obscured in these two years by (1) an overwhelming number of nondiscriminatory sentences, which at the aggregate level should offset individual cases of discrimination, and (2) low levels of interracial armed robbery, which may not have reached the point where disparity becomes detectable at the aggregate level of analysis. The findings from 1977 do suggest that a charge of racial discrimination can be made. For that year both prior incarceration and race have direct and independent relationships with sentence length.

As our data indicate, there has been a consistent and substantial increase in the actual number and proportion of the incarcerated population who are white, who have not been imprisoned before, and whose present sentence is based on a single offense. The reason for such an increase in prison admissions for those categories of offenders which have up until now held a status of being "advantaged" seems to be found in the public demand for protection and the courts' "get tough" response. Fear of crime is an important issue that judges and elected officials cannot escape. Specific court-watch programs have emerged to keep tabs on the public's elected legal representatives. The awarding of summary probation for offenders, including first-time offenders and persons charged with a single offense, is met with growing hostility. Judges seem to be responding by giving harsher treatment to all categories of offenders. If so, it may well be that inequality in the judicial process has shifted away from the decision to incarcerate or not to incarcerate to the determination of sentence length. "Harsher treatment" may mean active sentences for members of groups which have traditionally received summary probations and longer sentences for members of groups which have traditionally received active sentences. Such an interpretation, we believe, is consistent with the data presented. In any case, the findings of this study should alert the researcher to some of the problems inherent in sentencing research that can affect one's empirical findings and hence one's interpretations. If, as we suspect, criminal sentencing is a phenomenon influenced by social, political, and economic factors (not unlike other phenomena), and crime is becoming less intrastatus, previously undetectable disparity may become more apparent. As overt race and class disparity in criminal sentencing becomes more detectable at the aggregate level, charges of structural discrimination will be more tenable.

CONCLUSION

Our primary objective in this paper has been to describe and in some instances to demonstrate common pitfalls in the detection of sentencing discrimination. We are sure that some readers will claim that we are engaged

in a futile effort, since studies have yet to show convincingly the existence of disparity in the criminal justice system. We maintain, however, that in a society based on equality, such research should be encouraged and its techniques continually subjected to careful scrutiny. The issue of committing type I versus type II errors is relevant here. Is it preferable to conclude that discrimination may exist when in fact it does not, or is it preferable to conclude that discrimination may not exist when in fact it does? In the past, research which found no discrimination demanded little, if any, explanation or reanalysis, while research which found discrimination was correctly subjected to such scrutiny (Hagan 1974; Hindelang 1969). As we have tried to show, reanalysis and critical evaluation should also be demanded of those studies which suggest that no discrimination exists.

We have attempted to demonstrate that, when contextual variation is considered, racial discrimination is more or less pronounced. Although many studies have considered some of the issues outlined in this paper, none has previously addressed the temporal contextual factor. Future research is needed to account for all of the methodological and contextual factors discussed here. Such research should resolve a great deal of the controversy surrounding the existence of sentencing disparity.

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

CIRCULARITY IN THE DUAL ECONOMY: COMMENT ON TOLBERT, HORAN, AND BECK¹

Given the increasing interest in industrial segmentation in sociological theorizing and research (Stolzenberg 1975, 1978; Bibb and Form 1977; Spilerman 1977; Hodson 1978; Beck, Horan, and Tolbert 1978; Daymont and Kaufman 1979), we are pleased to see a new attempt to define industrial sectors empirically. Most previous work on economic sectors has been based on narrative descriptions of industrial structure (Averitt 1968; Bluestone 1970; O'Connor 1973; Bibb and Form 1977; Beck et al. 1978). Using readily available secondary data sources of industrial characteristics, Tolbert, Horan, and Beck ("The Structure of Economic Segmentation," *AJS* 85 [March 1980]: 1095-1116) advance the task of empirical definition. Their emphasis on the multidimensional nature of the economy is sound. Their theoretical work demonstrates a recognition of the multidimensionality of economic segmentation by drawing a distinction between potential for oligopolistic power and oligopolistic behavior. In addition, they escape from the narrow focus on manufacturing industries inherited from economics, where the bulk of the research on segmentation has originated. Their work exhibits a welcome openness to the question

¹ We would like to thank Charles N. Halaby and Robert M. Hauser for helpful comments on this paper.

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of how to operationalize segmented economics by giving serious consideration to both categorical and continuous measures.

While we think their task is a significant one, serious flaws exist in Tolbert et al.'s theoretical and empirical analysis. Most important, their conceptualization is contaminated by a circularity between the defining characteristics of economic segmentation and outcomes resulting from economic segmentation. Second, their reported finding of a one-factor solution is incorrect. At least two factors are needed to represent their data adequately. Third, their categorization of factor scores into two sectors is arbitrary. Given these shortcomings, their measures of economic segmentation cannot be used for the analysis of labor market outcomes.

In both earlier work and the current piece the authors appear to be aware of the distinction between defining characteristics and outcomes:

From our perspective, these labor market characteristics are seen as predictable outcomes of the sectoral structure, not as their defining characteristics. [Beck, Horan, and Tolbert 1978, p. 706]

Dual economy writers acknowledge the existence of segmentation in labor markets, but treat it as the consequence of more fundamental processes of segmentation in the economic order. "The central theoretical assertion . . . is that behavior observed in the labor market . . . reflects more fundamental processes in production itself. . . . To understand the labor market processes which 'produce' group differences in incomes, unemployment, and mobility, then, we must investigate the institutional arrangements governing production" (Edwards et al. 1975, p. 4). [P. 1097]

It is essential to maintain such a distinction both conceptually and empirically if a construct is to have any utility for the testing of hypotheses.

It is somewhat surprising and unfortunate, then, that Tolbert et al. contaminate their operationalization of economic segmentation with measures of key labor market outcomes. That is, included in the indicators that they use to define sectors are measures of important labor market outcomes which should be considered only as dependent variables. Their inclusion of median personal annual income, job tenure, and frequency of job terminations in the definition of economic segmentation renders the resulting operationalization unusable for the testing of hypotheses concerning labor market outcomes (e.g., wages and job stability).

This circularity is particularly evident in the procedure employed to create scores for the professional and government industries for which Tolbert et al. have few data. The only characteristic used for this procedure was median personal annual income, which results in simply sorting these industries into high and low wage categories. To use a measure constructed in this way in the analysis of individual earnings is clearly unacceptable.

A second issue of importance for segmentation research is raised by

Tolbert et al.'s attempt to conceptualize oligopolistic power. The authors organize their measures under the concepts of oligopolistic capacities and oligopolistic behavior.² But many of these measures tap dimensions of power which deserve to be conceptualized separately from oligopolistic power. For example, corporate size only tangentially indicates a capacity for oligopolistic power in product markets. More important, it taps the ability to engage in long-term planning, to introduce new technologies, to relocate plants, to influence the political process, to deal with organized labor, and to establish internal labor markets (Galbraith 1967; Averitt 1968; Doeringer and Piore 1971; O'Connor 1973; Edwards, Reid, and Gordon 1975). These are all key components of industrial structure which cannot be subsumed under the concept of oligopolistic market power. In addition, there are other dimensions of industrial structure which are important for labor force analysis, for example, capital intensity, foreign investment, and conglomerate domination (Averitt 1968; O'Connor 1973; Amin 1976). Given the multiple dimensions of industrial structure important for labor force analysis, the imposition of a unidimensional solution on the operationalization of economic segmentation seems artificial.

Indeed, a reanalysis of Tolbert et al.'s data based on the correlations which they present in their table 2 for the nine variables in their final model reveals the need for (at least) a two-factor solution.³ In our table 1 we present the initial eigenvalues extracted from their matrix of correlations. The first two factors have eigenvalues greater than 1.0, which is

TABLE 1
INITIAL EIGENVALUES EXTRACTED FROM
TOLBERT ET AL.'s CORRELATION MATRIX

Factor	Eigenvalue	% of Variance	Cumulative % of Variance
1.....	4.09	45.4	45.4
2.....	1.40	15.6	61.0
3.....	.92	10.3	71.3
4.....	.76	8.4	79.7
5.....	.57	6.3	86.0
6.....	.44	4.9	90.9
7.....	.34	3.7	94.6
8.....	.31	3.4	98.0
9.....	.18	2.0	100.0

² One of their indicators of oligopolistic behavior is the mean profit in an industry. Such an indicator measures corporate size to a greater extent than it measures oligopolistic behavior. Profit rate would be a more appropriate indicator of oligopolistic behavior.

³ By forcing a one-factor solution on the correlations which Tolbert et al. present in their paper, we are able to reproduce, with at least two decimal point accuracy, the results which they present in their table 4.

the standard criterion for including factors in subsequent analysis (Harman 1976). Moreover, it would not be unreasonable to consider including the third factor, as it accounts for an additional 10% of the variance. In table 2 we present the factor pattern and communalities for the two-factor solution. These results indicate that mean corporate profit, employment size, and the measures of job stability load heavily on the second factor and are not adequately represented by the first factor. Tolbert et al.'s reporting of a one-factor solution when more than one factor is required to represent the data is inaccurate.

Since we argue that their inclusion of median personal annual income, job tenure, and frequency of terminations created circularity in the definition of labor market segmentation, we performed an additional factor analysis using a subset of their variables. Table 3 presents the initial eigenvalues extracted from the correlation matrix, and we see that the first two factors have eigenvalues greater than 1.0. Examining table 4

TABLE 2
ROTATED TWO-FACTOR SOLUTION FROM TOLBERT ET AL.'s DATA

VARIABLE*	FACTOR PATTERN		COMMUNALITY
	Factor 1	Factor 2	
Concern.....	.690	.257	.542
Mincome.....	.815	.191	.701
Profit.....	.040	.912	.834
Psuper.....	.674	.260	.522
Punion.....	.484	.099	.244
Quits.....	-.234	-.402	.216
Tenure.....	.403	.456	.370
Wklyhrs.....	.792	.219	.675
Wnworkers.....	.269	.722	.594
Eigenvalues after principal axis iteration.....	3.649	1.050	...

* For the definition of these variables see Tolbert et al.'s table 1.

TABLE 3
INITIAL EIGENVALUES EXTRACTED FROM TOLBERT
ET AL.'s CORRELATION MATRIX USING
NONCIRCULAR VARIABLES

Factor	Eigenvalue	% of Variance	Cumulative % of Variance
1.....	2.95	49.2	49.2
2.....	1.19	19.8	69.0
3.....	.76	12.6	81.6
4.....	.48	8.1	89.7
5.....	.33	5.5	95.2
6.....	.29	4.8	100.0

TABLE 4
ROTATED TWO-FACTOR SOLUTION FROM TOLBERT ET AL.'s DATA
USING NONCIRCULAR VARIABLES

VARIABLE*	FACTOR PATTERN		COMMUNALITY
	Factor 1	Factor 2	
Concen.....	.618	.200	.422
Profit.....	.108	.834	.708
Psuper.....	.645	.225	.466
Punion.....	.539	.092	.299
Wklyhrs.....	.875	.163	.793
Wnworkrs.....	.329	.770	.701
Eigenvalues after principal axis iteration.....	2.546	.843	...

* For the definition of these variables see Tolbert et al.'s table 1.

shows that the two measures of corporate size (mean profit and mean employment) load most heavily on the second factor, with the remaining variables loading on the first factor. Again, a two-factor solution is required to represent even this limited set of industrial characteristics.

Turning from the factor analysis to Tolbert et al.'s categorization of sectors, further questions can be raised. Given that a minimum of two factors are necessary to represent the internal structure of the data, it is unlikely that in moving to a categorical representation a dualistic measure will suffice.⁴ Even restricting attention to a one-factor solution as Tolbert et al. do, there are multiple possibilities for constructing a categorical measure. The authors create two categories by dichotomizing the factor scores based on a break in the distribution near the mean. A look at the distribution of factor scores presented in their table 5 reveals four other breaks of comparable magnitude within 1.25 standard deviations of the mean. Using these breakpoints, one could construct from two to six sectors using a variety of industrial combinations. In addition, their inclusion of government industries in the core highlights the artificiality of the forced dual solution. Tolbert et al.'s measures of economic power have totally different meanings when applied to government as opposed to private industries. For the government industries there are different factors that are important for labor market outcomes, such as the impact of periodic public elections, job security based on political patronage, and the civil service system (Hamernesh 1975). Most of the existing literature on economic sectors and labor market segmentation acknowledges these differences by consistently treating the government as a separate sector (Baran and Sweezy 1966; Averitt 1968; Bluestone 1970; O'Connor 1973; Hodson 1978). Given the multidimensionality of the economy, a move

⁴ If, indeed, there are only two clusters of industries, it should be possible to represent these clusters by a single factor.

away from a simple dualistic categorization is both conceptually and empirically necessary.

In conclusion, Tolbert et al. should be commended for helping to introduce a multidimensional and quantitative approach to defining economic sectors and for demonstrating an openness to the issue of categorical versus continuous measures of industrial structure. Nevertheless, their work is fundamentally flawed by circularity, which renders their subsequent analysis of individual earnings meaningless. In addition, their work fails to fulfill the promise of a truly multidimensional approach. Future theoretical work in this area could build most profitably on the work of Tolbert et al. by developing numerous conceptual dimensions of economic segmentation. Similarly, researchers must engage in serious empirical work designed to construct continuous and categorical measures of economic segmentation which adequately represent the complexity of the American economy. In this respect we would like to call attention to a number of readily available data sources, listed in our Data Source Bibliography. An additional avenue is the measurement of economic structure at the enterprise level. Stolzenberg (1978) moves in this direction, utilizing a self-report of employment size to analyze differentials in educational returns across corporate sectors. To the extent that data sets reporting name of employer are available or can be constructed, researchers may find useful some of the corporate-level data sources in the Data Source Bibliography.

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THE CIRCLE HAS NO CLOSE¹

Hodson and Kaufman propose three criticisms of our work: (1) our analysis of economic segmentation is "contaminated" by circularity, (2) our sectoral measures are invalidated by our use of a one-factor solution, and (3) our choice of cutting point for the dichotomous sectoral measure is arbitrary. Taking each in turn, we will show that the first two criticisms are demonstrably false and that the third is irrelevant.

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Their charge of "circularity" in the construction of our measure of economic segmentation involves two separable elements. First, they suggest that it is circular to use aggregate industry-level measures of income in the construction of an economic segmentation index which will be used to account for individual-level income variation. Their confusion on this point apparently stems from a lack of appreciation of the complex relationship between aggregate-level and individual-level relationships. Three decades ago W. S. Robinson (1950) drew attention to this distinction through a discussion of what was called the "ecological fallacy." Since Robinson's original essay there has emerged an abundant literature on this issue (see, e.g., Hannan 1970, 1971).

While we shall not reproduce the algebraic manipulations here, it can be easily demonstrated that there is no necessary relationship between empirical relationships at the aggregate level and those at the individual level. In fact, it is quite possible to find a strong relationship between two variables at the aggregate level and yet find no relationship, or even a relationship of opposite direction, at the individual level.

Moreover, there is some irony in Hodson and Kaufman's comment. Precisely the same criticism was raised concerning the use of Duncan's Socioeconomic Index to represent occupation within the status attainment model. That is, the SEI is constructed using aggregate (occupational) measures of income and education and then used in individual-level analyses of the interrelationships among education, occupation, and income. Thus if Hodson and Kaufman believe that the logical consequence of such a construction "renders the resulting operationalization unusable for the testing of [individual-level] hypotheses," they have in effect dismissed the entire empirical literature in the status attainment tradition. While we are not totally unsympathetic with such a conclusion, we would use a much different line of argument to reach it (Beck, Horan, and Tolbert 1978, 1980; Horan, Beck, and Tolbert 1980). For a discussion of the circularity issue in status attainment, see Blau and Duncan (1967, pp. 124-25).

The second element in Hodson and Kaufman's circularity critique is the argument that it is incorrect to include measures of labor market outcomes in an operationalization of dual economy theory. We disagree with this point on theoretical grounds (see the top of p. 1097, where we stated the rationale for our analysis). Our reading of the dual economy literature led us to identify the fundamental underlying theoretical position as that of the existence of systematic causal relationships between the industrial organization of production and the characteristics of industrial labor markets. While our analysis cannot provide a test of the direction of causal flow between such elements, it can and does provide a test both for the existence of systematic relationships between the two and for the corre-

spondence between the observed relationships and those expected on theoretical grounds. In short, the distinguishing characteristic of dual economy theory, as opposed to dual labor market theory, is the proposal of a correspondence between the economic organization of industry and the characteristics of industrial labor markets. It is this correspondence which our aggregate-level analysis was designed to test and which our measures of economic segmentation were constructed to represent.

Our analysis sought to operationalize and evaluate dual economy theory using current aggregate industrial data. The literature from which our specification of dual economy theory derives is diverse and heterogeneous, and there is certainly foundation in that literature for theoretical specifications which differ from ours. Until Hodson and Kaufman present an alternative theoretical model, it is impossible for us to evaluate comparisons between their unstated model and our own. But given our theoretical specification, the inclusion of labor market characteristics in the analysis was not only appropriate but necessary.

Another criticism presented by Hodson and Kaufman concerns our selection of factor solutions. They argue that our choice of a one-factor solution "seems artificial" and that a reanalysis of the data "reveals the need for (at least) a two-factor solution." There are two separate issues here. The first concerns the choice of criterion for the retention of factors. We interpreted an eigenvalue of 1.039 as not rounding to a value greater than unity and chose not to include this marginal second factor in our final solution.² They argue that this second factor should be included and that the use of one rather than two factors renders our measure of economic segmentation "inaccurate." Not wishing to quibble over 39/1,000 of an eigenvalue, we will address this issue empirically. In table 1 we present the two-factor oblique solution for the nine aggregate variables included in our original analysis.³ Note that the two factors here correlate at .423. This second factor can be interpreted in a manner consistent with dual economy theory as representing the economic scale of industry (mean industry profits and number of workers).

But the real test of their criticism is an empirical one. How different is an economic segmentation index based on a two-factor solution from that based on a one-factor solution? To evaluate this question, we repeat our index construction process for the two-factor solution. This new index (I_2) is defined as a weighted linear combination of the factor scores (F_1 ,

² We assume that the differences between these and the eigenvalues presented by Hodson and Kaufman in their table 2 are the result of their use of the two-place correlation coefficients from table 2 of the original text.

³ Readers are encouraged to consult the original text for sources and definitions of the variables.

TABLE 1
TWO-FACTOR ITERATED PRINCIPAL AXIS SOLUTION
OF INDUSTRIAL CHARACTERISTICS
(Promax Rotated Factor Pattern)

Variable	Factor 1	Factor 2
Concen.....	-.693	-.092
Mincome.....	-.845	.015
Profit.....	.142	-.951
Psuper.....	-.673	-.101
Punion.....	-.508	.026
Quits.....	.171	.366
Tenure.....	-.336	-.387
Wklyhrs.....	-.812	-.025
Wnworkers.....	-.132	-.707
Eigenvalue.....	3.651	1.039
Portion of variance explained.....	.779	.222

F_2) in which the weights are the relative importance of each factor as reflected by their respective eigenvalues (E_1 , E_2):

$$I_2 = \frac{E_1 F_1 + E_2 F_2}{E_1 + E_2}.$$

Table 2 presents the one-factor (I_1) and two-factor (I_2) indexes for the set of 55 industries. How different are these two indexes? The product-moment correlation between I_1 and I_2 is .995, and the Spearman rank order correlation is .994. The dichotomous version of the indexes based on one- and two-factor solutions exhibits a level of correspondence which will presumably meet any standard of accuracy. These indexes are identical.

The third criticism raised by Hodson and Kaufman concerns the "arbitrary" character of the cutting point for the dichotomous measure. We made no claim to the contrary, taking care to note that the choice between a dichotomous and a continuous measure would be conditional on prior theoretical specification as well as on research strategy. Given a symmetric distribution for the continuous measure, we justified using a cutting point adjacent to the mean on the basis of a substantial gap between industries at that point. Note that, in the two-factor solution favored by Hodson and Kaufman, the same dichotomy derives from a comparable gap at the mean of the distribution. Thus while there is nothing "sacred" about our cutting point, neither is there anything problematic about it.

Hodson and Kaufman's suggestion that comparable gaps in other parts of the continuous measure are indicative of multiple sectors borders on mindless empiricism. This is not to say that divisions of the continuous measure into 3, 6, or even 16 categories would not be a legitimate (but arbitrary) operation for researchers whose theoretical model can attach some meaning to such multiple categories on the oligopoly-competition

dimension. It is to say that such decisions cannot be made in a theoretical vacuum.

Finally, Hodson and Kaufman suggest that we violate a convention in the literature of "consistently treating the government as a separate sector." But the five references they cite reflect no consensus on this issue. Bluestone's (1970; Bluestone, Murphy, and Stevenson 1973) model does in fact have three sectors, but government is not one of them. In addition to the conventional core and periphery sectors, Bluestone (1970, p. 25) suggests an "irregular" economy existing primarily in the urban ghetto. While Averitt (1968, pp. 155-72) points out several dissimilarities between public employment and core sector industries, he places considerably more emphasis on similarities between public employment and the industrial core (see esp. pp. 156-59). The position that the government should be treated as a separate sector is more common in the Marxist literature, but even here there is little in the way of consensus. Baran and Sweezy (1966, p. 344) suggest that government employment falls in a "service sector" but argue against distinguishing between the state and the monopoly capital sector (p. 67).

Thus of the five references cited by Hodson and Kaufman, only two (O'Connor [1973] and Hodson's [1978] operationalization of O'Connor) provide unequivocal support for their position. A more representative survey of the dual economy literature would provide even less support. While we have no objection to the notion of a state sector, the decision about whether to include one must be based on theoretical grounds. Contrary to Hodson and Kaufman's assertion, there is little precedent for such a distinction in the dual economy literature.

More generally, Hodson and Kaufman exhibit a certain confusion about both the purpose and meaning of our analysis. They suggest that our purpose was (or should have been) to determine the ultimate dimensionality of industrial differentiation in the United States, when in fact our purpose was to operationalize and evaluate a particular theory of industrial organization. We did not include every conceivable indicator of industrialization, only those available indicators which seemed relevant to a dual economy theory. We did not seek to account for all variation among these indicators. Instead, we sought to test for the systematic kinds of variations proposed by the theory. In fact, the results of our analysis (as well as the parallel efforts of Oster [1979]) provide striking empirical support for the dual economy perspective.

We have demonstrated that Hodson and Kaufman's criticisms of our empirical analysis have no foundation. But how can we account for their divergent interpretations of our analysis? How is it that they look at evidence for important regularities in economic organization and see only "complexity"? We can only suggest that the apparent complexity (or

TABLE 2
COMPARISON OF ONE-FACTOR AND TWO-FACTOR SOLUTIONS

Industry	One-Factor Index	Industry	Two-Factor Index
Nmfg: petroleum products.....	2.40	Nmfg: petroleum products.....	2.07
Nmfg: motor vehicles.....	2.21	Dmfg: motor vehicles.....	1.86
Dmfg: primary metal.....	1.31	Nmfg: chemical and allied products.....	1.18
Nmfg: chemical and allied products.....	1.30	Dmfg: transportation equipment.....	1.10
Dmfg: transportation equipment.....	1.25	Dmfg: primary metal.....	1.06
Mining: petroleum and natural gas.....	.99	Mining: petroleum and natural gas.....	.97
Nmfg: paper and allied products.....	.92	Utilities and sanitary services.....	.85
Utilities and sanitary services.....	.87	Nmfg: paper and allied products.....	.79
Nmfg: tobacco.....	.86	Dmfg: machinery, except electrical.....	.74
Dmfg: professional and photographic equipment.....	.85	Dmfg: professional and photographic equipment.....	.74
Dmfg: electrical machinery.....	.83	Dmfg: electrical machinery.....	.68
Dmfg: machinery, except electrical.....	.81	Mining: nonmetallic quarrying.....	.63
Mining: nonmetallic quarrying.....	.68	Communications.....	.63
Communications.....	.68	Mining: metal ores.....	.61
Mining: metal ores.....	.64	Nmfg: tobacco.....	.60
Mining: coal.....	.60	Mining: coal.....	.53
Dmfg: stone, clay, glass products.....	.46	Finance: securities, commodities brokerage.....	.49
Transportation.....	.45	Transportation.....	.47
Dmfg: fabricated metal.....	.43	Dmfg: stone, clay, glass products.....	.45
Nmfg: food, kindred products.....	.42	Dmfg: fabricated metal.....	.42
Finance: securities, commodities brokerage.....	.38	Nmfg: food, kindred products.....	.41
Miscellaneous manufacturing.....	.31	Nmfg: printing and publishing.....	.32
Nmfg: printing and publishing.....	.29	Nmfg: rubber and miscellaneous plastics.....	.32
Nmfg: rubber and miscellaneous plastics.....	.27	Const: general, except building.....	.30
Const: general, except building.....	.26	Miscellaneous manufacturing.....	.30
Wh Tr: machinery, equipment, supplies.....	.16	Wh Tr: machinery, equipment, supplies.....	.26
Finance: banking.....	.06	Const: general building.....	.12

TABLE 2 (Continued)

Industry	One-Factor Index	Industry	Two-Factor Index
Const: general building	.05	Const: special trade contractors	.12
Const: special trade contractors	.05	Finance: insurance	.05
Finance: credit agencies other than banks	.03	Finance: credit agencies other than banks	.03
Finance: insurance	-.01	Finance: banking	.01
Wh Tr: groceries and food	-.06	Wh Tr: groceries and food	0
Nmfg: textile mill products	-.28	Nmfg: textile mill products	-.23
Finance: real estate	-.30	Dmfg: lumber and wood	-.32
Agricultural services	-.39	Finance: real estate	-.33
Dmfg: furniture	-.47	Dmfg: furniture	-.34
Dmfg: lumber and wood	-.48	Bus Serv: advertising	-.40
Agricultural production	-.56	Agricultural services	-.51
Wh Tr: miscellaneous wholesale trade	-.58	Nmfg: leather products	-.51
Bus Serv: advertising	-.61	Wh Tr: miscellaneous wholesale trade	-.52
Nmfg: leather products	-.63	Rt Tr: building materials	-.54
Rt Tr: building materials	-.68	Rt Tr: furniture, home equipment	-.71
Rt Tr: furniture, home equipment	-.86	Agricultural production	-.72
Rt Tr: auto sales, service stations	-.88	Rt Tr: auto sales, service stations	-.75
Nmfg: apparel products	-.92	Bus Serv: miscellaneous business services	-.80
Bus Serv: miscellaneous business services	-.94	Nmfg: apparel products	-.83
Bus Serv: auto repair	-1.00	Bus Serv: auto repair	-.88
Entertainment and recreation services	-1.03	Rt Tr: miscellaneous retail trade	-.94
Rt Tr: miscellaneous retail trade	-1.04	Entertainment and recreation services	-.99
Rt Tr: food stores	-1.13	Rt Tr: food stores	-1.07
Rt Tr: general merchandise	-1.43	Rt Tr: general merchandise	-1.38
Hotels and motels	-1.45	Hotels and motels	-1.42
Other personal services	-1.52	Other personal services	-1.50
Rt Tr: apparel	-1.61	Rt Tr: apparel	-1.52
Rt Tr: eating and drinking places	-1.93	Rt Tr: eating and drinking places	-1.88
Mean	0	Mean	0
S.D.	.93	S.D.	.85
Skewness	.17	Skewness	-.05

NOTE.—Key to abbreviations: Nmfg = nondurable manufacturing; Dmfg = durable manufacturing; Const = construction; Wh Tr = wholesale trade; Bus Serv = business services; Rt Tr = retail trade.

simplicity) of an empirical phenomenon is in part a function of the theoretical framework which is used to understand it (Horan 1978). In the absence of a theoretical framework, any phenomenon may appear complex.

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Review Essay: Kings or People¹

Kings or People: Power and the Mandate to Rule. By Reinhard Bendix. Berkeley and Los Angeles: University of California Press, 1978. Pp. xii+692. \$20.00.

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Most contemporary sociologists prefer to explain the rise of large-scale national societies and state structures as products of the power of self-interested groups in coercive and exchange relations with each other. Internal or external groups gain economic or military power and use this power to construct and legitimate central institutional authority. This is the conventional theory of the rise of the modern state. Differing lines of thought emphasize economic or military power, or see the system as functioning for society as a whole, a single dominant group, or several conflicting groups. The important point is that modern theorists tend resolutely to be reductionist: the power and interests of groups exist first, and the state is a superstructural product of these prior interests. Alternative notions stressing systemic rather than subunit forces (e.g., Parsons's general values) are discredited as idealistic, symbolic, and unreal. We have brought men back in, individually and in groups, and conceive of their interests as creating the larger society. Contemporary explanations of collective life and action must start with the narrow interests of one or another group: the capitalists, the state managers, the military, or even the intellectuals.

Much is left out by this strategy, and a good deal of evasion seems to be permissible. In general, one may successfully get away with being an idealist at the group level, but not at the societal level. Capitalists or economic managers may be seen as faithful agents of large-scale organizations, or even of capitalism as a whole, rather than of private interests. Military elites can be seen as agents of their authority as a political estate and state functionaries as agents of the state structure as a whole. But such explanations seem unacceptable at the societal level. To assume that actors and groups act as agents of society as a whole is unthinkable. To assume that group interests are derivative—constructed, sustained, and legitimated by a collective order—seems naive. The history of the modern world, with its construction of the "individual" as a constitutive element of society, with the attribution of purposes and interests to such individuals, and with the legitimation of both the individuals as entities and

¹ I am indebted, for suggestions, to Francisco Ramirez, George Thomas, and Morris Zelditch, Jr.

their purposes as forces, has established a distinctive ideology and language for social theory. Actors are individuals in pursuit of food pellets and the like: they are not agents of collective authority and of the gods, which appear only as precipitates of the fundamental process.

These individualist and interest group theories tend to evade the problem of accounting for the construction and persistence of social order. Complex authority systems obviously have enormous staying power: they survive and change through processes our theories poorly explain. As a concrete instance, we have little good sociological theory of the origins and persistence of the modern state and state system.

Reinhard Bendix addresses this issue directly. In part 1 of *Kings or People*, he describes the development and legitimation of the central institution of collective authority—kingship—in Japan, Russia, Prussia (and the Empire), and England, and in less detail in other instances. Then, in part 2, he gives a parallel account of the later rise and evolution of the institutions legitimating popular authority in England, France, Germany, Japan, and Russia. The effort is enormous. It will be given little credence by specialized historians—by and large the case studies, built on secondary sources, add little new in the way of historical analysis and interpretation. Their point is to show empirically the great persistence of collective authority and to show that even major changes tend to be constrained by previous forms. New developments in response to changed power distributions take long periods to form, as the requisite foundation of custom—legitimation—is built up and as new forms of organization slowly incorporate the altered authority arrangements. Much cultural, religious, and intellectual work is involved.

This larger point is supported by the crushing weight of the historical record. And it is indeed a point glossed over in current theory. But Bendix's work, for all its extraordinary empirical detail, adds curiously little to any general conceptualization. Methodologically, the book is an example of the case study approach gone out of control. Detail swamps attempts at clear generalization. Comparisons are fuzzy. The general argument becomes an ideological canopy that covers disjoint case materials. The case studies collapse into historicism—quite literate as little histories, but lacking more general sociological substance.

A more central difficulty exists. Bendix relies almost entirely on empirical examples to sustain his point. The book is strangely atheoretical: of all Bendix's work, it is the most Weberian in aspiration, but the least so in its theoretical content. The reason is clear: Bendix is utterly defensive in the face of an intellectual generation of theoretical reductionists and materialists. He accepts the framework of his foes—society is depicted as filled mainly with groups and individuals infused with particular private interests of their own—and takes for himself only what little is left in law, ideology, values, and intellectual constructions. This sad retreat is conducted with some bitterness: the obviously relevant works of Waller-

stein (1974) and Moore (1966) are dismissed in one or two footnotes, and Tilly (1975) and Perry Anderson (1974) are ignored entirely.

In avoiding the current materialist and interest group theories, Bendix gives up almost the whole ground, retreating to a narrow idealism. What is left for a conception of the enduring power of collective order? Some religious legitimation. The power of the law and the lawyers. And a great inclination to credit with causal force the cultural products of the intellectuals. Everything else is given over to the materialists for whom Bendix evidences such distaste. Interests are brought in when necessary—especially those constituted by political forces—but grudgingly: Bendix accepts with little awareness or criticism the view of his theoretical opponents that interests reflect the private goals of groups. We have, then, a version of 19th-century idealist history, with almost no conceptual innovation or adaptation in the light of contemporary thinking—it is as if Bendix hoped recent decades in the history of comparative work on political systems would just go away. In the course of the retreat, Bendix makes himself a straw man—easy to dismiss as attributing too much causal power to disembodied ideas. This despite the fact that the historical record sustains him empirically in his emphasis on the persistence and partial autonomy of legitimated forms of authority.

What is the intellectual solution? Bendix takes little account of one side of the phenomenological tradition—the idea that the entities of social reality, as well as their relations, are socially constructed. It is a side of Weber he has always played down, perhaps in deference to fashionable, if primitive, positivisms. Actors, groups, and societies come to exist in a sense beyond the social construction of particular legitimated rights, duties, obligations, and functions: indeed, the rights and functions often follow from the constructed entity of the societal system and its component groups and actors. Legitimizing ideas, thus, are often much more than values or norms resting in sentiments: they are accounts of how the reified parts of the social world fit together and function and have a cognitive status as much or more than they do a moral one. Their moral components rest on the definition of the entities involved. The legitimation of the authority of the king is an account, predicated on the existence of the realm, of how this realm functions in crucial respects. If one sees the king's authority as simply derived from social relations in a more naive way, one ends up, as Bendix does, attributing a peculiarly dominant role to a few lawyers and intellectuals and religious leaders. These functionaries are giving an account of society, not only of the king.

What social forces construct and maintain the realm and its king? One must go beyond an analysis of a few elites whose interests are directly involved. All sorts of actors and groups in society have interests, not only in pursuing their courses of action, but also in sustaining their existence within the constructed collectivity. The same collective reality that sustains the legitimate functions of the king also constructs and sustains others. Actors and groups are defined and legitimated by accounts of their

functions in the wider collectivity: they are agents, not only of "private" interests, but also of that collectivity. They have, thus, a considerable stake in the collective account that stabilizes their agency and legitimates their jurisdiction: Bendix could have put to good use the ideas of his colleague Swanson (e.g., 1971). As it is, he lacks a language in which to describe the links between legitimated rights and the reality in which the rights make sense. He is left describing systemic phenomena in limited and idealistic terms, as values, culture, and ideas.

In any event, all social action is undertaken on behalf of the gods (now often secularized) representing collective authority, as well as on behalf of the "men" who are themselves constituted by this authority. We have only the most cumbersome ways to discuss the point: actors are Other to their conduct as well as Self. Their actions construct, maintain, and reflect wider authority: every social behavior is both an action within a frame and a theory of the frame itself.

Language is much of the problem: our conceptions of societal structure tend to be simple generalizations from the system of action and exchange. Structure then becomes rules and norms and values—idealist reflections of the lower level we see as real. At the lower level, we take for granted, rather than analyze, the existence of the acting and exchanging units, ignoring their social construction. Similarly, at the higher level, we ignore the main constituent parts of the societal structure—the entities that are constructed. We study the rules and leave out the entities within which the rules make sense and from which components acquire purpose, meaning, and legitimacy. Our conception of societal structure is all vacant reified process: the entity of society and its central components is left out. Thus we have Bendix's history of kingship and its legitimization with little account of the historical evolution of the realm (i.e., the constructed collectivity) within which the kingship makes sense. Without a clear depiction of the evolving collectivity of which the king is agent, his legitimacy is a collection of airy ideas and sentiments and his authority a set of legal rules. To understand why so many social actors devote so much effort to building up a given authority we had best understand the world that authority acts for. Otherwise support will seem to arise from attitudes.

We could all do with a moratorium on the abstracted use of these terms, ruling out any use of the term "authority" that does not specify the nature, purposes, and structure of the collectivity for which the authority is agent, and ruling out any use of the term "legitimacy" that does not include an account of how authority fulfills the purposes of the collectivity.

This theoretical problem gives rise to a specific conceptual problem as Bendix's book wears on into its discussion (pt. 2) of modern nation building. It is empirically quite clear—and one of Bendix's main points—that the process is a world-system one. Events in one country (e.g., the French Revolution) radically reconstruct the vision of reality in the others and become models of binding force. Thus the evolution of the modern world-

wide conception of citizenship moves and develops from society to society around the world. How to say this—that ideas sweep the world? This is the intellectual style Bendix adopts, with no conceptualization beyond the observations themselves. There is mainly the repeated assertion of the power of the intellectuals; there is no serious structural mechanism to account for the phenomenon. It is as if Bendix imagined that any larger process would refer to the world economy and the “interests” it generates and thus would be an unacceptable concession to his foes—as if a reference to, say, Wallerstein (1974) would be required.

This is not quite the case. All the European societies in the modern period were deeply embedded, not only in a world commodity economy and system of exchange, but also in a constructed world collectivity—a society and a stateless polity: natural reality and human action were defined as objective and lawlike, progress was defined in increasingly rational and homogeneous terms, and the individual was reified as a central societal agent (with attendant religious supports). In such a world, with societies organized in a common rationalized reality, but with little in the way of an organized and rational supersocietal state, social functions drifted rapidly to rationalized competitive states. States and societies acquired entitativity and the corresponding capacity for collective action within the common world frame. Each organizational innovation along such lines is quickly copied (through competitive imitation, through selection, and so on). In this process the intellectuals are not the powers: they are the agents of functions created and recognized in the wider world community.

Bendix's book requires a rewriting around some such theory. As it stands, it will receive attention for the enormity of its effort, but despite the power of its main empirical points its argument remains sadly reactive and defensive. Its theoretical weakness may even enable others to dismiss these empirical points, although the support of the historical record is great: Collective entities and their authority systems often have enormous persistence beyond the constellations of interests that help construct them. And in the modern world-system, collective forms and their embedded authority systems flow much more rapidly from society to society than our present accounts suggest or explain.

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Book Reviews

The Development of the Modern State: A Sociological Introduction. By Gianfranco Poggi. Stanford, Calif.: Stanford University Press, 1978. Pp. xiii+175. \$10.95 (cloth); \$3.95 (paper).

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The Development of the Modern State is an outstanding example of an all-too-rare genre: a brief historical and theoretical synthesis, rich in ideas and not readily categorized under any particular theoretical heading. Gianfranco Poggi weaves materials from a variety of different theoretical traditions into an account of the development of the modern state that has something to contribute to all of the major schools of historical and comparative sociology.

The book falls into two parts. The first develops a historical typology for analyzing the development of the modern state from feudalism to Ständestaat to absolutism. The account focuses on France, Germany, and Italy, and the author draws heavily on Continental scholarship in filling in his argument. In the chapter on the Ständestaat, "the polity of the Estates," Poggi sees himself correcting recent scholarship, such as the work of Perry Anderson, by stressing that there was a distinct system of rule between feudalism and absolutism.

Poggi argues that the emergence of the Ständestaat was closely linked to the growth of towns after A.D. 1000. The towns were clearly part of the feudal system in that their "internal economy presupposed, and was inscribed within, a wider division of labor between town and countryside" (p. 41). Yet the more powerful towns collectively had an interest in a wider and more uniform context of rule in order to facilitate trade. This collective interest of the towns was reflected in the creation of late medieval assemblies—the Stände—in which urban groups as well as the nobility and the clergy were represented. While the clergy and the nobility had formal precedence in such institutions, Poggi insists that the emergence of these assemblies marked a fundamental shift from feudal forms.

Poggi goes on to provide an intriguing analysis of the three-sided relationship among ruler, urban interests, and the nobility which characterized the Stände. While discussing the crisscrossing alignments as nobility and urban interests joined to defend the prerogatives of the Estates against the crown, he stresses that urban interests were generally allied with the crown against the nobility. This alliance provided the crown with the financial resources necessary for the centralization of power that cul-

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minated in absolutism. Yet it was the rivalry among states that created the impetus for centralization. For Poggi, the emergence of the centralized state and the emergence of the competitive state system cannot be separated.

The chapter on absolutism recounts the familiar story of the rise of bureaucratic state administration. But Poggi also seeks to explain the apparent paradox of the collapse of resistance to absolutist centralization, particularly by the cities, and the later assertion of opposition to the *ancien régime* by the national bourgeoisie. Why did the cities make their peace with absolutism at one moment and then emerge as a source of revolutionary opposition at another? Poggi's solution to this puzzle is not adequate, since he relies too heavily on Habermas's discussion of the emergence of a public sphere without sufficiently addressing the social and economic crises of the *ancien régime*.

The second part of the book consists of a chapter on the 19th-century constitutional state and a chapter on state and society in the 20th century. The 19th-century chapter is the least satisfying of the book. Poggi shifts his focus from the history of political institutions to an emphasis on constitutional law. Instead of a discussion of the major forces shaping the state, we are presented with a series of disjointed, though often provocative, statements about legitimacy, the nature of politics, and the modernity of the state.

But the final chapter more than compensates for these weaknesses. Here Poggi presents a compact discussion of the problems of the modern state. He carefully analyzes the forces that have eroded the state-society boundary in the 20th century, locating those that are rooted in capitalist development and those that are rooted in the state itself. His vision is of an increasingly powerful state dominating society, but a state decreasingly effective in societal and economic management. While this is hardly a unique vision, it is compelling because it develops out of Poggi's historical account. He ends the book with a call for a return to the classic question of the liberal and democratic tradition, How can the state be controlled by society? He insists, as many neo-Marxists now acknowledge, that the socialist tradition has not addressed this problem adequately because of Marxism's tendency to see state power simply as an outgrowth of class power.

As one expects in a book subtitled "A Sociological Introduction," Poggi leaves many questions unanswered. He is often vague in his arguments about historical transitions. At his best, he analyzes changes in the state in the context of social and economic transformations, but he is not consistent in doing this. Nevertheless, this is a valuable work and a model effort to synthesize arguments from diverse theoretical traditions.

George Herbert Mead: Self, Language, and the World. By David L. Miller. Chicago: University of Chicago Press, 1980. Pp. xxxviii+280. \$7.95 (paper).

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While David Miller is an eminent philosopher in his own right, he also ranks as the foremost expositor of George Herbert Mead's scholarly views. His book, *George Herbert Mead: Self, Language, and the World*, is unquestionably the most thorough presentation of Mead's philosophical thought in the literature. Previously unreviewed in the *American Journal of Sociology* (it was originally published in 1973 by the University of Texas Press), the book deserves to be brought to the attention of students in sociology.

Miller has given us an excellent treatment of the central lines of Mead's thought. An early chapter entitled "Mead's Intentions and His Basic Terms" is an admirable account of the intellectual units used by Mead to form his philosophical thought—units such as "events," "passage," "process," "the present," "novelty," and "sociality." Subsequently, excellent chapters are devoted to Mead's theory of the self, his analysis of language and meaning, his strikingly original analysis of the "physical object" with its profound implications in giving rise to a distinctly human world, his incisive treatment of perception and thinking, his unusual analysis of the "present," putting the "past" and the "future" into a new perspective, and his development of the idea of "sociality" as ordering all systems in the universe. Miller's presentation of Mead's views on these matters is comprehensive, carefully developed, and remarkably simple and clear. Scholars who wish to understand the foundations of Mead's sociological and social-psychological thought should definitely read Miller's book.

The great value of the book for sociologists and social psychologists is to enable us to identify the analytical scheme of human society developed by Mead. This scheme is markedly unique. Mead had to develop it to meet fundamental considerations that did not confront the general run of social scientists and thinkers. It is well to remember that he was primarily a philosopher. He differed from the bulk of philosophers in believing that the cardinal problems of philosophy arose in the realm of human group life and not in a separate realm of an individual thinker and his universe. For Mead, such basic categories as existence, reality, mind, matter, perception, action, time, causation, change, and creation had to be seen and addressed as social formations. In order to develop this approach it was necessary for him to identify the basic nature of human society that gave rise to human thought. He could not merely turn to the schemes of human society that had been worked out by social scientists or previous thinkers; their schemes were grounded, even though unwittingly, in the

very philosophical premises that Mead found it necessary to question. Thus, he was forced to work out his own analytical picture. In the picture he came to develop, the core ingredients may be said to be: (1) that group life consists of "social" or joint acts, (2) that the formation of social acts in human societies is possible only because human beings are able to develop and use "significant symbols," (3) that the use of significant symbols introduces a new form of interaction between human beings, a form that we speak of today as "symbolic interaction," (4) that the human organism develops a "self" which allows it to engage in self-interaction as it interacts with others, and (5) that social interaction in human society is an emergent or creative social process—a form of "sociality."

This fivefold set of basic constituents of human society represents a view that is strikingly different from the ones dominant today in the social and psychological sciences. Let me point out here the more important differences.

In the case of the "social act," Mead gives us a picture of human group life that exists in the form of continuous ongoing activity. Human society is seen in constant formation, in which the activities of the participants are being formed, maintained, interjoined, broken down, and eliminated. This process is just not caught by the customary sociological concepts of culture, structure, values, norms, status positions, social roles, or institutions.

The "significant symbol" is the key element in Mead's social thought. It is the idea that the human being may respond to his own gesture as others in his group respond to it. This common or shared response enables human beings (1) to develop "meanings" and hence to create and live in a world of objects, (2) to anticipate the behavior of others and hence to construct acts in the light of that anticipation, (3) to carry in their own makeup the behavior of others, and (4) to form "selves" which enable the construction of action by taking account of situations. Typical schemes of human society do not even touch on these matters, much less explain them. In short, they fail ignominiously to explain how human beings can act jointly, even though acting jointly is the basic feature of human societies. Mead's idea of the significant symbol is a penetrating attempt to give us such an explanation.

Mead went much further than any other scholar in analyzing the nature of human social interaction. He observed that such interaction may take place through a "conversation of gestures" (as in the case of subhuman species) or through the use of significant symbols. In the latter case the participants approach and respond to one another indirectly, that is, through an interpretive process. This interpretive process makes interaction a matter of formative adjustment instead of a mere release of already formed patterns of response. Prevailing schemes of society do not catch this interpretive character of interaction.

Mead identified the "self" as an object which the human being makes of himself, thus allowing the human being to interact socially with himself.

This self-interaction takes place concurrently with his interaction with others and enters into such interaction as a vital part of the social process that makes up human group life. Very few schemes of human society make a place for self-interaction in the case of the participants, and those that do rarely see such self-interaction as a social process through which the individual is able to adjust his behavior to others.

Mead saw the social process in human society as creative, in the sense of giving rise to novel formations as people adjust to one another in forming their joint acts. This creative or emergent character of human group life, represented by his idea of "sociality," is almost completely absent from the schemes of human society in the social and psychological sciences. The view that social interaction is a continuing source of novelty and change is a much overlooked part of Mead's thought, a part with which he was concerned a great deal in the later period of his life.

As the foregoing comparisons suggest, Mead's scheme calls for a significantly different form of study and analysis of human society. It presents a different picture of human beings as actors, of how they interact, of how they piece together their acts, of how the individual act enters into the social or joint act, and of how social acts come to change. To study human society with this scheme raises different questions, poses new problems, requires a different line of inquiry, and points to a different form of analysis. With a sense of this rather great shift that is posed by Mead's scheme, I wish to refer back to Miller's book. As I have said, Miller has done an excellent job of explaining how Mead saw the social act, the significant symbol, symbolic interaction, the self, and sociality. Yet it is unfortunate that he was not led to take the next step—to identify and discuss the important questions raised by these five fundamental parts of Mead's picture of human society. I am not referring to the highly important question of what methods to employ in the empirical study of social acts, the significant symbol, symbolic interaction, self-interaction, and sociality; Mead and Miller say nothing about how to study these matters empirically. Instead I refer to the consideration of very basic theoretical questions which are not answered in Mead's discussions. For example, what is given and what is withheld by participants in forming social acts? What is the nature of the "response" that is common to a significant symbol? What is the nonsymbolic base for symbolic interaction? What are the forms taken by the "generalized other" in self-interaction? And which areas of human group life are susceptible to "emergence" in the play of sociality? These are only a few of many vital theoretical questions dimly raised by Mead's scheme but left untouched.

We must conclude that Mead's scheme for the analysis of human society is far from being complete. Indeed, his brilliant insights in the case of the five components are merely initial openings that need to be clarified, sharpened, elaborated, and added to. The drawback to Miller's otherwise excellent work is the absence of effort to engage in this necessary development.

Self and Social Context. By Ray Holland. New York: St. Martin's Press, 1977. Pp. x+303. \$21.95.

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In *Self and Social Context*, Ray Holland promises his readers nothing less than a critical documentation of the interdisciplinary failure of the social or, as he prefers to call them, human sciences and an explanation for the serious gap between psychology and sociology. An imaginative and provocative review of the study of roles serves the author's first intention. Through a critique of seemingly innumerable role theorists, including Goffman, Kelly, Levinson, Mead, and Merton, as well as philosophers he finds relevant such as Emmet, Macmurray, and Mayo, the author argues that the human sciences have worked independently of each other, each science operating with an idiosyncratic view of what roles are and holding only a stereotyped and distorted view of the others' definitions. The result has been a limited theory of roles which stands in the way of an adequate understanding of how individuals operate within social contexts. Holland's second intention, to explain the gap between disciplines, as well as the gap within disciplines which has resulted (e.g., the conceptual and research boundaries between personality and social psychology), is hard to accomplish. According to the author, only a critical analysis which combines close textual criticism, psychoanalytic probing, and a social critique modeled after the work on the sociology of knowledge of thinkers such as Ben-David and Shils can explain the current rigid state of the human sciences and point the way to Holland's ideal of transdisciplinary human science.

Although Holland has set no small task for himself, he claims to discover an explanation in the form of three characteristic errors plaguing the human sciences: a failure to be reflexive, superficial or distorted readings of basic texts (e.g., Marx's works), and false polarization in theorists' presentations of their own works and in their understandings of others' contributions. He singles out American personality theorists as prime perpetrators of the three errors, but he implicates most of the major social scientists of the past century. Examples of such error, as Holland cites them within American personality theory, will serve to clarify his charges. According to Holland, it is because they have failed to be reflexive about their work that personality theorists have not recognized the extent to which their own self-structures influence their supposedly objective and systematic theorizing. One of the most dire consequences of the unnoticed influence is the prevalence of a characteristic American bias, namely, "positive generality" within American "personology." Carl Rogers, for example, with all of his Midwestern enthusiasm, is attracted to existentialists' discussions of such issues as existential living and openness, but he

wrings existential philosophy dry of any sense of pessimism, despair, and the inevitability of human struggle.

Holland's charge of misreading is sharpest when he takes personality theorists to task for distorting the writings of Freud, the predecessor the Americans are so intent on reacting against. Their misreading, according to Holland, blinds them to the extent to which they are still dependent upon Freud. Harry Sullivan, for example, dismisses what he sees as Freud's overly aggressive and negative assumptions about human nature, but he also defines his own central concept of security operations as little more than orthodox defense mechanisms. Holland sees the effects of false polarization in Kelly's discussion of role, which completely submerges role's social aspect. Kelly, like all of the role theorists considered by Holland, polarizes self and society in such a way that the social is left an excessively unitary or undifferentiated concept and the individual an unduly autonomous or indeterminate one.

In unraveling his elaborate analysis, Holland offers personal reflections on over 70 major and minor human science thinkers. Along the way, he makes some contributions (e.g., his critique of Kelly as a reluctant existentialist) and offers provocative insights (e.g., his pairing of Kelly's personal constructs with the binary forms of Lévi-Strauss). He does not, however, perform the impossible feat which he promised. His retelling of Allport's story about his meeting with Freud, for example, does not qualify as the psychoanalysis which Holland deems essential for a thorough explication of the currently inadequate state of human science.

The author's own method of criticism reveals the most serious flaws in his book (I have taken his points about the value of reflexivity seriously). While hoping not to commit the *ad hominem* error which Holland often verges on, one might question the extent to which he has allowed his own self-structure to invade his work. With a very uncharacteristic lack of skepticism and criticality, Holland portrays the work of Louis Althusser, Jacques Lacan, and Anthony Wilden as the hope for a better future for the human sciences. I had to wonder about some aspects of this portrayal: How, for example, can Holland so easily dismiss Maslow's "mystification" and, less than 200 pages later, embrace the undeniably esoteric and barely penetrable Lacan? Holland takes great pains to emphasize not only the value but also the radical nature of the work of Althusser, Lacan, and Wilden. For him they are thinkers out of the mainstream. Using Holland's notion of reflexivity, one has to ask how much his attachment to these radical thinkers has to do with his own self-perception as marginal and radical. He clearly expresses the latter in his discussion of his position in a college more committed to social administration than to transdisciplinary human science.

Holland also appears guilty of the errors of misreading and polarization. He criticizes others for not closely reading the texts they are rejecting or elaborating. He himself, however, relies almost exclusively on a secondary source, Wilden, for the portrayal of Lacan which forms a crucial part of his text. He criticizes such thinkers as Gerth and Mills for polarizing Marx

so that they can argue for the distinctiveness of their work over against his. But it is this very same technique of contrast with another's work that Holland uses when he finally, in the last few pages of his text, presents his own form of transdisciplinary science.

In spite of its flaws, Holland's book deserves to be read by personality and social psychologists, sociologists, anthropologists, and philosophers of science. The book may not provide the transdisciplinary solution for social science, but it effectively illustrates some serious consequences of not having one. The book is also noteworthy for its general tone. Holland expresses a sincere enthusiasm and concern for our shared subject matter. His energy should be contagious.

Conceiving the Self. By Morris Rosenberg. New York: Basic Books, 1979. Pp. xvi+319. \$16.95.

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For more than 20 years Morris Rosenberg has studied the "self-concept," which he defines broadly as "the totality of the individual's thoughts and feelings with reference to himself as an object" (p. 8). His previous books on this topic (*Society and the Adolescent Self-Image* [Princeton, N.J.: Princeton University Press, 1965]; *Black and White Self-Esteem*, with Roberta Simmons [Washington, D.C.: American Sociological Association, 1972]) can be identified by their titles as sociological works, and their primary value is as works of empirical sociology. By applying, with care and intelligence, the methods of survey research to the study of certain highly delimited aspects of this self-concept, Rosenberg has contributed much to our understanding of how the social environment impinges on these circumscribed aspects of the self. It is a tribute to Rosenberg's empirical studies that reading them evokes a sense of discovery, of perplexities being confronted and unraveled, of more general principles emerging and being usefully applied; but this sense of discovery is always in the realm of social context and its effects, not in the realm of the self.

As probes into this realm of the self, survey research items like those that constitute Rosenberg's scale of "global self-esteem" ("On the whole, I am satisfied with myself," "At times I think I am no good at all," etc.) are fundamentally limited. In *Society and the Adolescent Self-Image* Rosenberg recognized the limitations of this research, that "the surface has barely been scratched," and stated the difficulty of going beyond the surface about as concisely as it can be put: "The phenomenon is not easily amenable to experimental manipulation, and the problem of validating something as quintessentially phenomenological is difficult in the extreme" (p. 272). Yet from Ruth Wylie's most recent assessment of self-concept measures (*The Self-Concept: Revised Edition* [Lincoln: University of

Nebraska Press, 1974]) we know that, limited as they may be, the items in Rosenberg's scale are, in terms of demonstrable reliability, validity, and scientific usefulness, about as good as any measure of the self-concept currently available. This does not create a bullish feeling in those of us concerned with enhancing our understanding of the self through quantitative research.

Given this state of affairs and the fact that Rosenberg is an empirically oriented sociologist, we are bound to be disappointed if we expect *Conceiving the Self* to have, as its dust jacket suggests, "vastly deepened and enhanced our understanding of the human personality." It does not. What it does, and this is recommendation enough, is present the current status of a long-range research program that has contributed much to our understanding of the interplay of social context and certain quantifiable aspects of the self-concept and that is now evolving in promising new directions.

The book consists of four parts: (1) a wide-ranging theoretical discussion of the self-concept; (2) a reworking, with some expansions and shifts of interpretive emphasis, of previously published material on social determinants of the self-concept; (3)—and here is where the promising new directions emerge—a discussion of research exploring developmental trends in self-concept formation; and (4) a section entitled "Beyond Self-Esteem," a discussion of future directions for self-concept research.

The theoretical section stresses four major principles of self-concept formation—"reflected appraisals," "social comparisons," "self-attribution," and "psychological centrality"—which underlie Rosenberg's "contextual" approach to the self-concept. These principles serve Rosenberg well in his discussions of social determinants of the self-concept and are even quite useful in understanding some longitudinal data on self-concept disturbance in early adolescence, but there is an important sense in which certain of his most interesting developmental findings have outpaced his current theoretical stance.

In a chapter entitled "Exterior and Interior Self-Concept Components" Rosenberg describes clear age trends in the categories by which children "spontaneously" conceive the self in response to open-ended questions. A very general description of these age trends—that young children tend to conceive the self in terms of external qualities, while older children do so in terms of internal qualities—does not do justice to the more subtle qualitative changes which Rosenberg detects in the data. These changes are clearly a manifestation of the phenomenon described by Jane Loevinger and Ruth Wessler in their analysis of responses to incomplete sentence stems (*Measuring Ego Development* [San Francisco: Jossey-Bass, 1970]). Loevinger draws extensively on psychoanalytic and cognitive theories of development to derive a clearly articulated sequence of developmental stages, "ego development," which she contends underlies this phenomenon (*Ego Development* [San Francisco: Jossey-Bass, 1977]). The current direction of Rosenberg's empirical work demands an infusion of such theorizing and a little greater distance from the "contextual" approach which

has served him so well in the past. Harry Stack Sullivan had much to say about the kind of developmental process which Rosenberg's empirical work is beginning to touch on, but Rosenberg introduces Sullivan, in a nondevelopmental discussion of "reflected appraisals," only as the progenitor of the term "significant other."

But that this criticism can be made at all is a sign of one of the book's greatest rewards to the reader, that it is a document in the natural history of an evolving research enterprise. It is unusual for a social scientist to pursue one topic as productively, and for so long a time, as Morris Rosenberg has pursued the self-concept. In his work one can observe the interplay of theory and empirical discovery over time, see where he has been and where he seems to be going, and learn something far more useful about the process of social science than is ever captured in formal expositions on the appropriate relationship of theory to empirical research.

Social Science in America: The First Two Hundred Years. Edited by Charles M. Bonjean, Louis Schneider, and Robert L. Lineberry. Austin: University of Texas Press, 1976. Pp. 221. \$3.95 (paper).

Buford Rhea

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As their contribution to the national Bicentennial, the editors of *Social Science Quarterly* asked six scholars from as many social sciences to evaluate the part American social science has played in shaping the national experience, in helping us to understand that experience, and in enlarging the social science disciplines as a whole. The result was a special issue of the journal, reprinted as *Social Science in America*, edited by Charles Bonjean, Louis Schneider, and Robert Lineberry, which fails to achieve the goals the editors set, but which is a welcome addition to the literature nonetheless—partly because the editors set those unachieved goals in the first place.

The nominal failure of the book is easy to explain: the editorial objectives call for historical review and synthesis, but only one of the contributors is a historian, so only one of the chapters—his—successfully melds disciplinary and national history. William Goetzmann's paper is a little gem, and his success where others fail serves to make a familiar point: if you want good history, get a professional historian to write it. In point of fact, only two other contributors even tried to meet the Bicentennial objectives, Joseph Spengler (economics) and Robin Williams (sociology), but the first merely sketches some parallel histories loosely coupled, while Williams, who adheres to the guidelines more strictly than most, is obviously uncomfortable with his format. Yet all of the chapters are excellent, each in its own way, and I suspect that they are so uniformly good precisely because the editors stimulated the authors to think in historical

perspective. The book, then, should be approached as a series of state-of-the-discipline reports, not as a set of historical syntheses, but those reports have been greatly enriched and made much more accessible to laymen by the historical approach the writers have adopted.

Spengler, for example, although his historiography is of the "just-one-damned-thing-after-another" variety, generates a panoramic view of American economics and the economy, and this framework in turn helps the noneconomist to organize the scores of names, most only vaguely familiar, that constitute so much of the history of the discipline. Spengler is also delightfully opinionated—he seems to feel that it is not classical economics that has failed the people, but the people who have failed economics (by being irrational, political, etc.)—and the broad historical perspective rescues his obiter dicta from the appearance of mere crotchettiness.

In its historical portions, Robin Williams's chapter will strike most sociologists as fairly routine (nonsociologists will find them informative), but Williams is probably incapable of writing an unintelligent piece, and his chapter is thickly laced with arresting observations. The claim that the problem of "externalities" and the problem of the "free rider" will be strategic sociological issues for the remainder of the century, for example, seems to me to be particularly worth considering.

The remaining three solicited papers pay only lip service to the Bicentennial theme, but the influence of the historical perspective is still apparent. Heinz Eulau bluntly announces that he is going to limit his discussion to the outstanding books of political science since World War II, and he does just that in what turns out to be a first-rate essay review. Walter Goldschmidt provides a short history of anthropology with only passing reference to national history, but it is an excellent disciplinary history, the best I have seen in so few pages. And Kevin Cox, writing about geography as a "social science emergent," gives a clear and well-organized sketch of a discipline that really has little or no history, at least as a social science, although the historical approach is again visible in his choice of the metaphor of emergence. Geography, incidentally, apparently claims social science status largely on the basis of adopting many of the quantitative tools of sociology ("Geography discovers the SPSS" is my marginal note), a development which may leave some readers less enthusiastic than Cox seems to be.

The volume is rounded out by a short introduction by one editor, Bonjean, and a longer summary conclusion by another, Schneider, correctly emphasizing the cross-disciplinary value of the preceding papers. Schneider ends the volume with what was for him a characteristic touch, that is, on a note of "robust good sense and irony" conveyed in a priceless quotation from Mandeville.

This book, then, is not a notable Bicentennial production, nor is it suitable for the classroom. It is, though, the sort of book sociologists will want to *read*, for it effectively communicates across the greatest barrier to social science knowledge, the boundaries between disciplines. It is modestly priced and adequately bound, at least by current standards, and I

recommend it as an opportunity to visit neighboring disciplines in the company of well-informed and articulate native guides.

Pride and Solace: The Functions and Limits of Political Theory. By Norman Jacobson. Berkeley and Los Angeles: University of California Press, 1978. Pp. xv+166. \$10.00.

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At first glance it is difficult to discern a connection between pride and solace, on the one hand, and political theory on the other. At last glance, after the book is read, the difficulty remains. The author has little to say about pride, but it appears that the history of political theory in the West, beginning with Plato, may be characterized as successive efforts to build "structures of solace," structures that have been more or less persuasive. These structures, however, began to be undermined with the dawn of the 19th—a disenchanted—century, and today we are faced with political theories that no longer offer solace. We are thus bereft. The work of George Orwell, Hannah Arendt, and Albert Camus convinces us that we confront a world without meaning.

Political theory begins when things—unspecified—become "unglued." To the Greeks, therefore, as Pascal observed, the ideal state is an instrument for the government of a madhouse. In *Pride and Solace* Norman Jacobson is not interested, except briefly, in the Greeks, but rather in Machiavelli, Hobbes, and Rousseau, to each of whom he devotes a chapter. Machiavelli designed not a madhouse but a menagerie, in which the trainer has to keep a close and often treacherous watch on his charges. For Hobbes it is not a zoo but a surgical facility, where the cripples are on display. And in Rousseau we have a citizen army waiting for a battle that never comes because the citizen soldiers absorb themselves in the General Will.

The chapter on Machiavelli is perhaps the best in the book. There have been so many interpretations of *The Prince* that it is difficult to say something new. The author regards it as political alchemy rather than political science and cannot contain his admiration. For it was the great Florentine's goal to travel in the footsteps of Moses, to save the city's pride, and somehow to find a key to the unification of Italy. As Jacobson says, "The god of Machiavelli is his divine vision of what Italy might one day become, his Tables the manual prepared under the direction of the muse of History, like the Tables of Moses shaped in the wasteland, in exile from his people" (pp. 31–32). But as soon as the prince succeeds he must be cast aside. His very qualities render him a threat to the stability of the state his genius creates. The first legitimate act of the new republic, therefore, will be the murder of its founder.

With respect to Hobbes, the author believes that the state of nature, the state in which man is the wolf among men and a predator upon his own kind, is not a temporary state that will disappear with the signing of the social contract. On the contrary, it is a perpetual state, "a metaphorical description of what moves us all." Competition and conflict are perennial and find their locus in what we "euphemistically call society." Hobbes must try to muffle all external expression of antisocial passions and also try to curb them from within. He accomplishes this by depriving them of a vocabulary. Hobbes himself is a poet. "He is forever placing us in peril by means of his poetry, then rescuing us in the nick of time by his science" (p. 67).

In his discussion of Rousseau, Jacobson quotes a sentence from the *Discourse on the Origin of Inequality* that Cooley must have read, namely, that man "seems to receive the consciousness of his own existence merely from the judgment of others concerning him." In the *Social Contract* Rousseau raises questions, especially in his confrontation of the antagonism between nature and culture, that our author confesses he cannot answer. No matter; Rousseau himself said that anyone who understood it was cleverer than he. In any event, Rousseau was "one of the last of the solacers."

In the final chapter, Arendt, Orwell, and Camus—obviously the author's culture heroes—arrive on the scene and give us political theory free of solace. Actually, however, each was "in the solace business himself," and merely to read them in the midst of the horror of our days is to be solaced.

I am not sure what to make of all this. It is difficult to tolerate the contradiction just mentioned. A political theorist is either a "solacer" or he is not. *Pride and Solace* is full of such words as horror (also just mentioned), disaster, collapse, abandoned, despair, calamities, tyranny, terror, cruelty, blasphemy and evils, all applied to we know not what. The modern state? The human condition? We are left in ignorance. The word "solace" means alleviation of grief or anxiety, but the author does not tell us what kind of grief or anxiety political theory is designed to cure. Would we be disconsolate if we were deprived of political theory? What balm does it provide? The book is difficult to read, not because its prose is dense but because it is fanciful. The author tries the reader's patience when he refers to Darwin and Freud as "incorrigible juveniles." One knows what pandering and chastity mean, but not "the lowest metaphysical and political pandering," and "the highest theoretical chastity." The book, in short, though not without insights, remains a puzzle. The rather odd notion that political theories are structures of solace is an insufficient reward for reading it.

Where Have All the Voters Gone? The Fracturing of America's Political Parties. By Everett Carl Ladd, Jr. New York: W. W. Norton & Co., 1978. Pp. xxiv+86. \$7.95 (cloth); \$3.95 (paper).

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The title of this book might more appropriately be "Where Have All the Parties Gone?" Everett C. Ladd sets up in the introduction, in a coherent and sensible manner, the necessary attributes of a "successful" party system, acknowledging at the same time the difficulty of achieving such ideals as a high level of representation or a completely nonrancorous process of political succession. The current positions (predicaments?) of both the Republican and the Democratic parties are described in the following two chapters; in the third chapter Ladd writes on the "perils of party reform" and tries to tie together many of the arguments that have had currency in recent years about the reasons for the supposed decline of the American political parties.

With regard to the Republican party, Ladd argues that despite the inevitable ups and downs of particular elections the GOP can be seen to be generally on the decline, showing a sustained inability to compete at levels other than the presidential level. He blames this on the narrow basis of the Republican party, exacerbated by the Goldwater nomination in 1964 and by the tendency of the upper middle class to attach itself increasingly to the "new establishment," which is, to a large extent, located in the Democratic party rather than in the GOP. (An example of this is the fact that Political Action Committees have, despite expectations, made substantial contributions to both Democratic and Republican political candidates.) The questions he raises about the Republican party—particularly whether it is going to be a "church" or a "coalition"—seem particularly salient and appropriate from the perspective of the 1980 election year.

Ladd refers to the Democrats as "the everyone party." By this he means that they show greater strength than their opposition in virtually every social category: regional, class, religious, ethnic, and so forth. In attempting to answer the question why the Democrats have not been particularly successful at the presidential level, Ladd is very good at presenting and simplifying much of the writing from recent years in the areas of political science and communications research. In particular, he stresses that voting for president has come to have less and less to do with party, as the mass media, particularly television, have taken over many of the communication functions previously performed by political parties.

It is also in the chapter on the Democratic party, however, that the book's weaknesses become obvious. One of these weaknesses is a certain carelessness in the use of survey data. For example, at the beginning of chapter 2, Ladd says, "Three-fifths of the populace profess to believe that 'quite a few' of the people who run the national government are at least

a little crooked" (p. 26). Even in a book written for an essentially non-professional readership, one should take care to specify the questions that elicited this response, the sample it refers to, when the survey in question was conducted, and so on.

The author also seems at times to ignore or explain away contradictory findings. For example, in the first chapter after a fairly long argument about the movement of the upper middle class away from the Republican party, he cites a 1976 *Fortune* survey of business executives in which 56% of the chief executive officers of the *Fortune* 500 called themselves Republicans. Only 6% regarded themselves as Democrats. Despite this apparent contradiction of his previous argument, Ladd says, "Top executives may still be Republican but they are no longer partisan" (p. 17). And in the second chapter, he asserts, "There is simply no base for an anti-New Deal party" (p. 29). According to Ladd's general argument, an anti-New Deal party would be one which objected to the ever-increasing levels of government spending on various social welfare programs. But if we look at the Gallup poll data which he presents, it is clear that in many cases a plurality or at least a substantial segment of the population favors keeping expenditures for various programs at the present level, reducing them, or ending them.

It is in his third chapter, entitled "The Perils of Party Reform," that Ladd makes his strongest argument. He briefly describes the political party reforms which have been instituted since 1968, beginning with the recommendations of the McGovern-Fraser commission. He argues that these reforms, in particular the rise of the presidential primary, have brought to a position of dominance in both parties an upper-middle-class elite who are far to the left (or right, in the case of the GOP) of the rank-and-file party members. More important, as Ladd sees it, these reforms have taken away the party leaders' abilities to plan the outcome of elections and election campaigns and have instead left us with what he terms "flea market politics."

Where Have All the Voters Gone? is interesting and easy for the non-social scientist to read. It condenses and simplifies much recent political science and other social science research, although it often does this at the risk of oversimplification or distortion of the data which it uses. Since the book was originally written in 1977, there are some arguments that now seem slightly "dated"—a measure of how much politics changes in only a few years. For example, Ladd relies heavily on the "two liberalisms" argument, similar to that advanced by Scammon and Wattenberg and others in the early 1970s. If we look at politics today, however, the "social issues" that formed the basis of the new liberalism have been eclipsed as important political issues by international events and economic trends. Similarly, Ladd refers to the fact that the Democrats are under criticism from the so-called old liberals (or new conservatives) on such issues as the damaging effects of too much regulation and an unresponsive bureaucracy. He further argues that this debate has not penetrated to the mass public. But the current economic situation seems to be stimulating much

more widespread thinking about these issues, and when we look at the political candidacy of such people as Jerry Brown and John Anderson, such thinking seems to have had some effect on the party system.

Despite such flaws, many of which are inevitable in a book written by a professional political scientist for nonprofessional readers, the book is an interesting and thought-provoking one.

Gandhi as a Political Strategist: With Essays on Ethics and Politics. By Gene Sharp. Boston: Porter Sargent Publishers, 1979. Pp. xxi+357. \$14.95 (cloth); \$7.95 (paper).

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A book by Gene Sharp, the foremost historian and analyst of nonviolence, about Gandhi, the best-known practitioner of strategic nonviolence in this century, commands immediate attention. No one is better qualified than Sharp to analyze Gandhi as a political strategist and to put his work in the context of 20th-century nonviolent political action in its totality. Had Sharp sat down to do such a review using the tools and concepts he has developed over the past 20 years, I am convinced we would have had a book of major importance. Instead, in *Gandhi as a Political Strategist* Sharp has reprinted essays written between 1959 and 1970 in a variety of contexts. They are somewhat repetitive and do not hang together well, focusing in turn on aspects of Gandhi's work and on ethical-political issues associated with nonviolence that were on Sharp's research agenda at the time. The first essay, a review of *Gandhi's Truth* by Erik Erikson (New York: Norton, 1969), is one of the best. It describes the various nonviolent movements active toward the end of the 19th century in Africa (including South Africa), Russia, China, Ireland, England, and India itself, thus documenting the largely unrecognized models available to the young Gandhi. Gandhi was not as much of an innovator as has been thought, Sharp suggests. Other essays restate various analytic issues regarding Gandhi's thought, including the question of his ambivalence regarding the nonviolence of the weak in contrast to the nonviolence of the strong. It was the latter which Gandhi regarded as the spiritually ideal nonviolence. Sharp's own insight that the nonviolence of the weak contains the strength of noncompliance was never clear to Gandhi, as these essays point out.

While Sharp includes a respectful review essay on Joan Bondurant's classic study of Gandhi, *The Conquest of Violence* (Princeton, N.J.: Princeton University Press, 1958), he does not himself provide the new systematic analysis of Gandhi's strategies in their political context that his title seems to promise. Referring the reader to part 3 of his own *Politics of Nonviolent Action* (Boston: Porter Sargent, 1973) only made me wish that Sharp had done that analysis in this book. Nor does he do a systematic

analysis of the evolution of Gandhi's thought over time, although he gives tantalizing suggestions for such an analysis and points out that others have neglected this evolutionary process. Much of this book, particularly part 2, "Essays on Ethics and Politics," is a polemic against the peace movement generally and Christian pacifism in particular. Sharp repeatedly complains that Gandhi has been quoted out of context and misunderstood by muddle-headed Christians who cannot distinguish between a "do-or-die" principle and a political strategy. While it is certainly true that many pacifists, along with many nonpacifists, have misunderstood Gandhi, Sharp's lumping together of all pacifists (a result, we learn on p. 251, of his own run-in with British Quakers and members of the Fellowship of Reconciliation in London in 1963) does not further the analysis of the relationship between ethics and politics. One can only agree with Sharp that more research is needed on this topic. Some of that research might well be directed to the peace movement itself, which is more diverse and sophisticated than Sharp recognizes. He cites only a very old pamphlet of A. J. Muste, a towering figure in the 20th-century peace movement and a major theoretician and practitioner of nonviolence. He does not refer to the publications of peace research historians such as Charles Chatfield on 20th-century peace movements. His statement that religious pacifists do not think strategically ignores the whole thrust of such developments as the Movement for a New Society, which includes religiously oriented strategists such as George Lakey.

The appendixes and bibliographies will be useful in college courses on Gandhi, as will the historical first chapter and the closing summary-and-evaluation chapter. The remaining essays discussing politics "versus" morality at length may be useful for undergraduates unfamiliar with these issues, but the systematic analysis of Gandhi's strategic thinking in the context of 20th-century currents of nationalism and social change movements has still to be written. I, for one, hope Sharp himself will write it.

No Man's Land: Combat and Identity in World War I. By Eric J. Leed. New York: Cambridge University Press, 1979. Pp. xii+257. \$14.95.

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To those who grew up under the shadow of the atomic bomb and later became involved in Vietnam—by now, the majority of adults—the symbolic reality of a "no man's land" probably has little meaning. Yet World War I was a shattering experience for the major European nations, particularly for those who for over four years manned the front-line trenches. The reality survived the end of the war. In this little book, slightly over 200 pages of tightly written text, Eric J. Leed, a historian,

has dissected the several aspects of the wartime experience in a highly illuminating way. *No Man's Land* is worth every bit of its price.

What Leed has written is not in any sense a conventional account of battles and campaigns, the relation between generals and civilian politicians, or even the casualties, destruction, and suffering caused by the 1914-18 holocaust. It represents instead a creative synthesis based on phenomenological, psychological, anthropological, sociological, and literary analysis. One cannot avoid being impressed by the wealth of intellectual perspectives the author brings to the question how the war changed the men who participated in it. He neither obscures nor oversimplifies but leaves the reader thinking. The many thoughts he provokes are difficult to summarize in a brief review.

The curtain raiser consists of an analysis of war as a generic category of experience. Neither the drive-discharge model, according to which combat affords an opportunity for the discharge of unconscious aggression, a temporary instinctual liberation, nor the cultural-patterning model, which stresses the continuity between civilian socialization and conduct on the battlefield, is adequate. Neither is anchored in the concrete historical experience of the First World War, which, implausible as it may seem today, was greeted with enough enthusiasm to bring forth a large number of volunteers. The point driven home by Leed is that for those who fought in it the war was a discontinuous and yet a communal experience, one that fundamentally transformed the participants in ways that the concept of disillusionment does not even approach.

Another theme is how the community of war meant an escape from "modernity." Its outbreak signaled the destruction of an economic order built on industrialization and rational management. This was especially true of Germany, where the crisis of values had been most keenly felt and where, as Veblen has already pointed out, modern technology coexisted in an uneasy alliance with preindustrial and archaic habits of mind. At the front, writes Leed, "the cultural polarities become redefined . . . [into] an antithesis of 'home' and 'front.' The antithesis of a technological and human world is mapped upon the landscape of war to define the tension between the 'external' world of physical threatening forces and an 'internal' world of mechanical solidarity and fantasy" (p. 72).

Of special interest to me is Leed's treatment of war neurosis. Here, as elsewhere, he exhibits a fine feel for the paradoxical or, if you like, the dialectical aspects of his subject. The official acknowledgment of neurosis as behavior "appropriate" for combatants was at one and the same time an organizational problem and a safety valve. If, on the one hand, these neuroses were the "direct product of an increasingly alienated relationship of the combatant to the modes of destruction" (p. 164), it was better, on the other, to "have the power of command evaded through an individualized symptom than challenged directly by mutiny" (p. 168). The neurotic soldier was caught in the middle, between his ideals and moral imperatives and his natural instinct to survive. He was unable to make a

decision, and it was the job of the psychiatrist, if he was to effect a "cure," to refit the patient for his military role.

The last chapter is devoted to the returning veterans and their role in society. The idea or, better, the utility of war as an escape, similar to a ritualistic transformation, was destroyed by the *Materialschlecht* that pitted human flesh against the machine. Soldiers became proletarianized in the most literal sense. The one positive value of the experience was the communal bond. The comradeship of the trenches stood in sharp contrast to the "society" (in Tönnies's sense) of home where things had not changed. But proletarianization did not produce political socialists. The survivors from the trenches were unable to link their war experience to general social concerns. The industrialization of war led them to withdraw into a narrow circle of comrades. For many the war never ended. They became homeless men, fixated on the dead as the central figures in a cult of suffering and self-sacrifice.

This sampling may not convey the full flavor of the book, richly seasoned as it is with the most intriguing ideas. It is the product of a cultivated imagination, the kind of thing that one misses in the many prosaic studies that have become the staple of our discipline. It also gives us a scale against which to measure the "something new in history" so many have come to associate with Vietnam.

Primordial Characters. By Rodney Needham. Charlottesville: University Press of Virginia, 1978. Pp. x+89. \$7.95.

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After a quarter-century of productive scholarship devoted to the analysis of ethnographic data with special attention to marriage systems and symbolism, Rodney Needham has turned in the past few years to reflective statements on the nature of the human experience and the character of social anthropology as one of its exponents. A slim, handsomely produced volume, *Primordial Characters* presents in "virtually unchanged" form a series of lectures in this vein delivered at the University of Virginia in September of 1977. The author makes it clear in his prologue that he took some delight in the physical and intellectual setting during his visit. From the contents of the book it can be assumed that those who attended the addresses must have been equally impressed by the intellectual climate which graced their community at that time.

Needham begins his discourse with the assertion that a scholarly discipline "must be capable of saying something about the grand themes of consciousness, will, freedom, and the true character of man" (p. 4). He proposes to accommodate this self-imposed charge by discussing three topics—"primary factors," "synthetic images," and "evasive fantasies"—

which he takes to be basic features of the human intellect. Needham takes up these issues because he believes that social anthropology's mistaken concern for generalities, laws, institutions, structures, systems, collective practices, and ideologies is far removed from the study of human nature, which has been neglected on the assumption that it is well understood. He argues, however, that this is not the case, nor will it be until we more fully appreciate "the capacities, proclivities, and constraints" of the intellect (p. 8).

After these brief prefatory remarks, Needham considers "primary factors of experience," which are defined as "elementary constituents of culture" (p. 8). From among the potential list of primary factors he identifies the linguistic resort to physical contrasts and material properties such as hot and cold, soft and hard in making value judgments; the reliance on white, black, and red in making symbolic statements; the significance of certain numbers in marking social distinctions; the emphasis on percussive sound to indicate social transition or categorical mediation; lateral symbolism and oppositions; and, finally, in the realm of social organization, the limited number of logical possibilities in the formation of descent systems. These are offered as only a few examples of a yet-undetermined number of such phenomena which nonetheless demonstrate the pervasiveness of factors constraining the intellect.

In the essay on synthetic images, Needham revives the classical anthropological topic of witchcraft. After a masterly review of the ethnographic and topical literature, which succinctly elucidates many of the problems and unresolved issues, Needham chooses to concentrate on the characteristic features of this belief complex. He suggests, on the basis of "sporadic resemblances," that there is a common image of the witch. Its features involve moral opposition, physical inversion of standard human expectations, and a variety of other factors associated with darkness. This synthesis permits a polythetic representation when ideas from disparate parts of the world are considered. For Needham, these clues again point to properties of the brain which are the consequence of a constricted rather than unfettered imaginative capacity.

The final section again deals with a familiar topic, myth, which is poetically characterized as composed of "evasive fantasies." As before, Needham points to the attending problems, including the absence of a satisfactory mode of analysis, due primarily to the fact that the word "myth" does not refer to a monothetic category. More positively, he remarks that with a concern for properties rather than types, it is possible to discern an invariant feature of these narratives. He identifies this quality as "the abrogation of constraint—in form, space, time, personality, logic, and incidental attributes" (p. 59). He also takes this to be a feature of the imagination in general, so that this proclivity to break normal constraints is displayed in many other contexts, including art forms, dreams, and the popular conception of religious and political leaders.

I have taken some pains to summarize Needham's thoughts, for, despite its brevity and lucidity, his argument is disarmingly complex and pro-

found. In some instances greater clarification would have been helpful. A further elucidation of his distinct mode of analysis and ultimate intent, as distinguished from structuralism, which he disavows, is a case in point, for there are many obvious parallels. Explicit references to those with whom he finds fault on theoretical grounds would also be helpful signposts to those not familiar with contemporary arguments in the discipline.

Yet these are only minor objections to the author's economic expository style, which is refreshing and far more palatable than the typical meanderings of many massive tomes. Those who would rashly assume greater faults because of the elegance and clarity of the argument will be sorely mistaken in their estimation. However, serious responses by equally thoughtful advocates of other schools of thought are to be expected, for Needham's work is a provocative call to reconsider the basic subject matter and orientation of social anthropology.

Exiles and Migrants in Oceania. Edited by Michael D. Lieber. ASAO Monograph no. 5. Honolulu: University Press of Hawaii, 1978. Pp. xii+417. \$17.50.

Land Tenure in Oceania. Edited by Henry P. Lundsgaarde. ASAO Monograph no. 2. Honolulu: University Press of Hawaii, 1974. Pp. x+288. \$12.00.

Mission, Church, and Sect in Oceania. Edited by James A. Boutilier, Daniel T. Hughes, and Sharon W. Tiffany. ASAO Monograph no. 6. Ann Arbor: University of Michigan Press, 1978. Pp. xiv+500. \$33.50.

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The Pacific has sheltered many illusions; not the least of these is a view of island societies as timeless social wholes. Over 40 years ago, this view provided a working basis for classic texts such as *We, the Tikopia*. It encouraged the hope that the Pacific islands would provide a laboratory of ecological and cultural variation in which social theory could be tested. However, advances in anthropological theory have come from confrontations with other societies, notably ones in Africa and South Asia. Meanwhile, ethnographers continue to work in the Pacific, and their work shows that Pacific cultures cannot be contained in the petri dishes of the laboratory.

Pacific peoples are not the native bearers of unchanging traditions, nor are they constrained by invariant ecological parameters. The books under review offer detailed testimony of cultural adaptation—but of adaptation to a world larger than the isolated atoll. Pacific peoples are mobile and

missionized. At the edge of the world system, they deal with legal systems imposed or crystallized by colonial powers, and they trade in a world economy. Yet their very distance from the center makes thoroughgoing dependency impossible, and in every domain of life islanders have negotiated their own history, if only as part-societies.

Ethnographers have tried to document social change in the Pacific, but the effort has borne little theoretical fruit. The books published by the Association for Social Anthropology in Oceania, written largely by younger fieldworkers, offer a useful alternative to the usual monograph, for the case studies in each volume show not only social change but also the range of variation found in Pacific adaptations.

Each volume focuses on a topic which is broadly defined and discussed in introductory chapters. Next, in a series of specialized ethnographic analyses, attention is paid both to description of the topic and to its importance for the social organization and culture of particular island populations. A trade-off between comparative and particularistic concerns is made, often in favor of a thorough account of the local situation. Consequently, the concluding comments of each volume are apt to be tentative. This is all to the good, if an understanding of social structure and process is wanted, rather than the last word on, say, land allocation in small territories.

Recurrently, the authors show that social organization and process are culturally structured. This truism takes on power from the variety of cases in which it is employed. Thus people from the Polynesian atoll societies of Nukuoro and Kapingamarangi form distinctive emigrant populations under identical circumstances. As Michael Lieber concludes, people from both atolls "have replicated their cultures on Ponape; because of this, the Nukuoro community falls apart whereas the Kapinga community does not" (*Exiles and Migrants in Oceania*, hereafter *Exiles*, p. 355).

This comparison and a study of four emigrant Rotuman populations on Fiji (*Exiles*, chap. 7) are notable for their success in dealing with a complex colonial history without falling into the trap of being merely local chronicles. These accounts of ethnic interaction and community solidarity, or its absence, are welcome in a field in which comparative studies have largely been ahistoric exercises in model building.

A few studies in the volumes deal with isolated, "test-tube" cases in which a delimited input from outside a local system is succeeded by a series of organizational and ideological developments. In these studies the authors find, not homeostasis, but strivings for cultural order that offer only momentary equilibria in a context of demographic, economic, and social change.

Ron Crocombe, for instance, shows how the descendants of William Marsters on Palmerston Atoll in the Cook Islands evolved multiple principles of property rights and limitations on travel and exploitation in order to uphold their egalitarian values as the population increased and social divisions emerged (*Land Tenure in Oceania*, edited by Henry Lundsgaarde, chap. 8). Similarly, Peter Black's study of Catholicism on Tobi Atoll in

the Western Carolines shows that Tobians' wholesale conversion and continuing orthodoxy respond to an ideological imperative. Christianity offered Tobians "an escape from the paradox of formal secularism without a concomitant secularization of worldview" (*Mission, Church, and Sect in Oceania*, edited by James Boutillier, Daniel Hughes, and Sharon Tiffany, hereafter *Mission*, p. 352). As Black notes, the systematic quality of Tobians' synthesis of Christianity has set the parameters of future social problems. Eric Schwimmer's study of the relocation of Orokaiva people of Papua New Guinea after a volcanic eruption (*Exiles*, chap. 11) is notable for its detailed account of local beliefs and speculation concerning the eruption. Schwimmer deals with competing explanations of the disaster, finding their shared roots in older ideologies and their disjunctive assessments of the colonial situation. This study is not only of interest for mapping the diversity of Orokaiva views; it also offers a French structuralist account of the conditions for the emergence of competing views of a historic event and the dominance of some of them. Schwimmer's chapter is of special interest as a test case of the methodology on which he explicitly depends.

"Test-tube" cases are the exception, not the rule, and most of the studies in these volumes discuss changing contexts and processes. Accounts of migrant communities show the failure of migrants' expectations as much as they do the replication of cultural forms. Studies of Christianity in the Pacific deal with competition between churches, sects, and, in Melanesia, cargo movements. The *Mission* book offers suggestive portraits of the missionary and the place of competition among churches in Oceanic societies. Kenelm Burridge develops an ideal-typical view of the missionary as the exemplar of individualism (chap. 1). Filling out the picture, Sione Lātukefu gives an indication of the social role of the Oceanic missionaries, who, in much of the Pacific, had more extensive contact with local congregations than with their European overseers (chap. 4). In contrast to these studies, accounts of denominations in New Britain (chap. 13), Tonga (chap. 14), and Samoa (chap. 15) show proliferation to be possible as local political cultures support the maintenance of choices among traditions. Messages which missionaries saw as universal have become accepted as part of local identities, as integral to "the Samoan way" and the like. In this process, the individualism of the missionary or pastor may become routinized as sanctioned deviance. It is little wonder, then, that Marists in the Pacific report themselves and their colleagues to be alienated from both theological trends and local cultures (*Mission*, pp. 285-86).

Fault can be found with these books. The volume on land tenure includes no study of the more populous and multiethnic areas of the Pacific. The *Mission* compendium is thin at points, especially where it makes its major contribution of attending to islanders' religious organization, not mission aims. A description of interactions between congregations and their pastors, even a microstudy similar to Martin Silverman's account of one of the Ocean Islanders' debates with their British adviser (*Exiles*, chap. 6), would be helpful. Some chapters in each volume may be too

particularistic in their aims to engage the sociological reader. Despite such failings, these volumes and the series as a whole are of value as the first attempt to study Pacific social processes in a format that does justice to their scope, their diversity, and their detail. As such, they provide a vehicle for the reader who would follow the islanders outside the closed environment of the homeostatic single-island study.

Societal Growth: Processes and Implications. Edited by Amos H. Hawley. New York: Free Press, 1979. Pp. xii+388. \$16.50.

Wilbert E. Moore
University of Denver

The idea, introduced a half-dozen years ago, of publishing in book form a very partial and selective "proceedings" of the American Sociological Association annual meetings has produced several valuable volumes. The president of the association selects a theme and is allocated a number of "thematic sessions" scattered throughout the course of the meetings. Some of the papers there presented, revised and often expanded beyond the skimpy length allowed for oral presentation, are then assembled and edited by the president (with or without collaborators) for prompt publication. *Societal Growth* is a major addition to that gradually growing series.

Reviewing any symposium within tolerable space limits is likely to result in less than even-handed justice for all contributors. The reviewer will inevitably reflect personal tastes, preferences, and prejudices. My own prejudices are strongly favorable to the range of topics addressed in this book, which may have had something to do with the invitation to review it. And, let me add at once, my evaluation of the general merits of the 17 essays here dressed up for a second, formal viewing is highly positive.

After a brief introduction by the editor, Amos Hawley has ceded pride of place to Gerhard Lenski's skillful essay, "Directions and Continuities in Societal Growth." This is followed by Hawley's "Cumulative Change in Theory and History," Judah Matras's "Mechanisms and Societal Growth," James S. Coleman's "The Measurement of Societal Growth," and Nathan Keyfitz's "Causes and Consequences of Population Change." Although Lenski's sweeping evolutionary view of the human condition is followed by a mildly cautionary note on the limits of growth, Hawley is carefully attentive to the limits of the evolutionary model itself and notes the increased hazards of major errors in a world grown increasingly interdependent. Again, exercising arbitrary preference, I must especially commend Keyfitz's deft demonstration of the integrating utility of the sociological perspective in analyzing population dynamics.

A second group of papers, headed "Structural Ramifications of Societal Growth," includes essays by Stanley H. Udy, Jr., on organizational complexity, Robert H. Hodge and Garry S. Meyer on social stratification,

J. John Palen on urbanization, Gerald Suttles and Morris Janowitz on metropolitan political processes, Judith Blake on differentiation of familial forms, Harold L. Wilensky and Anne T. Lawrence on occupational allocation, and Philip E. Converse on quality of life. With no derogation of other papers intended, I should like to note Udy's essentially cautionary message deriving from what I should call the diseconomies of scale. The longish essay (pp. 202-48) by Wilensky and Lawrence takes a critical look at the conventional assumption that job assignment by achievement wins systematic victories over assignment by ascription, with long side-glances at apartheid in South Africa, credentialism in education, and other somewhat related topics.

A third and concluding group of papers is presented under the rubric "The Future of Societal Growth," with essays by Immanuel Wallerstein on the politics of the world economy, Irving Louis Horowitz on ideological components of the sociology of development, Joseph Gusfield on the (limited) modernity of social movements, Scott Greer on discontinuities and fragmentation, and Kenneth E. Boulding on another sweeping evolutionary view that reiterates Hawley's sense of increased vulnerability. Wallerstein's essay involves a complex argument about the emerging character of world systems and suffers from at least two defects, both stemming from a touching if suicidal late-Marxist stance: (1) like Marx, Wallerstein attends only to physical production, which leads to the neglect of the predominant mode of production (services) in highly rationalized economies; (2) he still uses the language of class warfare in a situation in which I think a less apt conceptualization would be hard to imagine. Horowitz professes to avoid ideological extremism and votes for a more pluralistic "developmentalism," as opposed to "modernization" and "dependency theory," in interpretation of the universal quest for economic growth and structural change. He arrives at somewhat defensible conclusions by devious routes and despite strange deficiencies in familiarity with the relevant theoretical literature.

Surely no macrosociologist will want to neglect this excellent collection, and, honorable motives aside, it would be foolishly hazardous to do so.

Scale and Social Organization. Edited by Fredrik Barth. Oslo: Universitetsforlaget, 1978. Distributed in the United States by Columbia University Press. Pp. 291. \$20.00 (paper).

Ralph Thomlinson

California State University, Los Angeles

Scale and Social Organization explores relations among several variables that are fundamental to societal functioning. Whether or not a reader likes this collection will depend partly on his or her attitude toward large-scale

studies, for it is a near-perfect example of what Merton has classified as global theorizing. The issues discussed are significant and sometimes fascinating, but while reading, I found myself wondering if the evidence cited was convincingly in support of the interpretations offered. Having finished reading, I still do not know. Hence this is a book better designed to inspire argument than to provide the truth.

The appropriate audience is social science scholars. Most students would find the essays too difficult because of references to theories they would not know, extensive use of jargon, occasional vagueness, and frequent wordiness.

These 10 papers (and an introduction and conclusion by the editor, Fredrik Barth) were originally prepared for a symposium sponsored by the Wenner-Gren Foundation for Anthropological Research and later rewritten to incorporate insights developed during the discussions. The meeting apparently took place in about 1973, and the authors appear to be anthropologists, although the book fails to identify them in any way, merely stating that 16 (named) participants met in Burg Wartenstein (country unspecified). The diversity of names implies that the scholars came from several countries, but I believe that most are Americans.

By "scale" the editor means "size, numbers, and complexity" (p. 11) or "the numbers of something that can be counted: size in the sense of both the number of members or parts, and the spatial extension" (p. 253). The pertinence of scale to societies and their constituent groups is evident, but Barth is so devoted to the concept that he also applies it to the social space surrounding an individual, a topic which has been analyzed adequately without the use of his approach.

Half of the 10 contributors found the idea of scale sufficiently intriguing or troublesome to devote a good bit of space to a struggle to define it. Most authors are in rough rather than exact agreement with the editor's conception.

The book's purpose is to confront problems of scale: "the causes and consequences of differences in size and numbers in social systems" (p. 9). How and to what extent does a change in the population of a society affect the forms and functioning of groups within it? Do small and large societies have different capacities to organize social institutions? Do different forms of social organization have different capacities to promote large or small populations? Do differences in the actuality, awareness, and illusions of scale affect societal norms, statuses and roles, stratification, the organization of labor, political systems, and so forth? These and related questions are investigated by examination of a wide range of ethnographies.

Unfortunately, several of the essays wander from the assigned topic. Wandering is, indeed, invited by the great breadth of the book's theme. I question the appropriateness of such diversions as Barth's case study of two persons' social interactions over a two-week period. He argues that "we can see the scale of any society in the aggregate consequence of the networks the different members of the society spin" (pp. 181-82),

but he does not actually calculate this aggregate. Here lies my main complaint: the book tries to analyze a lot but ends by analyzing rather little because most of the data are incomplete.

Much of the writing consists of discussion of material that is familiar from another context but justified for inclusion here by occasional references to scale. At times I had the impression that the application to scale was added on afterward, for some of these essays would read about the same if they were published in a book having no special concern with scale.

That there is utility in this work is illustrated by a generalization that could be quoted in introductory sociology: Caste in India "works most efficiently within a certain *range* of scale, limited at both ends. Evidently *Pahari* [mountain] villages are too small and isolated for it to work easily. . . . On the other hand, contemporary cities seem to be too large and complex for India's caste system to work easily or well" (p. 60).

There is a fairly long bibliography, but the book would have benefited from an index. The publisher selected good-quality paper and did a good job of design, printing, and binding, but there are far too many typographical errors.

Given the nature of the issues presented, it is not surprising that the book's contribution lies more in raising these important questions than in answering them. Although I did not find the papers fully satisfactory, I did find their ideas usually entertaining and sometimes exciting.

Social Psychology and Discretionary Law. Edited by Lawrence Edwin Abt and Irving R. Stuart. New York: Van Nostrand Reinhold Co., 1979. Pp. ix+370. \$18.50.

Austin T. Turk
University of Toronto

What is to be expected in a collection of ad hoc general papers solicited for some unknown reason by editors who appear to be at best newcomers to the field of sociolegal studies? And what is to be expected from editors who apparently equate social science with psychology, present the concept of discretion as if it were a newly discovered "integrating idea" for examining the "interface" between law and social science, and bring forth a volume written mostly by psychologists (several with joint psychology-law positions), along with several academic or practicing lawyers, a hypnotist, a police inspector, a few professional criminal justice researchers, and numerous student coauthors? (That's right: no sociologists or political scientists, and virtually no criminologists or sociolegal research specialists.) Anticipating the worst—just another pedestrian nonbook—I was happily surprised to find that despite its dubious credentials, *Social Psychology*

and Discretionary Law is an often informative, occasionally stimulating, and pedagogically useful volume. There are at least six or seven really good papers, several offering something of value, and only a few better left unmentioned.

After an interesting historical essay by Joel Samaha on the tension between discretion and law as exemplified and debated in Anglo-Saxon times, three excellent papers follow in which various issues of research and its uses are considered. Wallace Loh's succinct review of the legal status of research evidence and statistical argument leads to the conclusion that neither a "collating strategy" (direct insertion of research findings) nor a "de novo research" strategy (commissioning ad hoc studies) is as promising as the "strategy" of basic research, whose findings "filter into popular culture and then into judge-made law" (p. 38). Observing that eliminating discretion in one sector of the criminal justice system merely displaces it to another, Leslie Wilkins analyzes the system as a decision network involving an information-search and interpretation process that terminates in selection from a set of available dispositional alternatives. He suggests that accountability can be built into the system by the method of "structured discretion," applying a cybernetic model of policy control that requires appropriate feedback both within the system and from the public beyond the professional, collegial, and administrative networks directly involved in the system. Michael Saks and Meridith Miller use "open systems theory" to analyze several problematic features of the legal system, beginning with gatekeeping and external decisions determining entry probabilities. In particular, they point to ways in which subsystem maintenance tends to subvert system responsiveness (adaptability), plausibly hypothesize the increasing transition probabilities of cases as they pass through the cumulative decision sequence of the system (noting the irony that the existence of so many decision nodes may promote routinization instead of due process), and indicate the double-loop feedback problem in the fact that the law is only one of the multiple inputs determining whether discretion is used to maintain or change the system.

Most of the remaining papers review research evidence regarding the exercise of discretion at each stage of the criminal justice process. There are discussions of citizen discretion in reporting theft, police discretion, prosecutorial discretion, jury selection, jury decisions, sentencing, and confinement. Additional papers are concerned with juvenile justice and the legal status of women. In general, major research findings are covered adequately and amended in a few instances by some original quantitative data analysis (e.g., Greenberg and Wilson on citizen reporting of theft; Buckhout et al. on jury selection; Mowen and Linder on jury decisions). Charlan Jeanne Nemeth provides an exceptionally lucid analysis of the complex group dynamics of jury behavior.

Commendably, throughout the book there is a thread of concern with policy implications and applications of research knowledge, or at least with policy admonitions. Especially stimulating to me were Leon Fried-

man's discussion of the legal sources and limits of American prosecutorial discretion, James Herbsleb et al.'s analysis of the legal and ethical issues involved in using social psychological techniques to select jurors, and Stanley Brodsky and Raymond Fowler's review of the evidence and issues with respect to the social-psychological impact of involuntary confinement.

Although specialists will find little new in this collection, it will be a valuable teaching aid at both the graduate and undergraduate levels. And even specialists are likely to glean a few suggestive ideas and some helpful information. However it happened, Lawrence Abt and Irving Stuart have given us a pretty good book.

A Social Interactional Theory of Emotions. By Theodore D. Kemper. New York: John Wiley & Sons, 1978. Pp. ix+459. \$19.95.

Susan Shott

University of Chicago

This often insightful book is a welcome contribution to the sociology of emotion, an area that has only recently attracted some of the attention it deserves. Not only has Theodore Kemper developed a useful and systematic analysis of the ways that specific emotions are elicited by various social situations, he has also used a fairly simple power-status model of human interaction to generate considerable insight into affective behaviors ranging from mental disorders to love.

Proceeding in an unusually orderly fashion, Kemper first describes the types of emotions produced by varying combinations of power and status levels. Excess power, for example, tends to produce guilt, while insufficient power generates fear and anxiety; excess status elicits shame, while insufficient status induces depression. An extensive typology of social relational outcomes is developed, focusing on gains and losses of power and status and on the attribution of such gains and losses to oneself or to others. On the basis of this typology, the emotions characteristic of various relational outcomes are predicted. Some of this typologizing becomes (necessarily) a bit tedious. The reader who perseveres, however, will be amply rewarded by an increased appreciation of the regularity underlying the often bewildering variety of affective responses to social situations.

Kemper has coherently tied together a wealth of empirical studies, which alone might make the book worth reading, but he has also done much more. In applying his power-status model, he often deals with familiar topics in ways that generate interesting and intuitively appealing hypotheses. The discussion of punishment in the socialization process, for example, abandons the usual classifications of punishment as physical or psychological, or as love oriented or object oriented, and focuses instead on two dimensions of punishment which, Kemper believes, determine the

nature of affective responses to it. These concern (1) whether the punishment causes the child to focus on power or status and (2) whether the punishment is proportional to the transgression. Punishment viewed by the transgressor as status withdrawal evokes shame when considered proportional to the offense and produces depression when considered excessive. Similarly, punishment interpreted as an exercise of power elicits guilt when proportional and anxiety (without guilt) when excessive. The book abounds with such plausible, testable hypotheses about affective responses; and the areas discussed include such diverse topics as mental disorders (particularly schizophrenia and depression), the role of proper socialization in producing positive emotions, and love. (The two chapters on love are perhaps the most insightful, illustrating exceedingly well how much sociology can illuminate emotional behavior.)

Some of the physiological aspects of emotion are also considered, particularly the question whether different emotions are accompanied by distinct physiological patterns (the specificity question). Kemper argues for the specificity position (i.e., the position that distinct physiological patterns characterize different emotions) and provides a thorough review of the relevant literature (including both pro- and antispecificity work) in doing so. Unfortunately, he also goes to great lengths to explain away results inconsistent with specificity theory, sometimes resorting to rather farfetched reinterpretations of studies. (When discussing one of L. Levi's studies, for example, he dismisses anger evoked by a movie as "intellectual anger, the kind we think but do not feel" [p. 196].) Such reinterpretations do not, of course, discredit the evidence favoring an antispecificity position, since only a reasonable amount of ingenuity is needed in order to explain away any result one does not like. Kemper's discussion of the antispecificity position would have been greatly improved by a recognition of this point.

Nonetheless, *A Social Interactional Theory of Emotions* is, overall, a fine work, admirably demonstrating how very amenable emotions are to sociological analysis. Sociologists who are unconvinced that emotions fall within the purview of sociology will find this book well worth their time, as will those sociologists already concerned with emotion. Both will learn a great deal from it.

The Social Order of a Frontier Community: Jacksonville, Illinois, 1825-70.
By Don Harrison Doyle. Urbana: University of Illinois Press, 1978. Pp.
xiii+289. \$12.95.

Thomas Bender
New York University

Frederick Jackson Turner's portrait of the western movement implied, if it did not explicitly argue for, a process of dissolution of established social

forms. In *The Rise of the New West, 1819-1829* (New York: Harper, 1906), Turner quoted Morris Birkbeck: "Old America seems to be break-up, and moving westward." In *The Social Order of a Frontier Community* Don Harrison Doyle also quotes Birkbeck, and the burden of his book is to demonstrate that Birkbeck's observation referred to the departure not the arrival: social life in the West was not, he argues, atomistic. Restating a theme broached by Stanley Elkins and Eric McKittrick 25 years ago, Doyle stresses the community-building aspect of the western movement. Various historians, most notably Allan Bogue and Robert Dykstra, have tried to expand upon the suggestive article by Elkins and McKittrick, and Doyle uses their work effectively while advancing beyond them. All of these historians, beginning with Elkins and McKittrick, who drew upon theories of community building developed by Robert Merton in a housing study, have been unusually open to and sophisticated about the use of sociological theory.

Elkins and McKittrick had relied upon local histories for many of their data. But since such "booster" histories studiously avoid any mention of conflict or division, Elkins and McKittrick's work probably understated the role of conflict in community building—a point that became the focus of criticism by Bogue and Dykstra. Doyle's book begins with a summary of this issue, but he does not pursue it directly. Instead, in the course of exploring the principles of social order in an expansive, mobile society he illuminates the sources of both conflict and consensus.

Doyle uses Jacksonville, Illinois, for a case study. Jacksonville was created in 1825 as a compromise county seat to settle a three-town rivalry. By 1870, it had a population of 10,000. Its location in southern Illinois meant that it received migrants from both North and South. By mid century, it was also becoming the home of immigrants from Portugal, Germany, and Ireland. During the period under study, therefore, Jacksonville was a heterogeneous town. It was a town of ambition, but it never attracted the commercial-industrial economic base it sought, nor did it become the state capital (a serious possibility before neighboring Springfield won the prize). Failing in its quest for economic and political significance, the town cultivated an image of cultural accomplishment by becoming the home of educational and state charitable institutions.

"Boosterism" is an old theme for amateur local historians, but Doyle argues that it can be used by sophisticated professional historians as a key to understanding the process of community integration. For the middle class, he suggests, boosterism was a way of life. The heads of middle-class families, who represented perhaps 25% of Jacksonville's nondependent population, were propertied, residentially stable (three or four times more so than nonproperty owners or unmarried men), and upwardly mobile. This established core population "built businesses, homes and careers in Jacksonville. . . . [and they] raised children there and watched as those children took their own places in the community" (p. 97). In the eyes of this group, individual and collective progress coincided and reinforced each

other. Indeed, Doyle shows that 85% of these men maintained or improved their already favorable economic position between 1850 and 1870. He also demonstrates that after about 1850, they turned increasingly to formal institutions, including the city government, which they dominated, to confirm their social power and to shape the social order in their own image.

In this portrait, though he may not think of it in these terms, Doyle provides a valuable account of the origins of midwestern, small-town Victorianism. Yet in the context of his argument, one misses any suggestion of conflict over the middle-class ideology of progress. While he is excellent in delineating organized conflict over the middle-class commitment to gentility, as evidenced in the temperance issue, he seems uninterested in getting at conflict over the fundamentals of boosterism. It is striking that he fails to pursue references to oppositional "Old Fogeyism" in the newspapers he quotes.

Accepting Doyle's argument for a stable core within the town leaves two questions. Does the core represent only an isolated island of community in a more pervasive swirl of chaos? What is the relation of the core to the social order of the whole, which includes transients, immigrants, and the lower classes? Doyle recognizes and addresses these questions. The core, he suggests, provided a touchstone and perhaps a model for less stable elements in the community. There was, Doyle points out, a good deal of personal contact between the middle class and the more fluid portion of the population who lived with them as servants or boarders. He also indicates that an analysis of kin networks reveals that at least one-third of all adult males (in all classes) had at least one adult male relative in town. And he suggests that the burgeoning nationalism of the period often produced local activities and celebrations that enhanced rather than detracted from local solidarity.

Doyle places most of his emphasis on the integrative role of formal institutions. Using what must, I guess, be called a trickle-down theory of middle-class associational models, he argues that the pattern of organization found in middle-class churches, fraternal organizations, self-improvement societies, and the like found parallels in the organizations of ethnic and lower-class populations. His point is that this organizational parallelism provided a deep structure of order in the community that bridged the "boundaries of culture" separating, for example, ethnic groups. This, he further argues, nourished a general acceptance of the basic orientation to community life established by the core group. Hence while he insists upon the existence of organized conflict in his discussion of temperance, politics, and religion, at the heart of his argument is a notion of order and consensus based upon the hegemony of middle-class models of social organization. What he gives on one level of social life to Bogue and Dykstra, he takes back for Elkins and McKittrick on another and deeper level.

Marxism and the Metropolis: New Perspectives in Urban Political Economy. Edited by William K. Tabb and Larry Sawers. New York: Oxford University Press, 1978. Pp. viii+376. \$10.00 (cloth); \$5.50 (paper).

Frances Fox Piven
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In February of 1975 a group of economists affiliated with the Union for Radical Political Economists called a conference in New York City entitled "Marx and the Megalopolis." The conference attracted hundreds of enthusiastic participants, from Europe as well as North America. *Marxism and the Metropolis* consists mainly of papers culled from that event.

Both the conference and the volume are an expression of a kind of renaissance of intellectual Marxism that has taken place over the past decade, particularly in the United States and particularly among social scientists in the United States, who until the renaissance tended to dismiss Marxian thought as if it consisted of one simple proposition about economic determinism, thus blithely ignoring the complex, rich, and diverse perspectives that had flowered elsewhere in the Marxian tradition. (This theoretical parochialism in American social science nicely complemented its empirical parochialism, its tendency to ignore the possibilities of comparative research or to consider that studies of Iowa and Idaho, of Boston and San Francisco, exhausted the range of comparative studies.)

The renaissance has finally made available to young American social scientists one of the historically most important theoretical traditions in the world. But it is not simply a revival of the Marxian masters, as this volume demonstrates. One form that it has taken is the effort to apply models derived from the Marxian theoretical tradition to the set of both intellectual and social problems associated with the field of urban studies, problems which have been a preoccupation of American social scientists and practitioners since the 19th century, but which loom even larger today in this era of "urban crisis." The articles collected here thus represent a growing effort to develop a substantially different model, with roots in classical Marxian theory, for interpreting the age-old problems denoted by such American catchwords as urban decay or urban blight or urban fiscal crisis.

The articles in this collection are provocative. They begin to move us beyond what the editors, William Tabb and Larry Sawers, refer to as the "fetishism of space" that has characterized urban studies. For too long we have relied for explanations of urban phenomena on models of urban development which are in fact organic analogies, and which explain particular patterns of urban growth and decline by invoking a theory of natural urban form. The editors might have added that we fetishize the market and governmental structures that pattern urban development in a similar way. Thus the recent spate of commentary which interprets the decline of the Northeast as the result of inexorable market forces rests

on the premise that these forces are enduring and inevitable and not the product of human activity. And we tend to view the particular arrangement of fragmented and decentralized government structures that contribute to American patterns of city development as if these structures also were inevitable, so that we pay scant attention to the variable structural arrangements of the state in different advanced societies (including different capitalist ones), and the variable urban experiences that result. In the place of such fetishes of mainstream urbanism, these articles advance a model which emphasizes the central and problematic role of the capitalist mode of production in shaping the capitalist city, and the centrality of the dynamic of class struggle in reshaping capitalist urban patterns over time. And the model, preliminary though it still is, goes far toward illuminating features of the American urban experience which have hitherto been viewed as intractable and recurrent "urban problems."

The collection also has some of the flaws that we might expect to characterize a preliminary effort. For one thing, there is a tendency in some of the articles to engage in the exercise of translating what are in fact familiar mainstream perspectives on urban patterns into a Marxian terminology. It is not clear to me that much is gained by such an exercise; if anything, it is a step backward into opacity. Moreover, the relative ease with which the translation is sometimes accomplished suggests that the difference in perspectives is overstated. Marxian thought is after all part of the Western tradition; it shares some of the same roots as American social science, and it has influenced American social science, albeit in ways that have remained unacknowledged. Finally, both the critique of "liberal" solutions and the presumption that a Marxian analysis automatically provides more fundamental solutions are naive. It is surely true that standard liberal solutions to urban problems have not "solved" those problems; but neither does it solve these problems to argue that they are embedded in basic features of American capitalism, at least not until we develop a coherent and feasible strategy, guided by theory and shaped by experience, for changing American capitalism.

Socialism in Provence, 1871-1914: A Study in the Origins of the Modern French Left. By Tony Judt. New York: Cambridge University Press, 1979. Pp. xiv+370. \$42.50 (cloth); \$12.95 (paper).

William Brustein
University of Washington

Since the publication of A. Siegfried's classic study of the social bases of French political regionalism in 1913 (*Tableau politique de la France de l'ouest sous la troisième république* [Paris: Colin]), there have been numerous attempts by scholars to explain why certain French regions mani-

fest distinctive voting patterns. Southern France, which has consistently supported the Left, has been the focus of much of this attention.

Tony Judt's *Socialism in Provence, 1871-1914* is a most promising inquiry into the nature of political regionalism. Judt seeks answers to two specific questions: What factors were responsible for socialism's initial success? Why has socialism persisted in Southern France?

French sociologists have often suggested that the distinctive regional setting in the French Midi—including a grouped habitat, a market-oriented economy, an independent small-holding peasantry, and a special kind of southern sociability—can explain socialism's initial success. While accepting the distinctive regional setting as a significant factor, Judt contends that socialism emerged in this region chiefly as a consequence of the collapse of the southern rural economy during the 1870s and 1880s. He argues that the collapse of agriculture and local industry at that time led to socialism in those rural areas characterized by a (formerly) complex social structure, intensive peasant interdependence, and a long tradition of collective interests. Socialism appealed to the depressed peasantry because it provided collectivist solutions to peasant problems.

This conclusion leads Judt to reject the view that rural socialism in the Midi was part of a regional leftward progression, begun in 1849, from the Démocrates-Socialistes through the Republicans, Radicals, Socialists, and most recently the Communists. Data from 21 communes in the Var provide the basis for his conclusion. Some of these communes were conservative in 1849 but by 1880 had become socialist; others were leftist in 1849 but by 1880 had not turned socialist.

Finally, Judt attempts to explain why socialism has persisted in the Midi. Until 1914 the party's advocacy of specific peasant issues won it support; thereafter this support became institutionalized: "Voting for the candidate of the SFIO became as much a part of local culture as attending church" (p. 300). He sees the post-1914 decline in socialist support as a result of the party's dependency on a set of specific issues that were no longer relevant and of the decreasing size of its social base, namely, the peasantry. The recent resurgence of socialist support is, he argues, due to the personality of socialist leader François Mitterand and to a confluence of different circumstances.

Judt claims that these findings have important implications for the success of socialism in France, Europe, and the Third World. If they are to be successful, socialist revolutions must occur during the early stages of industrialization when there is a large peasant base and considerable economic deprivation.

How well are these hypotheses supported by existing data? It does appear that the Midi departments of the Var, Gard, and Hérault were severely disrupted by the post-1875 social and economic crises and then became increasingly socialist. However, the departments of Vaucluse, Pyrénées-Orientales, Ariège, and Aude were similarly developing a strong rural socialist movement, even though they were not markedly affected by the post-1875 disruptions (see, e.g., Robert Laurent, "Les Quatres Ages

du vignoble du Bas-Languedoc et du Roussillon," in *Economie et société en Languedoc-Roussillon de 1789 à nos jours*, ed. Centre d'Histoire Contemporaine du Languedoc Méditerranéen et du Roussillon [Montpellier: Centre National de la Recherche Scientifique, 1978], pp. 19–20).

Judt provides no data to support his contention that deindustrialization is significantly related to the growth of socialism. This might have been done by comparing occupational profiles from a pre-1875 population census with a post-1875 one. Moreover, how can Judt explain why certain rural cantons—such as in the Montpellier-Lunel, Nîmes, and Avignon-Orange regions—that experienced the most severe deindustrialization were among the most politically conservative areas of the entire Midi during the late 19th century?

Judt's analysis of the post-1914 fluctuations in socialist support as a uniquely peasant phenomenon is debatable. The chief interests of the peasantry were compatible with those of the disenfranchised nonagricultural populace. Both groups shared similar views on such key issues as the role of big business and large property, clerical interference in the public domain, a tax reform, and the establishment of a "république sociale et démocrate." More important, the livelihood of this nonagricultural rural populace was also dependent upon the fate of the region's agriculture.

A more plausible explanation, one that is not discussed by Judt, is that these fluctuations are a consequence of the socialist party's role in government. When the French Left has challenged the established economic, political, and moral order it has been capable of mobilizing the rural populace of the Midi. However, as each political movement turned rightward the rural populace moved leftward in search of a new ally.

Judt's argument that rural socialism is not part of the regional leftward movement of the Midi since 1849 is questionable on many accounts. His sample, taken from the Var, is not adequate for the entire Midi. Within the departments of the Vaucluse and Hérault the rural cantons of Apt, Pertuis, Cadenet, Cavaillon, Mèze, Frontignan, Pézenas, Agde, Bédarieux, and Clermont have drifted steadily leftward since 1849.

The analysis could have been improved by considering the effect of regional modes of production on political behavior (see Michael Hechter and William Brustein, "Regional Modes of Production and Patterns of State Formation in Western Europe," *American Journal of Sociology* 85, no. 5 [March 1980]: 1061–95). The most left-wing areas in the Midi were those in which conservative elites failed to establish political and social predominance. The class structure in the Midi fostered peasant class consciousness and obstructed the ability of the landowning elites to gain predominance by implementing dependent forms of property structure. In contrast, the French right has historically planted its deepest roots in such rural areas as Western France, where the mode of production produced a class structure in which the large landowning elites dominated village life by virtue of their rural residency and control over the property structure. An understanding of the social elites' ability to establish and

maintain a client system is fundamental to any study of rural socialism in France.

Although there are major flaws in Judt's analysis of rural socialism in Provence, particularly relating to the representativeness of his sample and overly confined discussion of the bases of socialist support, his work is commendable in two important respects: it takes exception to the traditional view of the peasantry as an inherently conservative or otherwise politically nondescript stratum; and it seeks a historical and sociological explanation of why the peasantry of the Midi has consistently supported the left.

The Structure of Socialist Society. By András Hegedüs. New York: St. Martin's Press, 1977. Pp. viii+230. \$9.95.

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The reading of this book, as well as some other publications in Eastern European social science, requires a great deal of background knowledge, especially concerning the social-political situation and pressing issues in the country at the time of publication, the biography of the author, and even the prevailing jargon and censorship policies. The publisher of András Hegedüs's *The Structure of Socialist Society* ought therefore to have provided a comprehensive introduction for this purpose.

The reader should keep in mind two conflicting paths in Hegedüs's development: the official career, leading from prime minister of Hungary in the middle fifties, through the directorship of the Institute of Sociology of the Hungarian Academy of Sciences in the sixties, to expulsion from the Communist party and forced retirement in 1973; and the intellectual career, in which the official sociologist of the early sixties becomes one of the leaders of the Hungarian intellectual opposition, one of the few who did not just deny the socialist character of East European societies but proposed a positive program of political and economic reform.

The Structure of Socialist Society consists of two parts. The first, devoted to theoretical problems in the structural analysis of socialist society, was published in 1966 during the peak of economic reform in East Europe, which was stopped after the invasion of Czechoslovakia in 1968. In his capacity as an official leader of Hungarian sociology Hegedüs attempted to close the tremendous gap between the Marxist theory of class structure, especially its Stalinist version, and the practice of empirical research. In his book he delivers a heavy blow to all official rhetoric in which property relations appear as the only independent variable of East European social structure. He refutes the ideological scheme according to which the distinction between state and cooperative property allows us to distinguish working class and peasantry. (This scheme still prevails

in Soviet social science, and Hegedüs's refutation required not only scientific objectivity but also great personal courage.) Hegedüs argues that the position occupied in the social division of labor is the most important variable determining the class structure of socialist societies, where types of property ownership do not act as significant "class-forming" agents.

Hegedüs follows up his ideas in the second, more empirically oriented part of the book, originally published in 1970, where he offers important insights into the process of transition of rural Hungarian society into a society with a modern industrial agricultural sector. He is especially interested in the growth of the new administrative apparatus, and in the separation of administrative and directly productive work in agriculture, which he justifiably considers to be "an extraordinarily significant process, and a necessary consequence of socialist reorganization, though not one desired or put forward as an objective" (p. 95).

Thus Hegedüs focuses on the application of Max Weber's ideas to the structural analysis of socialist society, demonstrating its advantages and, one might add, its drawbacks. While addressing problems of inequality in Hungarian society, he makes excellent use of a Weberian approach not limited to analyzing the end of all inequalities but allowing us to analyze how "new inequalities took the place of the old" (p. 108), largely because "the new structure significantly narrows the opportunities for intergenerational mobility" (p. 125).

Hegedüs's most striking idea is his denial of the role of political power in forming the social structure of Soviet-type societies. He asserts that the "relationships of domination and subordination under socialism arise directly from differences in the position occupied in the division of labour" (p. 52), and from the fact that the bureaucratic relation in socialist society is a social necessity. Quite consistently, in later works Hegedüs practically drops the notion of class, concentrating mostly on a hierarchy of strata (*La struttura sociale dei paesi dell'Europa orientale: Un'analisi marxista* [Milan: Feltrinelli, 1977]). In *The Structure of Socialist Society* he tries to demonstrate that there is no significant social difference between people doing the same work at a producers' cooperative or at a state farm. However, it would be sufficient to turn to the Soviet experience to realize the folly of ignoring how social structure is politically determined. *Kolkhozniks* and agricultural workers form two separate social classes owing to a powerful barrier between them, erected by the state: the internal passport system.

Hegedüs's book is replete with ideological clichés, stereotypes, and slogans, the use of which can be attributed to both the author's position in officialdom and the fact that it was written during an early stage of his intellectual development. However, to a significant extent he achieves his self-proclaimed aim: to help form a more realistic picture of East European societies. In the seventies Hegedüs continued to develop his analysis of these societies as a new social-economic formation. His current provocative ideas about the impossibility of discarding the one-party system, the need

to control political power through public opinion, and the desirability of independent social movements are widely discussed among East European intellectuals. His latest works deserve to be translated into English without another decade-long gap.

Rural Socio-cultural Change in Poland. Edited by Jan Turowski and Lili Maria Szwengrub. Warsaw: Ossolineum, Polish Academy of Sciences Press, 1977. Pp. 225.

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According to Ryszard Turski the general trend of social change in rural Poland is described by sociologists in several ways: urbanization of the country, linking of local rural groups with supralocal organizations, disappearance of rural-urban differences, the transition from isolation and autarky to integration with global society, disintegration of rural communities and integration of inhabitants into the national society, transition from peasant to national culture, and increasing participation of the rural population in the national life of Polish society (p. 201). These descriptive phrases are suggestive of Roland Warren's characterization of the "Great Change" period in American communities. And if one accepts Jan Turowski's assessment of the trends, industrialization and the formation of capitalism were the main forces driving the wheels of change in rural Poland as they were in the United States (p. 12).

Each of the 11 papers in *Rural Socio-cultural Change in Poland*, edited by Jan Turowski and Lili Maria Szwengrub, deals with one or more aspects of this process of transformation. Turowski's opening paper provides a brief review of the traditional rural community in Poland, its disintegration, and the newly emerging forms of social integration in the rural areas. His discussion of these changes places the matter clearly and explicitly on the horns of the sociological dilemma of microorganizational integration: internal versus external. In a society devoted to strong centralized national planning, some workable solution to this organizational dilemma must be devised. Yet solutions to fundamental conflicts of social existence are always fragile and usually temporary in the face of history.

The shifting character of local organizations and institutions is the central theme of several contributors. The first types of formal association began to appear in the late 1800s with the emergence of the commodity-money exchange system in agriculture. This emancipated the peasant farm from total functional dependence on neighbors and the local village community. According to Wacław Piotrowski, peasant supply and sales co-operatives and other specialized cooperatives which serviced the needs and interests of independent, family farms were well established by the turn of the century. One should keep in mind that during this period, and until

1918, Polish territory was partitioned among Prussia, Austria, and Russia. The First Polish Republic (1918–38) brought legal regulation and unification of formal structures in rural Poland, which further deepened the disintegration of local ties.

The Polish People's Republic was created after World War II and immediately began a program of land tenure reform. Existing cooperatives were replaced by state and collectivized farms. This did not bring the desired results, and, in 1956, collectivized farms were dissolved and members allowed to return to individual farming. According to Piotrowski, since 1956 local institutions and organizations have played an ever-growing role in the functioning of rural areas and communities. In 1973 further important reforms were adopted which changed the local administrative unit by replacing nearly 6,000 rural communes with 2,365 rural districts. Piotrowski maintains that the spatially expanded rural district is a more viable local social system with greater capability of achieving local and national integration simultaneously.

The tensions of vertical linkage and internal integration of communities are apparent also in Zbigniew A. Zechowski's discussion of sociological problems of schools and teachers in rural communities, in Włodzimierz Winclawski's treatise on sociological environmental theory as the basis of the characterization of "upbringing" in contemporary Polish society, in Andrzej Piekara's review of the rural cooperative movement under the Polish People's Republic, and in Bohdan Jaluwiecki's comparison of industrial workers with rural and urban heritages.

An extremely important consideration in the internal-external integration issue is the psychological and ideological commitment of country folk. There is a delicate balance needed between local identification and national consciousness. In Poland this state of affairs is complicated by the divergent ethnic, language, and religious heritages of the Polish people. The development of a national consciousness in the Polish peasantry is analyzed by Z. T. Wierzbicki, who argues that the transformation can best be understood within the terms of innovation diffusion theory.

There is a lengthy contribution by Kazimierz Dobrowolski entitled "Studies on the Theory of Folk Culture." It is rather oddly placed in this collection because it is a very detailed and carefully prepared statement on the problems of using cultural relics in the study of folk culture. Although this paper occupies 30% of the 225 pages, it seems to be tangential to the central theme of the volume. Nevertheless, ethnographers will find this piece of value.

As is usual in collections of papers, the quality is variable and their styles are discordant. In this case the translation to English is not uniform. The lack of an editorial introduction to bind the papers into a common frame of reference leaves the reader to infer the central theme. These limitations notwithstanding, I found there was something distinctive about this collection. There is a positive, creative ethos to the papers that gave me the distinct impression that the authors are strongly committed to the construction of a Polish society with explicitly stated goals. This is

in sharp contrast to much American sociological literature which either deals with intellectual issues without a societal reference or explicitly attacks the American society and its institutions. The constructive endeavor which is apparent in these papers may be the wellspring of the long-standing excellence of Polish contributions to world sociology.

Revolutionary Law and Order: Politics and Social Change in the USSR.
By Peter H. Juviler. New York: Free Press, 1976. Pp. 274. \$13.95.

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Revolutionary Law and Order provides the first comprehensive historical survey of the evolution of the Soviet response to their national crime problem. Placing the developments of the Soviet period in a historical context, Peter Juviler shows which patterns of criminal justice administration represent continuity with the past and which a definite break with tradition. He carefully illustrates the oscillations in criminal justice policy in the first years of the Soviet regime—the initial liberalization of criminal law and criminal policy that followed the 1917 revolution and the all-too-rapid rigidifying of law and penal policy under Stalin. Juviler gives a sophisticated synthesis of the scant information obtainable from the criminological literature of the late 1920s and 1930s to provide insights unavailable elsewhere on the dramatic impact that Stalinist criminal procedure had on the evolution of Soviet society.

Most of the book analyzes the post-Stalinist years, in which crime ceased to be treated as a primarily political concern. Juviler's analysis of the years from 1950 to the present examines three areas—the changing nature of criminal justice, crime patterns, and the criminological research that attempts to understand and explain the existence of crime in Soviet society. The latter two-thirds of the book expand some of the author's previous writings on contemporary Soviet criminality. This section is a definite contribution to the field in its synthesis of materials and analytical depth, but it lacks the unique information discussed in the first third. Juviler's discussion of criminological developments in the post-Stalinist period complements but does not overlap the work of Walter Connor, *Deviance in Soviet Society* (New York: Columbia University Press, 1972), and Peter H. Solomon's more recently published *Soviet Criminologists and Criminal Policy* (New York: Columbia University Press, 1978).

Juviler's analysis of crime and criminal justice during the Stalinist years is distinctive in that he has combed the limited criminological research of the period to provide startling insights into the dynamics of criminal justice in the initial decade of the Stalinist purges. These sources that he uncovered while conducting research in Moscow have been overlooked in such distinguished works on Stalinist persecutions as Robert Conquest's

The Great Terror (New York: Macmillan, 1973) and Aleksandr Solzhenitsyn's *Gulag Archipelago* (New York: Harper & Row, 1974-78). The value of Juviler's discoveries lies not only in the statistics he discloses but also in the analytical framework that he provides for this research. Juviler shows that the revolution orchestrated "from above" by Stalin was achieved by repressive legal controls enforced by a highly vigilant criminal justice system.

After Stalin's rise to power, a repressive politicized approach to criminal offenders replaced the liberal innovative approach of the first decade of Soviet rule. Juviler documents the major changes necessary in the nature of the criminological research community and in the laws governing society to institute the dramatic changes that Stalin desired in agricultural and industrial production. These changes, not warmly greeted by the populace, resulted in large-scale repression of wealthy agricultural workers and purported saboteurs of industrial plants. The massive arrests, whose annual increase Juviler documents carefully, produced a large labor camp population by 1930, years before the onset of the intensified purge period of the late 1930s. Juviler demonstrates that the professional criminal class was also affected by the increased repression, but this group of prisoners received the exclusive benefit of any amnesties.

After Stalin's death, major changes were instituted in the structure of Soviet justice to eliminate the irrationality and excesses of the previous decades. Under Khrushchev, criminality was redefined in the criminal code, making only a small portion of the total criminal population "political." Although the criminal code provided stability, the administration of justice in this period, according to Juviler, was marked by inconsistency. The halcyon days following Stalin's death resulted in the rapid liberalization of criminal justice practice. During the Khrushchev years fear of the consequences of the relaxed controls led to harsher penalties and periodic campaigns against particular sources of social disorder. Campaigns against hooligans, alcohol abusers, parasites (those refusing to work), and drug addicts characterized criminal policy in the 1960s, as Soviet authorities attempted to control the effects of a more permissive society.

The greater tolerance of crime in the post-Stalinist period was accompanied by a desire to understand the causes and dynamics of criminality. Juviler examines the debates that emerged in the Party and the legal community concerning the reintroduction of the discipline of criminology and the steps that were taken to establish serious scholarship. These new efforts to study crime included attempts to measure the level of crime (a ticklish problem in a country where crime statistics are still state secrets), the types of crime, and the relative frequency of particular offenses. In this section the author provides excellent statistical tables on conviction rates and the distribution of crime by offense that he has constructed using a large number of official Soviet sources.

The reemergence of criminological research has both distinct advantages and disadvantages for Soviet authorities. As Juviler points out, the study of criminality provides confirmation that the Soviet Union has been unable

to live up to its ideological pronouncement that crime will disappear under socialism. The assertion made by Soviet researchers that the crime problem is not increasing and is actually decreasing, however, allows certain ideologues to claim that "they have found a superior path of development" (p. 143). Criminological research, according to Juviler, is caught between its attempt to construct a serious academic discipline and the strict ideological controls that restrict its intellectual development.

Revolutionary Law and Order is a copiously researched and carefully documented account of the Soviet response to the problem of crime and social order. It does not, as the introduction suggests, provide broader insights into the impact of violence on the state of Soviet society or the evolution of Soviet politics. While Juviler's analysis does not fulfill this broader objective, it does provide an excellent treatment of the transformation of crime and criminal justice that has been so central to the evolution of Soviet society.

Critical Theory of the Family. By Mark Poster. New York: Seabury Press, 1978. Pp. xx+233. \$14.95.

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The family demands and deserves more of our attention, yet it remains peculiarly elusive. New interest in the black family and the issue of "matriarchy" provoked by Moynihan in the mid sixties, feminism, demographic history, and the spread of humanist psychology and therapy have in the past 15 years combined to generate a continuously growing supply of family studies. In *Critical Theory of the Family* Mark Poster's project is to develop an adequate theory of the family, an effort which places him outside main sociological efforts in this country. Those who share this interest in family theory are primarily feminists, who, ironically and gravely, Poster ignores, as well as commentators and those influenced by French linguistic and structural psychoanalytic theory associated with Jacques Lacan.

Poster's strongest contribution, and the bulk of the book, is his clear explication of a range of previous theories. He begins with Freud, the central figure here, suggesting that a "revised Freudian model offers the most comprehensive theoretical basis for family studies" (p. 145). After Freud, Poster considers the leftists, Engels, Wilhelm Reich, and the Frankfurt School, particularly Max Horkheimer; then Erikson and Parsons (in a chapter entitled "Ego Psychology, Modernization and the Family"); and then Lacan, the French Freudian who utilizes structural analysis of language as the key to the unconscious (and whom Poster makes intelligible, which is no small task and just about worth reading the book for). Finally, he examines Gregory Bateson, R. D. Laing, and several others under

the heading "Family Therapy and Communication Theory." The last two chapters offer Poster's own contribution to a theory of the family.

Poster's focus, then, is primarily psychological and emotional. He is concerned with theorizing the psychic structure of the family and with understanding patterns of emotional interaction historically. While profoundly indebted to Freud, Poster suggests that the first psychoanalyst inaccurately inflated the emotional economy of the bourgeois Victorian family into a cultural universal. Psychoanalysis was historically specific: Freud legitimated the private nuclear family, the necessity of social control of children by parents, and sexual repression of children. Poster, like psychoanalysis, is especially interested in the plight of the child in the modern nuclear family, demonstrating little interest in spouse relations or in the difficulties of parenting. He also appears blind to the main contribution of feminist theorists—the study of gender in the family—rarely indicating the importance of male-female differences between the child and parent for understanding the child's psychological development and the emotional content of parent-child relationships.

The argument that Freud's theory universalizes a historically specific family and lacks a theory of family structure that can be utilized to understand all kinds of families is in fact leveled against all the theorists Poster examines. In all instances he stresses that they have no historical theory of family structure which can account for wide variations.

There are several troubling components in his approach. First, Poster has perhaps created a "straw man" in that he faults all the theorists for not developing an adequate theory of the family. It is not at all clear that *their* project was to do this. In fact, he occasionally grants that some of them were concerned primarily with the health and development of the individual. It is an interesting problem to turn the tables on ourselves and consider why the family is center stage now when the individual was of more concern earlier in the century. I suspect that whichever institution appears most vulnerable generates social science attention. Second, as noted, feminist theory and theorists are absent from the book, which means considerations of gender are absent, a very serious omission in a theoretical book on the family. This is particularly irritating because Poster utilizes feminist insights into Freud and others without acknowledging them. It weakens his contribution because issues relating to gender—power between men and women, masculine and feminine socialization, and the sexual division of labor—are essential to understanding the family, not only in regard to love and authority between parents and children (Poster's main concern) but also as factors in, for example, family structure, family size, women's labor force participation, and family violence.

Third, Poster assumes that we are capable of developing one coherent and elegant "theory of the family." In spite of his warnings about how others have universalized and reified "the family," it is difficult to avoid the sense that Poster too has a theory waiting for us at the end of the book. He cautions about the enormous variations by historical period and class and in hierarchies of sex, age, and authority, but the implication of

his book is otherwise. This last issue is closely related to Poster's failure to stress the intimate linkages between the family and other institutions such as work and class, neighborhood, friends, day care, and medical services. One of the reasons the family is so elusive is that it is so affected by these other elements. The closer you get, the more obvious it is that family structure is as contingent on outside opportunities and pressures as it is on tradition and human will. The relevance of age and sex, Poster's main categories, must be considered in the wider society in order to understand their significance inside the family. This makes the development of a single critical theory of the family unlikely. It is the virtue of this book to clarify much of our theoretical heritage; we must be careful, however, not to eliminate the ambiguities and complexities which are likely to hold the keys to understanding families.

Commission Politics: The Processing of Racial Crisis in America. By Michael Lipsky and David J. Olson. New Brunswick, N.J.: Transaction Books, 1977. Pp. xiv+476. \$14.95.

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The most visible expressions of racial discontent in the late 1960s were the civil disorders experienced by many major American cities. These disorders inevitably bred commissions appointed to investigate the events and recommend policy changes that would prevent their recurrence. It has been a decade since these disorders, and one may ask what became of these riot commissions and their recommendations: Did they have a significant effect on race relations in America? In *Commission Politics*, two political scientists, Michael Lipsky and David J. Olson, provide a well-researched and detailed negative answer.

Their answer is based on three assumptions concerning the reaction of the political system to racial crises. First, governmental intervention in racial crises is aimed primarily at "reducing the levels and persistence of racial violence" (p. xii), rather than at immediately addressing grievances. Before grievances can be addressed they must be taken out of the street and placed in the context of interest-group bargaining—"politics as usual." Second, the political meaning of racial disorders depends not only upon the meaning given them by the participants, but also upon the way in which governmental elites respond to them. Therefore, another aim of governmental intervention is to deny their political meaning, instead labeling them criminal (the "riff-raff" theory) or conspiratorial (the "Rap Brown" theory) (pp. xii-xiii). Third, the most typical governmental response to racial crises of the 20th century has been the appointment of riot commissions (p. xiii). Commissions represent a symbolic recognition of minority grievances by the political system without necessarily promising to act substantively to re-

dress these grievances. Commissions buy time for passions to cool, and thus they are the major vehicles used by governmental elites to transform crisis politics into politics as usual.

Commission Politics focuses on four commissions appointed to investigate the civil disorders of 1967: the Kerner Commission, appointed by President Johnson; the New Jersey state commission, appointed by Governor Hughes; and city commissions appointed by the mayors of Detroit and Milwaukee to investigate riots in their cities. The authors argue that each commission was characterized by similar internal dynamics and relationships to external political structures which limited their impact on policy changes.

Because commissions were appointed by political executives and dependent upon them for political and financial support, their recommendations tended to be in accord with the executive's policy goals. When recommendations did conflict with these goals, they were likely to be ignored (as President Johnson ignored the Kerner Commission recommendations he felt were too costly). Individual commissioners were appointed because they represented interests with substantial political resources (e.g., business, government, labor). They were familiar with the bargaining process of American politics and sensitive to each other's interests and to the need for unity within the commissions. Therefore, most recommendations were aimed at interests not represented on the commissions or at jurisdictions outside the authority of the appointing executive (e.g., most of the recommendations of the New Jersey commission were aimed at Newark while most recommendations of the Milwaukee commission were aimed at the state of Wisconsin). In turn, interests under attack established their own commissions which exonerated them and placed the blame for disorders back on interests represented in the original commission. Such competition reduced the legitimacy of recommendations presented by either commission. Because of their ad hoc nature, commissions disbanded before their recommendations came up for legislative action. No lobby pointed out the urgent, crisis nature of these recommendations. Thus, they wound up as routine policy proposals within the context of preexisting partisan conflicts.

Unfortunately, Lipskey and Olson's conclusion that commissions had little substantial effect on race relations is based on a limited perspective. They focus on the immediate effects that commissions had on legislative and administrative action. Only in passing do they discuss some of the possible indirect and long-term effects of the commission process. While they argue that for elites one task of commissions is conflict reduction, they also point out that the number of racial disorders actually *increased* in the years after 1967, suggesting that commissions were not very successful at this task. The authors do not discuss the possibility that this increase may be linked to the failure of commissions to act substantively on the grievances which led to the initial disturbances. And only in the last few pages is there discussion of the long-term consequences of the failure of the political system to redress grievances of minority groups. The 1970s were marked by a steady decline in support for political institutions and an increase in

cynicism about the political process. One reason for this trend could be the failure of political structures to respond to the crisis politics of the 1960s—and riot commissions are a prime example of this failure. In this sense, commission politics of the 1960s could have a profound effect on the future of race relations in America.

The Politics of the Equal Rights Amendment: Conflict and the Decision Process. By Janet K. Boles. New York: Longman, Inc., 1979. Pp. x+214. \$6.95 (paper).

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In 1972, the Equal Rights Amendment received overwhelming support in both houses of Congress and was passed out to the state legislatures, there, it was thought, to be rapidly ratified. By 1978, however, ERA proponents were forced to go back to Washington to fight for an extension of the original seven-year deadline as the required 38-state ratification was still three votes short.

In *The Politics of the Equal Rights Amendment*, Janet K. Boles tells what happened during those intervening years to shift the public perception and legislative assessment of the Equal Rights Amendment from that of a basic principle of equality whose time had come to something that threatened to destroy the basic institutions and moral fiber of American society. As supporters gear up for the final push to get the ERA passed in three more states before the new deadline of June 30, 1982, they may be writing the final chapter to Boles's study of political decision making under conditions of high conflict.

The author attempts to assess the relative explanatory strengths of two academically well-established models of political conflict—the interest group conflict model and the community conflict model—as they apply to the ERA ratification struggle. The former refers to the “insider strategies” of pluralist politics in which lobbying of legislators and pressure group tactics are employed by well-established, usually middle-class groups. Under community conflict conditions, groups are more likely to be ad hoc and single issue, tactics are more likely to involve public confrontation, participants are more likely to have less social status, and the media are likely to play a prominent role in making the conflict visible. The interest group model is offered as a better description of the pro-ERA campaign, especially in its earlier stages, while the community conflict model characterizes the anti-ERA campaign, and, consequently, the tactics that the forces supporting the amendment were forced to adopt as the campaign became an ideological struggle.

Boles uses the case study method and applies the two models to the

amendment's progress in three states where it has met different fates: Texas, where it was ratified on the first vote in 1972; Georgia, where the amendment had not been ratified when the book was written and where anti-ERA consensus is strong; and Illinois, also a nonratified state but one whose legislature has been closely divided on the issue. In each state, key legislators and leaders or spokespersons for both sides were interviewed. In all, 78 interviews were conducted during the early months of 1974.

In Texas, only a few women's groups favoring the ERA attempted to influence legislators. Opposition forces never rallied (until an unsuccessful 1975 move to rescind). Texas presents a pure example of the interest group model of conflict. Although many legislators privately had misgivings about the ERA, in the absence of any opposition, ratification was achieved with limited activity.

The politics of ERA ratification in Georgia can best be viewed from the community conflict perspective (although Boles acknowledges the difficulties of applying a community model to a statewide controversy). Problems arose with coalition formation among groups supporting the amendment but advocating different tactics or soliciting the support of different constituencies. Within the opposition, there has been no official coalition, but Stop ERA, with representation from many conservative groups, has been organized since 1973. Potential sponsors and supporters were neutralized by the organized appearance of the opposition. Hearings and floor debate in the Georgia legislature were lengthy, highly emotional, and frequently irrational.

In Illinois coalitions of support groups, although not always operating in harmony, have been long-lived and well organized. At the same time, opposition forces in Phyllis Schlafly's home state have been evident from the beginning and have been led by a number of politically experienced women. This provides a classic interest group conflict situation with two strong opposing forces. The appearance of ad hoc groups as well, however, suggests that the community conflict model provides additional explanatory power. Thus, Illinois is best viewed as an admixture of the two models.

The application of these models of conflict to the ERA struggle is not always sharply focused. It is not, for example, immediately apparent at what point the struggle in Illinois moved from interest group conflict to community conflict. Was it when the prevailing tactics (of one or both sides) changed? Or when the arena of controversy broadened and formerly peripheral issues became the most widely debated? Why is the ERA victory in Texas labeled a result of interest group *conflict* (rather than interest group pressure or activity), when there was essentially no organized opposition to the amendment?

Despite this shortcoming, the models as presented are useful in providing a framework for understanding the shift in the political and legislative course of the ERA and the formidable roadblocks that have been placed in the path to success of one of the major women's issues in the United States in the last half of the 20th century. Documenting that course and those roadblocks with the input of some of the most active participants on both

sides makes this volume important history as well as good political science. It is straightforward and clear analysis, well suited for classroom use.

The conflict over the Equal Rights Amendment gained momentum nationally and took on the characteristics of the community conflict model as conservative political forces latched onto the issue as one that could gain them new visibility and adherents. The original issues of the amendment became changed, distorted, and simplified. The struggle moved into a much wider public arena, and opposition groups such as Stop ERA, HOW (Happiness of Womanhood), and POW (Protect Our Women) sprang up, frequently with ties to such conservative groups as the John Birch Society, the Daughters of the American Revolution, and the American Party. Boles correctly sees the ERA conflict as an expression of the clash of two important and powerful social movements, feminism and the New Right. While she does not elaborate on the ways in which the New Right has used and distorted the ERA, she does lay important groundwork for further analysis in this direction. Whether or not the Equal Rights Amendment becomes a constitutional addition in the next two years, the ideological battle around the fate and future of the American family and of women's options appears to be only warming up.

To Run after Them. By Louise Lamphere. Tucson: University of Arizona Press, 1977. Pp. xv+230. \$12.50 (cloth); \$6.50 (paper).

Terry Straus and Gwendolen Cates
University of Chicago

Louise Lamphere's *To Run after Them* is a welcome addition to the considerable literature on the Navajo, largest and wealthiest of the Native American peoples. Lamphere combines cultural, social structural, and organizational approaches in analyzing the bases of empirical cooperation in "Copper Canyon," a community in the eastern portion of the reservation where off migration and contact with Anglo society are the greatest. She distinguishes between "native" and "observer's" models but develops an observer's model consistent with the underlying themes of Navajo culture. Her broad approach allows her to deal effectively with persistent problems in Navajo studies, especially with variant and/or apparently conflicting analyses of Navajo social structure.

Lamphere sees Navajo society as becoming more and more heterogeneous and finds traditional anthropological approaches inadequate for representing such a society. She relies on the concept of domestic or developmental cycle to explain many of the apparent inconsistencies in the structural data, and she discovers the utility of "set" and "network" theory in studying the actual recruitment of aid in Copper Canyon. Network theory, developed in the study of heterogeneous urban African groups, has proved especially useful in dealing with societies in which structural approaches have been

unsatisfactory. Lamphere has contributed to the study of networks in an urban black community in the United States (Carol B. Stack, *All Our Kin* [New York: Harper & Row, 1974]), and she suggests the usefulness of the approach for the study of other reservations and, more generally, other small-scale rural ethnic communities. She recognizes "set" and "network" as observer's constructs but argues that, while not conceptualized by the Navajo, they do reflect the process of recruitment of aid within the community. A wealth of empirical case study material is provided to demonstrate strategies of network activation, especially concerning cooperation in livestock and agricultural management, transportation ("getting a ride"), and ceremonials and funerals. These various cooperative activities call forth different sets of relatives: the first, typically, the residence group, the last a larger group of kinsmen. Situational factors determine the actual cooperators from the potential network: for example, the proximity of someone with a car may be more important than kin considerations in an individual's strategy for getting a ride.

Lamphere's interest in the day-to-day problems and activities of contemporary Navajo is well served by the network approach. In her enthusiasm, however, she tends to blur distinctions between category and group, model and "how the system really works." For purposes of analysis she distinguishes Navajo kinship "sets" from clan and neighborhood "network" ties, but this distinction conflicts with native understanding, in which kinship, clanship, and neighborhood are merged, as indicated in the use of classificatory kinship terms. Navajo kinship, clanship, and affinity are determined through women, but in her effort to avoid stale anthropological categories, Lamphere obscures the importance of matriliney and its conceptual foundation. However, she does capture the central concern placed on balancing "autonomy" and "cooperation" in Navajo life, a focus she discovered while observing request making among the members of the community. "To run after them" means "to help them"; it summarizes the Navajo way of conceptualizing the giving and receiving of aid, and it demonstrates well the importance of action and movement in Navajo culture.

Lamphere's book provides an interesting contrast to Gary Witherspoon's *Language and Art in the Navajo Universe* (Ann Arbor: University of Michigan Press, 1977), published in the same year. Where Witherspoon provides a highly complex analysis of Navajo metaphysics with scant empirical exemplification, Lamphere gives us a strong empirical focus with little discussion of higher-level conceptual ordering. Together, the two new books make an excellent pair of resources. More generally, where other studies have identified categories and marveled at their "flexibility," Lamphere begins with that flexibility as the basis of her study; where others have focused on traditional or "memory" culture, Lamphere stands firmly in the present; where others have imposed artificial analytical frameworks, she aims to represent more closely a Navajo view; where others have over-generalized to all Navajo, she states that reservation communities are different and suggests modes of presenting those differences as well as historical background for understanding them.

Stairwell 7: Family Life in the Welfare State. By Neil C. Sandberg. The City and Society, vol. 3. Beverly Hills, Calif.: Sage Publications, 1978. Pp. 239. \$16.50 (cloth); \$7.95 (paper).

Gilbert Steiner

Fairleigh Dickinson University

Neil Sandberg's *Stairwell 7* "is the story of six poor and near poor families and the efforts of the modern welfare state to aid them. The families are black and white, young and old, the handicapped and the victims of floods who need emergency housing. Federal laws give them preferential access to housing at lower than market rentals. . . . Attractive apartments were made available to the six families in the Underhill housing project in Columbia, Maryland, a new town between Baltimore and Washington, D.C. Underhill was constructed under the 236 subsidy program of the Department of Housing and Urban Development (HUD) for low and moderate income families" (p. 15).

The families live in the six apartments located off the same stairwell entrance, Stairwell 7. The entire project houses 106 families and is located in an attractive wooded area adjacent to expensive private homes. Columbia itself began as a planned town in 1967 and had reached a population of about 26,000 by 1973. Its residents were largely upper middle class and highly educated.

Many of the residents of Underhill are part of that group "who are in need but who are neither aged, handicapped, nor parents of minor children, . . . who work at full or part time jobs but who do not earn enough to escape from poverty. . . . [They] move in and out of the welfare sector as their limited incomes place them just beyond eligibility for assistance" (p. 34). The bulk of the book is derived from transcripts of taped interviews with members of the six families and with the manager of the project. Most of the interviews were conducted during the summer of 1973 while Sandberg lived in the development.

Part of the rationale of the Underhill project and of the various agencies assisting these families is to help them regain a self-sufficient status. A secondary goal of Underhill and of its manager was to try to develop a sense of community among the residents. Neither goal was met despite nearly optimal conditions: the physical environment was new, attractive, and reasonably well designed; recreational facilities were available; the community was not only removed from the ghetto, but also was embedded in a larger community of highly successful people. Further, the manager of the project was apparently a tireless, dedicated, idealistic young man wholly committed to the task of building a community in which the human needs of its residents would be met and their lives enhanced.

The author attributes these failures to severe flaws in the machinery of the welfare state, to inadequacies of the project itself, and to interpersonal problems among the residents.

The failures of the welfare machinery are familiar to all who have studied it: overlapping and conflicting regulations of competing agencies, bureaucracy, a distrust of welfare clients, inadequate funds and staff, and so on. The failures within Underhill itself are more nearly unique to the project. Although there was a professed aim of helping the residents return to a state of self-sufficiency, there was no day-care center within the development. Consequently, mothers of young children were severely restricted in their opportunities to earn, or help earn, a living. For a relatively small expenditure, a day-care center could easily have been provided, which would have given employment to some residents and allowed many others to supplement their income. Similarly, despite the project's professed aim of trying to develop community spirit, the manager and residents were consistently ignored in their request for a few park benches. At one point the manager tried to combine this request with another: "With no place for these seniors to sit and chat, and no play area for preschoolers, I've wanted to see this island in the middle of the parking lot become an area where mothers could bring toddlers and where there was a sitting place for seniors. That would have created conversation and involvement across three generations" (pp. 79-80). Again the expense involved was small, and again the need went unmet.

Interpersonal problems included the expected ones attributable to racism, differing cultural backgrounds, noise levels, etc. However, the most serious barrier to building a community seems to have been the sense of shame and guilt most residents felt at having to live in subsidized housing and having to depend on the welfare sector to assist them in other areas as well. All of the residents in Stairwell 7 recalled "better" days and almost all had hopes of a better future, one in which they could be independent of the welfare state. Thus, there was no feeling of permanence at Underhill, no putting down of roots, and a minimal commitment to developing a community.

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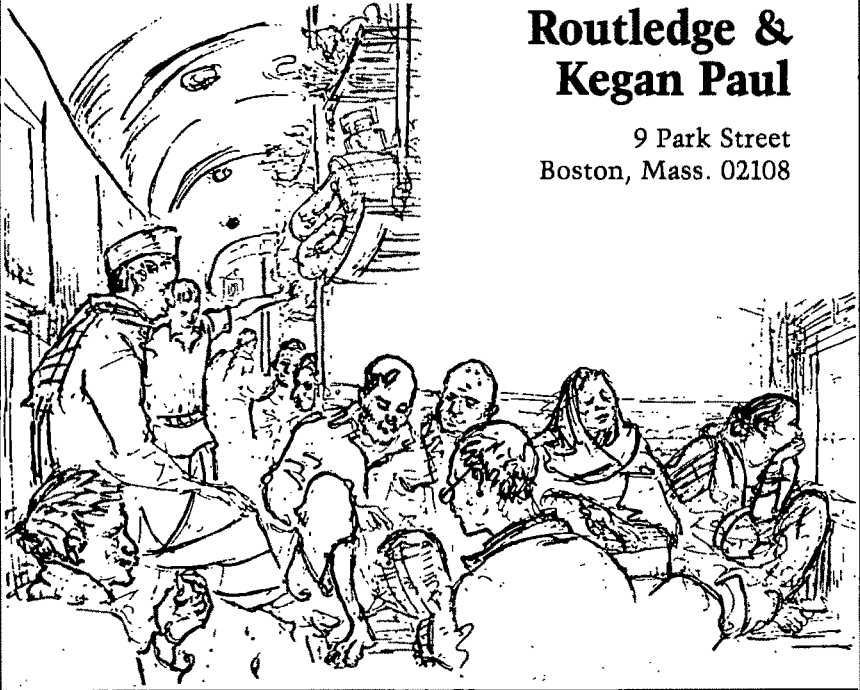
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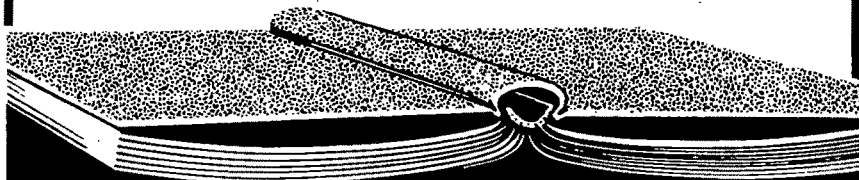
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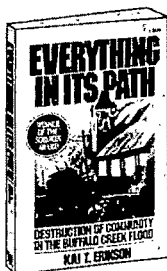
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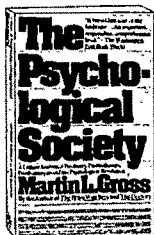
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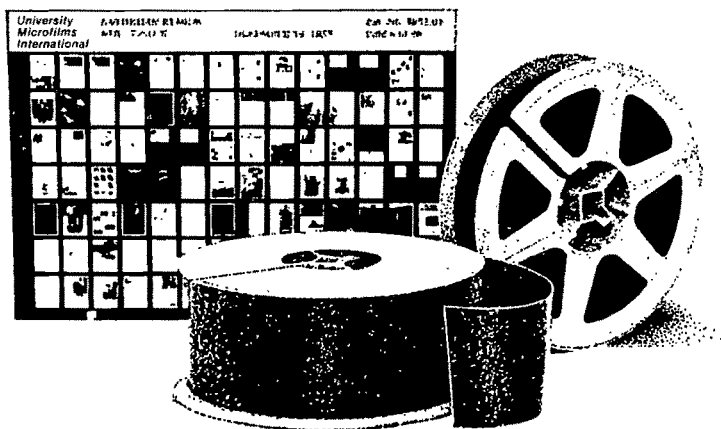
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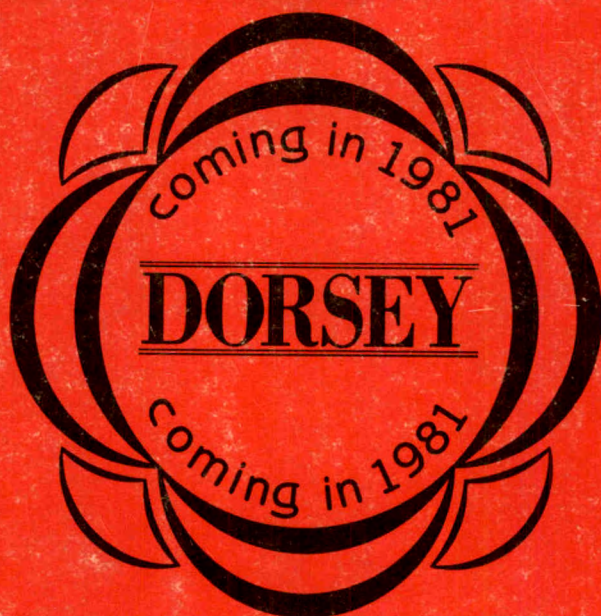
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IN THIS ISSUE

PAUL HUMPHREYS, assistant professor of philosophy at the University of Virginia, was trained in physics, statistics, and philosophy. Author of a number of articles in the logic and philosophy of science, he is interested in the methodologies of the natural and social sciences and is currently working on theories of probabilistic causality.

JOSEPH BERGER is professor of sociology and Chairman of the Department of Sociology at Stanford University. His major research interests are the development of theories for different types of social processes, the development of analytical methods to describe and assess the growth of sociological theories, and the use of sociological theories in applied research.

RANDALL COLLINS is professor of sociology and member of the Center for Advanced Studies at the University of Virginia. His current research interests include the sociology of the emotions, comparative sociology of science, and geopolitics.

SCOTT L. FELD is assistant professor of sociology at State University of New York at Stony Brook. His central research interests involve processes interrelating individual and aggregate social behavior. He is currently writing a book on social structure and social networks that will present theory and empirical findings on clusters and hierarchies of relationships among people in small groups and societies.

VIVIANA A. ZELIZER is assistant professor of sociology at Barnard College and in the graduate faculties of Columbia University. She is the author of *Morals and Markets: The Development of Life Insurance in the United States*. With the support of a Rockefeller Foundation Humanities Fellowship, she is spending this year working on a larger project concerning the changing economic and moral value of children, from which the article in this issue is extracted.

BURKE D. GRANDJEAN, assistant professor of sociology at the University of Virginia, is pursuing further research on stratification in the federal bureaucracy. Another current interest is income inequality at the macro level.

MICHAEL A. FAIA is professor of sociology at the College of William and Mary. Faia teaches everything from statistics to capital punishment, although his students often do not recognize distinctions among his courses. His research focuses on the academic profession, social stratification, and an attempt to formalize something called "dynamic functionalism." He is currently obsessed with ferreting out the implications of D. Premack's finding that, while a rat can be taught to run in order to eat, a rat can also be taught to eat in order to run.

MICHAEL PATRICK ALLEN is associate professor of sociology at Washington State University. His major areas of interest are political sociology, social stratification, and organizations. He is currently working on a study of the evolution of family control in the large corporation.

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3. For more than three authors, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation: (U.S. Bureau of Census 1963, p. 117).

4. With more than one reference to an author in the same year, distinguish them by use of letters (*a*, *b*) attached to the year of publication: (1965*a*).

5. Enclose a series of references with a single pair of parentheses, separated by semicolons.

Format of References: List all items alphabetically by author (providing the full list of multiple authors) and, within author(s), by year of publication, in an appendix titled "References." Examples follow:

Davis, K. 1963*a*. "Social Demography." Pp. 124-37 in *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic.

———. 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29: 345-66.

Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32: 5-19.

Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

Weber, Max. (1921) 1968. "Society's Problems." Pp. 12-16 in *Economy and Society*, edited by Guenther Roth and Claus Wittich. New York: Bedminster.

Errata

On page 145 of "The Deterrent Effect of Capital Punishment: New Evidence on an Old Controversy" by David P. Phillips (*AJS* 86 [July 1980]: 139-48), in line 13, " $17 + 21 = 38$ " should read " $19 + 21 = 40$." The remaining figures in that paragraph and the succeeding one must be adjusted as follows to correct for this error: 29.55 should read 29.73, 21.1 should read 19.5, 59.1 should read 59.5, 35.7% should read 32.8%, 19.4 should read 18.8, 88.65 should read 89.2. Likewise, 35.7% should read 32.8% in the first line of the abstract on page 139 and in line 15 on page 146.

These changes have no effect on any of the results in any of the other sections of the paper, and the statistical significance of each finding remains unchanged.

In "Prohibiting Employment Discrimination: Ideas and Politics in the Congressional Debate over Equal Employment Opportunity Legislation" by Paul Burstein and Margo W. MacLeod (*AJS* 86 [November 1980]), figures 1 and 2 are transposed. The figure on page 516 should appear on page 519; that on page 519 should appear on page 516. (The figure legends are correctly placed.)

Theoretical Consequences of the Status Characteristics Formulation¹

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In this paper we derive five major theorems from the latest version of the status characteristics theory developed by Berger, Fisek, and Norman. The first set of three theorems is concerned with the effect of status characteristics on the degree of equality and inequality that obtains among the members of a task group. Included here is a result that establishes a direct relation between status inconsistency and power and prestige equality. The second set is concerned with the relations between different types of status and task structures. The first of these theorems deals with the relations between task assignments and status expectancies, while the second describes relations between status expectancies and status-task associations. While variants of these theorems have been formulated previously as theoretical assumptions, now for the first time they are shown to be derivable from more basic status organizing principles.

The status characteristics theory was introduced by Berger, Cohen, and Zelditch (1966). In its initial formulation, it was designed to cover the status organizing process in situations involving two interactants oriented toward a single task, with actors possessing only one activated diffuse status characteristic. The second stage of the theory, formulated by Berger and Fisek (1974), enlarged its scope to include situations in which two actors possess any number of salient characteristics. These characteristics were divided into two types: diffuse and specific. The third and current stage, developed by Berger, Fisek, and Norman (1977), covers multicharacteristic situations involving more than two actors and actors of different types. The primary task of this paper is to formulate and derive five major substantive consequences or theorems from this third formulation of the status characteristics theory. These theorems are of considerable importance in understanding the operation of status organizing processes. We believe also that

¹ Research for this paper was supported in part by a grant from the National Science Foundation (GS34182). We are also grateful to Morris Zelditch, Jr., Murray Webster, Jr., and an anonymous referee for valuable comments and suggestions. We are pleased to acknowledge the assistance of Barbara J. Myrom, in particular for her work on the graphics in this paper.

they can provide theoretical bases for devising new ways to intervene in the operation of such processes.

In this article we (1) review the latest version of the status characteristics theory; (2) formulate and derive the five major theoretical consequences of the status theory, which is our primary concern; and (3) briefly consider some of the implications of these results.

Before proceeding it is important to note that we make no attempt in this paper to assess the available evidence for this third formulation nor do we consider possibilities for testing the theory. Such matters merit detailed discussion, and we leave them for a future paper in which they can be dealt with fully and in their own right.²

THE STATUS CHARACTERISTICS THEORY

The theory of status characteristics and expectation states is concerned with describing the operation of status organizing processes. A *status organizing process* is a process by which differences in cognitions and evaluations of individuals or social types of them become the basis of differences in the stable and observable features of social interaction. The patterned effects of sex differences in interaction, race differences, ethnic differences, occupational differences, and the effects of differences in reading ability are all examples of status organizing processes. In each case differences in evaluations and beliefs about types of individuals, such as males versus females or blacks versus whites, are the basis for the emergence of stable and observable inequalities in their behavior. We take the view that in each of these cases where interaction is organized by some particular status distinction or set of status distinctions, a similar status organizing process operates. As a consequence of this theoretical position, the status characteristics theory aims for an abstract and general description of status organizing processes. In turn, it seeks to understand and explain how different specific and concrete status distinctions produce behavioral inequalities by treating these distinctions as instances of the same abstractly conceptualized process.³

In this section we shall provide a description of the theory as well as a

² For a partial review of empirical research which is relevant to the present version of the status characteristics theory, see Berger, Rosenholtz, and Zelditch (1980).

³ For examples of this approach in describing and explaining the effect of specific status distinctions see Meeker and Weitzel-O'Neill (1977), Lockheed and Hall (1976), and Pugh and Wahrman (1978), where *sex* is conceptualized as a status characteristic; Cohen (1972), Cohen and Roper (1972), and Lohman (1972), where the application is to *race*; Cohen and Sharan (1976), Rosenholtz and Cohen (in press), and Yuchtman-Yaar and Semyonov (1979), where the application is to *ethnic* identities and differences; Webster and Driskell (1978), where the application is to differences in *physical attractiveness*; and Stulac (1975) and Rosenholtz (1977), where reputed differences in *reading ability* in classroom situations are analyzed from this perspective.

The Status Characteristics Formulation

formal presentation. This will include, among other things, a specification of the principal terms and relations used in the assumptions, the basic assumptions of the theory, and definitions of terms used in stating our theorems. Throughout the paper, we adhere to the following notational conventions:

p, o (with or without subscripts) refer to actors;

C_i denotes the i th specific status characteristic, with $C_i(+)$, $C_i(-)$ denoting the positively and negatively evaluated states of that characteristic, respectively;

D_i denotes the i th diffuse status characteristic, with states $D_i(+)$, $D_i(-)$;

T is the task outcome, with states $T(+)$, $T(-)$;

C^* is the task instrumental characteristic; and

S , with various superscripts, is a task situation.

A. The Concept of a Status Characteristic

The key concept in the study of status organizing processes is the *status characteristic*. A *status characteristic* is a characteristic of an actor that has two or more states that are differentially evaluated in terms of honor, esteem, or desirability; each state is associated with distinct moral and performance expectations, that is, with stabilized beliefs about how an individual possessing a given state of the characteristic will perform or behave. Expectation states are said to be *specific* if they concern how an individual will act in a clearly defined and specifiable situation; they are said to be *general* if they are not restricted to any specifiable situation. Thus "logical ability" is specific, "intelligence" is general. This gives rise to a distinction between two kinds of status characteristics, *specific* and *diffuse*.

A characteristic is a *specific status characteristic*, C_i , if (1) it involves two or more states that are differentially evaluated and (2) associated with each state is a distinct expectation state. For example, reading ability may function as a specific status characteristic. We distinguish different levels of the characteristic which are differentially evaluated, and we associate beliefs about how individuals possessing the different states will perform specified tasks.

A characteristic is a *diffuse status characteristic*, D_i , if (1) it involves two or more states which are differentially valued; (2) associated with each state are distinct sets of specific expectation states, each themselves evaluated; and (3) associated with each state is a similarly evaluated general expectation state. Thus sex, for example, is a diffuse status characteristic if (1) for a given population the states male and female are differentially evaluated; (2) males (or females) are assumed to be more mechanical, more mathematical than females (or males), hence distinct sets of specific expect-

tation states are associated with the states of the status characteristic; and (3) males (or females) are assumed to be more intelligent than females (or males), hence distinct general expectation states are associated with the states of the status characteristic.

B. Scope Conditions and Principal Relations

The theory is concerned with describing the status organizing process which occurs among a number of actors in a task-oriented situation designated as S^* . The following are the principal components of such situations.

1. Actors who may be either interactants or referents. We conceive of an interaction process in which, during any phase of the process, only two of the actors are involved in interaction with each other. These are the *interactants*. A *referent* is an actor who during a given phase is a noninteractant and whose status information is significant to the interacting pair. At the same time, we look at the situation from the point of view of each of the interactants and analyze their power and prestige position with respect to each other. In this sense the formulation is *p-centric*. In addition, we apply the same analysis to each interacting actor under the assumption that his definition of the situation is being determined by identical processes operating on the same initial task and status information.

2. Actors may possess any number of specific and/or diffuse status characteristics. Interactants must possess at least one status characteristic that either discriminates between them or is relevant to the task. It is assumed that initially the only information the actors have about each other concerns the status characteristics they possess.

3. The actors are addressed to a task, T , under conditions which make it necessary and legitimate to take each other's behavior into account. The task has two oppositely evaluated states. This means that the actors can distinguish or define for themselves a "success" or "failure" outcome. The actors are assumed to believe that there exists a particular characteristic or ability, C^* , that is instrumental to the group task. If an actor possesses the positively evaluated state of C^* , he expects or is expected to attain the success outcome of the task, $T(+)$, while if he possesses the negatively evaluated state, he expects or is expected to achieve the failure outcome of the task, $T(-)$. In other words, success at the task is not simply a matter of "chance" but involves an ability or set of related abilities. In the present analysis, it is assumed that there is a consensus among actors on their definitions of the task outcome states and on the ability or abilities instrumental to the task.

Actors, states of characteristics, and task outcome states can all appear as points or "elements" in the nondirected signed graph which is used as

our mathematical model of the status organizing process.⁴ Our results are formulated in a sufficiently abstract way that they cover situations containing any type of element except where explicitly noted, and we refer the reader to Berger, Fisek, and Norman (1977) for a discussion of the general properties of the elements not discussed here.

Lines between points of the graph can represent the following types of relations.

a) *Possession*. This can hold only between an actor and a state of a characteristic. For example, "*p* is white" or "*p* has high mathematical ability" is taken to mean that *p* possesses a state of a status characteristic.

b) *Relevance*. This is defined as follows: Element *e_i* is relevant to element *e_j* if and only if when *x* possesses *e_i*, then *x* expects or is expected to possess *e_j*. For example, the cultural belief that men are expected to have mechanical ability is taken to mean that a state of the sex characteristic is relevant to the high state of the specific characteristic of mechanical ability. Relevance can hold between any two elements in the graph, except where one or both points represent actors in the situation, or the points represent oppositely evaluated states of the same characteristic, task, or similar types of elements.

c) *Dimensionality*. This exists between elements *e_i* and *e_j* if and only if *e_i* and *e_j* are oppositely evaluated states of the same characteristic and both these states are possessed by actors in the situation. For example, if *p* and *o* are interactants and *p* possesses high and *o* possesses low mechanical ability, a dimensionality relation exists between the high and low states of this status characteristic.

Given that individuals are interacting in the task-oriented situation, *S**, the theoretical task is to describe how "status organizes behavior" under these conditions. This general task can be translated into the following specific theoretical questions.

1. Given that actors possess the states of any number of diffuse and specific status characteristics, what principles determine which of these status elements are admitted into the immediate situation? The answer to this question is given in our *saliency* assumption (Assumption 1 below).

2. Given that the states of a number of possessed status characteristics are admitted by the actors into the situation as usable items of status information, how is this information organized to define their situation? How are the states of characteristics related to their common task? These questions are dealt with in our *burden-of-proof* and *sequencing* assumptions (Assumptions 2 and 3 below).

3. Given that multiple items of status information have been related to each other and to the task, how is this information translated into the

⁴ In addition to these, other types of elements, not directly involved in the derivation of our results, can appear in the graph, e.g., *goal-objects*, *abstract task ability*, *specific task outcome* (see Berger, Fisek, and Norman 1977, pp. 101-2).

actor's behavior? This question is answered in our *aggregated expectation states* and *basic expectation states* assumptions (Assumptions 4 and 5 below).

C. Admitting Status Information into the Situation: Salience of Status Characteristics

The states of a status characteristic possessed by two individuals, say p and o , may be directly or indirectly related to the outcome states of a task. Consider the following examples: (1) If p and o possess high and low mathematical ability, respectively, and their task is to solve mathematical puzzles, states of mathematical ability are directly related to the task. (2) If p is white and o is black, and they believe that race is related in a consistent manner to mathematical ability and are working on mathematical tasks, the status characteristic race is also—though indirectly—related to their task. To cover both kinds of cases, the theory of status characteristics speaks of a path of task relevance. A *path of task relevance* is a path between the actor and the task such that it links the state of the status characteristic possessed by the actor to an outcome state of the task, either success or failure.

A path of relevance provides the actor with information about how well he can expect to perform at the task, given the characteristic he possesses and information about how it is related to the task. These paths can be longer or shorter. In case 1 above we have a shorter path than in case 2, although both connect the actor to the task. Other kinds of paths are possible. Particularly interesting cases are those involving referent actors. For example, (3) if the interactants p and o_1 are black and there exists a referent actor o_2 who is also black and is known by p and o_1 to possess the high state of the ability relevant to the task they face, then race is connected to the task by a path of relevance created by the referent.

Such paths of task relevance are one of the two ways in which status characteristics, whether specific or diffuse, become salient, that is, come to be admitted as usable cues in the immediate social situation. Basically, the theory assumes that if the interactants are connected to the task by a path of task relevance, the status elements and the relations between them become significant in the task situation. Thus, in case 3 the fact that p and o_1 are black and the fact that o_2 , who is also black, possesses the high state of the task ability is status information that becomes salient to the actors. Further, if there exists a path connecting the interactant to the task, the status elements become salient whether or not they discriminate between interactants. Thus, in examples 1 and 2 the status characteristic discriminates between p and o_2 —that is, they possess different states of it. In case 3 the characteristic equates p and o_1 .

However, it does not require an existent path of task relevance to make

The Status Characteristics Formulation

a status characteristic salient. A second and extremely important way in which status characteristics become admitted as usable cues in the immediate social situation is by discriminating the actors. In their search for social cues, interactants will focus on status elements, whether specific or diffuse, which provide a *basis of discrimination* among them providing only that they are not explicitly defined as independent from the task components in the situation. Thus, in a biracial task-oriented group, race becomes salient unless through cultural beliefs and prescriptions that status characteristic has been defined as independent from the task. There is an important difference between the two salience principles of the theory, however: where there is no path of task relevance, only discriminating characteristics become salient. We do not assume that an equating characteristic that is not connected by an existing path to the task will become salient. The principles about saliency are stated in *Assumption 1* (Salience Process):

- (1) Given existing paths connecting an interactant to the outcome states of the group task, the elements and relations in these paths become salient in the task situation; and
- (2) Given status characteristics that provide a basis for discrimination between interactants, the states of these characteristics become salient in the task situation.

D. Completing the Definition of the Situation

1. *The burden-of-proof process.*—As a result of the saliency process, some status states may be connected by paths of relevance to the task's outcome states but some states, those that discriminate between *p* and *o*, may be salient and yet *not* be linked to the outcome states of the group task. Even where a path of relevance exists, it may be so extended—involve so many and such indirect links—that it provides only weak information on which to base expectations for self and other. We assume that if no claim exists or is raised that such status elements are not relevant, the interactants will act as if the information embodied in these status elements is relevant. That is, they act as if the burden of proof lies in showing that the salient status characteristics are not relevant to their task, rather than the other way around. Therefore, unless their inapplicability is demonstrated or justified, status characteristics, and status advantages, will as a matter of normal interaction be applied to ever new tasks and ever new situations.

This burden-of-proof process operates whether the status characteristics are specific or diffuse. In the case of diffuse status characteristics, generalized expectation states (such as "intelligence") associated with the states of the status characteristic become connected with the task ability, *C**, involved in the immediate situation. In the case of specific status characteristics, states of these characteristics imply for individuals possessing them *relevant task outcome states*. That is, they imply the ability to succeed or fail at tasks.

relevant to the specific characteristic. Success or failure at specific types of tasks induces in the actors expectations for more general problem-solving ability, high and low states of *abstract task ability*. This in turn is seen to imply success or failure at the group's particular task. For example, the ability to solve mathematical problems may imply the ability to solve problems in general, including the problem that is confronting the actor.

Burden-of-proof paths will also come into existence when existing paths are so long, so extended, that they provide only very weak information links. The burden-of-proof process will operate in this case if it provides more direct information, that is, if the shortest path generated by the burden-of-proof process is shorter than the existing path of relevance. Existing paths longer than the shortest path generated by the burden-of-proof process are designated as *extended paths*. These ideas concerning the burden-of-proof process are stated in *Assumption 2* (Burden-of-Proof Process):

Given that a salient status element, possessed by or connected to an interactant, is not connected to the task, or is connected by an extended path, then

(1) If the status element is the state of a diffuse status characteristic, the associated generalized expectation state will be activated, and it will become relevant to a similarly evaluated state of C^* .

(2) If the status element is the state of specific characteristic, its relevant task outcome state will be activated. This task outcome state will become relevant to a similarly evaluated state of abstract task ability, and the latter will become relevant to a similarly evaluated outcome state of the group task.

2. *Sequencing of definitions of the situation*.—Saliency and the burden-of-proof process provide p and o with information required to define the immediate task situation. But assume there are more than two actors. In this case, the theory assumes that the definition of the situation proceeds stepwise; that is, any two interactants will define their situation fully as they interact with each other. If the partner of any one actor, say p , then changes so that some formerly inactive person becomes an interactant, further definition occurs if possible and if it is necessary to their interaction. More important, the theory assumes that, for each interactant, definitions achieved vis-à-vis the other in the past remain when a new interactant is engaged in the same situation. These ideas are embodied in *Assumption 3* (Sequencing):

A given structure will be developed through the saliency and burden-of-proof processes for the interacting actors. If a noninteracting actor should later become an interactant, then the structure will be further developed, if necessary, through the operation of the saliency and burden-of-proof processes. For any actor, those parts of his structure completed in relation to a former interactant remain while the actor is in the given situation S .

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3. *The significance, length, and strength of paths of relevance.*—Paths of relevance may differ in terms of their task significance. Some have positive task significance, providing the actor with information which leads him to expect to succeed, while some have negative task significance, providing information that implies task failure. There are three types of relations in paths of relevance: possession, relevance, and dimensionality. Possession and relevance are relations that “link” or “bond together” elements, for example, an actor with the status element he possesses, or one status element to a second status element. Consequently, we treat these relations as signed and positive. Dimensionality, on the other hand, indicates “segregation” or “opposition” of status elements, as in a dimensionality line between the “white” and “black” state of the racial characteristic possessed by actors. We treat this relation as signed and negative. We now can define the sign of a path. The *sign of a path* is positive if the product of the signs of its relations is positive; it is negative if the product of the signs of its relations is negative. Substantively, a positive path means that the actor has a status link to the task that implies that he will attain a particular outcome state, while a negative path implies that he will not attain that state. Thus a path of task relevance is defined as having *positive task significance* if it represents a positive path to a positive task outcome (success) or a negative path to a negative task outcome (failure). Similarly, a path of task relevance has *negative task significance* if it is a positive path to the negative task outcome or a negative path to the positive task outcome. Thus the task significance of a path is the product of the sign of the path and the evaluational sign of the outcome state of the task. To simplify terminology, we will refer to paths of positive task significance as simply positive paths and to paths with negative task significance as negative paths. These ideas on how to determine the sign of a path are summarized in *Rule A* (Sign of a Path):

The sign of a path from an actor to a task outcome is determined by the algebraic product of the signs of the lines constituting the path and the evaluational sign of the task outcome that is the final point of the path.

Paths of relevance differ also in length. The shorter the path, the more closely linked to the group's task is the possessed status characteristic. The longer the path, the more removed from the task is the possessed status characteristic. Consider the following two situations: (1) p possesses high mathematical ability which is relevant to the mathematical puzzles task that p and her partner o_1 are working on; (2) p is a female, o_2 is a referent female actor, o_2 possesses high reading ability which is relevant to high mathematical ability, and p and o_1 are working on a mathematical puzzles task. Clearly the path of relevance from p to the task in the first situation is shorter than that in the second. The status information possessed by p in

the first situation, the fact that she has high mathematical ability, is more closely linked to the task than the status information she possesses in the second situation, the fact that she is a female.

The idea of the length of a path of task relevance can be simply defined. The length N of a path of task relevance from an actor x to a task outcome state is the number of successive relations joining x to the task state, where one relation joins x to some element e_1 , one e_1 to e_2 , . . . , one e_{n-1} to e_n , where e_n is the task outcome state. In the first situation above, the length of the path of task relevance is 2; in the second situation, it is 5.

It is reasonable to assume that the longer the path of relevance between a status element and the task, the less information it provides the actor that he can use in defining the immediate social situation; and that beyond some given length the path will be "ineffective." In addition, some paths will be ineffective because they are redundant. That is, they provide information that duplicates the information contained in other paths. Our ideas about which paths are effective and are therefore to be counted in determining the actor's status-task expectations are given in *Rule B (Effective Paths)* (below, unless otherwise noted, whenever we use the term "path," we mean "effective path"):

The paths effective in determining expectations for an actor x are all paths linking the actor to one of the task outcomes, with the following exceptions.

- (1) Paths of length greater than 6 are not effective.
- (2) If a graph contains a line joining two points, neither of which is an actor, then any path containing a subpath of length 2 or more joining these same two points with an actor is not effective.
- (3) If there is a path connecting an actor to a task outcome, then any second path to the *same* task outcome of equal or greater length and having the *same* sign is not effective if it has more negative lines than the first path.

While it is reasonable to assume that as a path gets extremely long it becomes more difficult for an actor to "work" from the path to achievement expectations, nevertheless, the limit value of length 6 is a simplifying idea. However, it appears to be a very plausible simplification given the existing experimental data (see Berger, Fisek, Norman 1977).⁵

Part 2 of Rule B deals with situations in which paths involving referent actors convey redundant information. Consider this example: p possesses the high state of a status characteristic that is relevant to a high task ability which in turn is relevant to task success. At the same time, some referent actor, o_1 , is known to possess the high state of the status characteristic and high task ability. In this situation we assume that the information about o_1 is redundant to p in the formation of his expectations. Since

⁵ In describing status organizing processes, we on occasion use terms that have phenomenological connotations. Nevertheless, it should be borne in mind that, within our theory, central concepts such as *aggregated expectation states* (see below) are formulated and used strictly as theoretical constructs.

there is a direct relevance relation between the status characteristic and high ability, the path involving that relation will be effective, while the less direct path involving α_1 (with the same sign) is ineffective.

Part 3 of Rule B is based on the substantive notion that people find it hard to work from negatives and will not do so unless they are forced to. At the same time we commonly find a path, for example, with two negatives, which actually introduces no new information not already contained in an existing positive path. In such a case the path with two negatives is not effective.

Paths of relevance of different lengths contribute different amounts to the expectations of an actor—specifically, the longer the path connecting an actor to a task outcome, the weaker its contribution to his performance expectations. We now assume that there are characteristic numbers associated with paths of different length. One can think of these numbers as representing the “degree of task relevance” or the “strength” of a path. However, at this stage we do not commit ourselves to a single substantive interpretation of these numbers. They may refer to the strength of the actor’s expectancy, the strength of his belief, or his degree of confidence that he will attain a particular task outcome based on a particular status-task bond. To capture this idea, we introduce a decreasing function f from the positive integers to the open interval $(0,1)$, $f:N \rightarrow (0,1)$. This $f(\ell)$ is a measure of the “strength” of a path length ℓ .⁶

E. Translating Status Definitions into Behavior

1. *Aggregating expectation states.*—Because of the salience and burden-of-proof processes, the status situation becomes defined for the interactants. How is this status information translated into their behavior?

Each status element the actor possesses is connected to the task by a path of relevance. The task significance of these paths may differ: some of them may establish expectancies for success at the task, while others embody expectancies for task failure. Furthermore, some of the status-task links are closely tied to the task; others are further removed from it. Thus the actor may possess multiple status elements, some of which have positive and some of which have negative task significance, and these status items may differ in the strength of the bond by which they connect the actor to the task.

The basic idea of the theory is that the actor functions like an information-processing mechanism, combining all units of status information to form *aggregated expectation states* for self and other. The information-combining process is governed by what is called the *principle of organized subsets*. The

⁶ For the domain of experimental situations in which theoretical research is conducted, it is reasonable to assume that f is a fixed function. Experimental situations in this domain are highly standardized and our assumption about f is, in the first instance, formulated for this domain.

fundamental idea of the principle of organized subsets is that the actor organizes information within consistent (like-signed) subsets and then combines the valenced subsets. The "signs" to which the principle refers are the signs of the path of relevance connecting the actor to the task. Within like-signed subsets information is built up in accord with an *attenuation principle*: the strength of the subset increases in proportion to the strength of the paths being combined, but the strength produced by adding additional status items is a decreasing function of the strength of existing items in the subset. Thus each subset is assumed to be an organized structure of status information and to exist as such. If there is inconsistent status information, there will, of course, be two such subsets. And for each subset it is possible to determine an expectation value with e_p^+ representing the expectation value for p 's positive subset, and e_p^- representing the value for p 's negative subset. Because of the strong task demands there is pressure on the actors to use all the status information that has become relevant in the situation. As a consequence the actor combines the values of these subsets to form expectations for self, e_p . In a similar manner the actor forms expectations for others. These ideas are embodied in *Assumption 4* (Formation of Aggregated Expectation States):

If an actor x is connected to the outcome state of the group task by sets of positive paths and negative paths, these paths will first be combined within like-sign subsets to yield a positive-path value e_x^+ and a negative-path value e_x^- in the following fashion. Given strengths $f(i), \dots, f(n)$, and $f(i'), \dots, f(n')$ of paths within the positive-paths subset and negative-paths subset, respectively, then $e_x^+ = \{1 - [1 - f(i)] \dots [1 - f(n)]\}$, and $e_x^- = -\{1 - [1 - f(i')] \dots [1 - f(n')]\}$. The aggregated expectation state is then given by $e_x = e_x^+ + e_x^-$.

2. *Expectation advantage and the power-prestige order.*—The observable power-prestige order of the group refers to the distribution among its members of chances to perform, performance outputs, communicated evaluations, and influence. A position A is higher than a position B in this order if A is more likely than B to receive action opportunities, make performance outputs, and have performance outputs positively evaluated but less likely to be influenced in case of disagreement with another. The greater the difference in likelihoods of initiating and receiving these behaviors, the greater the distance between positions A and B. The power-prestige order of the group is assumed by the theory to be a direct function of the expectation states of the actors, and the degree of differentiation between actors in positions A and B is assumed to depend on the expectation difference between the actors. The expectation advantage of the actor, say p in position A, is simply the aggregated expectation state p holds for self minus that which p holds for the actor in position B. These ideas are formulated in *Assumption 5* (Basic Expectation Assumption):

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Given that p has formed aggregated expectation states for self and other, p 's power and prestige position relative to o will be a direct function of the expectation difference between p and o .

Because of the direct functional relation between the actors' expectation difference (or expectation advantage) and their power and prestige positions, the *degree of differentiation* between actors p and o is measured by the expectation difference between p and o : $e_p - e_o$. When $e_p = e_o$, we say p and o are status equals.

Empirical tests of the theory are made possible through application of Assumption 5. Within the general terms of this assumption it is possible to formulate specific functions relating $(e_p - e_o)$ to different power and prestige behaviors. However, an actor's power and prestige position is traditionally indicated by the probability of that actor staying with his own choice when faced with disagreement from another actor. This is the stay-response probability, and we consider it to be a direct function of an actor's expectation advantage.⁷ One specific form for this probability function is given in Berger, Fisek, and Norman (1977). It is important to stress, however, that the theorems in this paper do not depend on the particular form of the function.

Finally, we need to present an additional idea that is part of the theory and that will be used in the derivation of our last two results. Paths of relevance differ in their length and strength in such a way that the longer the path, the less its strength. How can we relate the strength of paths that differ in length? Consider, for example, a number of paths of length 3 in "parallel," that is, paths with the same origin and same sign. A certain number of them, when combined, will have the same effect as a path of length 2. Similarly, a number of paths of length 4 will "add up" to a path of length 3, and so on. We now assume that the number of paths of length ℓ that combine to be equivalent to a path of length $\ell - 1$ is the same for every ℓ , in other words, it is a constant. This constant we designate as k . These ideas are stated in the following assumption (Parallel-Paths Assumption):⁸

Given a number of paths of length $\ell + 1$ in parallel, there exists a positive factor k such that k parallel paths of length $\ell + 1$ have the same effect as a single path of length ℓ . That is, $[1 - f(\ell)] = [1 - f(\ell + 1)]^k$.

At this stage of the theory, we assume that k is a constant (not necessarily integer valued) for all ℓ . However, none of the theorems proved here depend

⁷ For information on a standardized experimental situation that has been developed to measure, among other things, the probability of stay responses, see Berger, Fisek, Norman, and Zelditch (1977), pp. 43-48.

⁸ For those familiar with the theory as presented in Berger, Fisek, and Norman (1977), we point out that the Parallel-Paths Assumption is not a new assumption of the theory. We display it as a separate assumption here simply because of the central role it plays in the proofs of Theorems 4 and 5.

essentially on that assumption, and generalizations of the theory to allow for a variable path combination factor k will still have these theorems as consequences.

DERIVATIONS FROM THE THEORY

The first three theorems that we prove are "differentiation theorems." All three are concerned with conditions related to the degree of equality and inequality that will obtain among the members of a task-oriented group.

As is true of all our theorems, they are formulated in general and abstract terms and are "conditionalized" only in the sense that it is assumed that the task-oriented group is operating under S^* conditions. Each of these theorems stipulates in an exact manner how a particular status condition affects the power and prestige order that exists in the group. It is to be noted that special cases or particular instances of these first three theorems have been taken previously as empirically supported assertions (see Berger, Fisek, Norman, and Zelditch 1977, esp. p. 76). Here we show that these results are in fact derivable from the basic assumptions of the status characteristics theory, and we conceptualize them in their most general form.

Before turning to our theorems, some preliminary comments are in order. Most of the examples we present are expressed in terms of specific status characteristics. It cannot be emphasized too strongly that all five theorems of this section hold for *diffuse* as well as *specific* status characteristics, and for structures containing both kinds. Further, in the first three theorems, states of specific task outcomes, generalized expectation states, or abstract task ability, which we refer to as *activated or induced elements*, can also occur as elements in the paths linking the actors to the task. In what follows, we define a *discriminating status characteristic* as a status characteristic both of whose oppositely evaluated states are possessed by (different) actors in the situation, and the *strength of relevance* of a path of length ℓ as the value $f(\ell)$ of the function f .

Finally, the strength of relevance between a state of a characteristic and a task outcome is the aggregated absolute strength of all paths connecting that state to the task outcome.⁹ States of characteristics A_i are *equally relevant* to the task outcome if the set of path lengths i, \dots, n connecting each state to the task outcomes is the same for each A_i .

A. Status Relevance and Degree of Differentiation

In our first theorem, we are interested in showing that the more direct the relation between a status characteristic and the task outcome, the more

⁹ The specific value and sign of the strength of relevance can be calculated using Assumption 4.

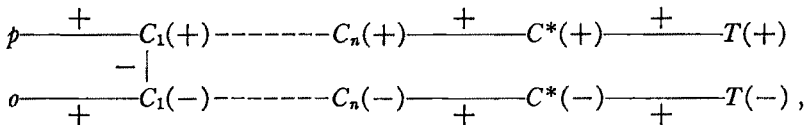
The Status Characteristics Formulation

differentiating is the effect of that characteristic. That is, the greater the strength of relevance between a discriminating status characteristic and the task to which actors address themselves, the more powerful is the effect of that characteristic in creating inequality among the actors in the system.

Consider the following two situations: (1) p and o are males and are known to possess different states of mechanical ability; let us say p has the high and o the low state of the ability; (2) p is male and o is female, and in their culture males are expected to possess high and females low states of mechanical ability. Further, assume that the task confronted by p and o in each situation is one involving mechanical ability. We argue that there is a smaller distance between the discriminating characteristic (states of mechanical ability) and the mechanical task in the first situation than between the discriminating characteristic (states of male and female) and the mechanical task in the second situation. In the first case, as compared with the second, the discriminating characteristic is more immediately relevant to the task. Alternatively, the strength of relevance of mechanical ability to the mechanical task is greater than that of the sexual status characteristic to the mechanical task. Now claiming that the inequality produced by a discriminating characteristic is a direct function of its strength of relevance, we argue that the power and prestige differentiation based on the status characteristic of mechanical ability will be greater than that based on the sexual status characteristic. Stating this relevance inequality relation as a general and abstract theorem, we have

Theorem 1 (relevance strength and differentiation). Given a pair of possessed states of a discriminating status characteristic, the greater the strength of relevance between these states and the task outcomes, the greater the degree of differentiation on that status characteristic.

Proof. We are concerned with the kind of status situation described in the following structure:



where we also allow the situation that $C_1 = C_n = C^*$. Then, as n decreases, the differentiating effect of C_1 increases.

Without loss of generality we may assume that p is connected by positive paths to T . Note also that there is a direct connection between the shortness of a path of task relevance connecting two elements and the strength of relevance between those elements, namely, the shorter the path the greater the strength of relevance.

Consider two situations S^* and S^A , with the strength of relevance of the states of the discriminating characteristic greater in S^* than in S^A . The

proof follows immediately from Assumption 4. This says that for p , the combined strengths of relevance of paths of individual strengths $f(i), \dots, f(n)$ is $\{1 - [1 - f(i)] \dots [1 - f(n)]\} = e_p^+ = e_p$ and $-\{1 - [1 - f(i)] \dots [1 - f(n)]\} = e_o^- = e_o$. Hence $e_p - e_o = 2 \{1 - [1 - f(i)] \dots [1 - f(n)]\}$. Thus as the strength of relevance increases from S^Δ to S^* , the degree of differentiation increases: $e_p^* - e_o^* > e_p^\Delta - e_o^\Delta$.

B. Number of Status Characteristics and Degree of Differentiation

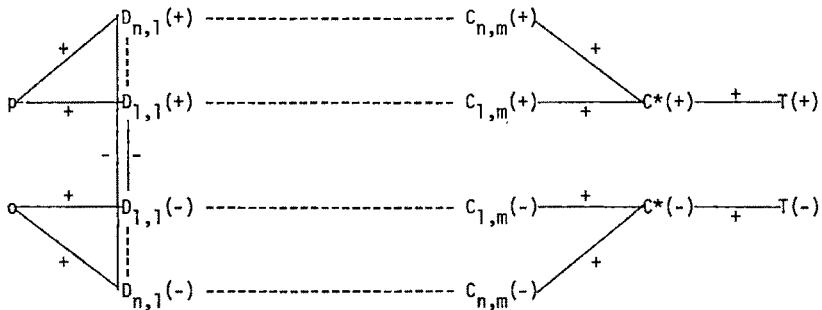
Theorem 2 of our differentiation theorems is concerned with relating the number of characteristics that discriminate between p and o and the magnitude of their status inequality.

If the actors possess common states of characteristics that are salient and there are no discriminating characteristics, then in their situation they will be status equals. As discriminating characteristics are introduced into the situation, status inequality is generated. As an example, imagine the following three situations: in situation one, p and o are males, and males are expected to have high ability at the task to which they are addressed; in situation two, p and o also know that p is a college graduate while o is a high school dropout; and, finally, in situation three, p and o , in addition to the other information, know that p is a professional and o is a semiskilled worker. With this example in mind we can consider the general question of the relation between the number of status characteristics that discriminate between p and o and the degree of differentiation in the system. Prior to the introduction of a discriminating characteristic there is no basis on which to distinguish between the actors in the system. Consequently, the introduction of an initial discriminating characteristic provides the actors with a relatively large amount of new status information. In turn, this new (and novel) status information is the basis for a large increment in status differentiation. Under these conditions the introduction of an *additional* discriminating characteristic, assuming that it is allocated consistently with the initial characteristic, confirms and strengthens the initial status distinction and therefore increases by some amount the differentiation between the actors in the system. However, the additional status distinction provides information which in terms of performance expectations is essentially "more of the same." Consequently, we should expect the increase in differentiation produced by the second characteristic to be less than that produced by the initial status distinction. And in general there should be a decrease in the incremental differentiation produced with each additional status distinction. These features of the relation between the number of discriminating characteristics and status differentiation are consequences of our formulation and are embodied in the second theorem.

The Status Characteristics Formulation

Theorem 2 (number of discriminating characteristics and differentiation). Given that the actors possess states of characteristics that are equally relevant to the task outcome and are consistently allocated, the greater the number of discriminating characteristics, the greater the degree of differentiation. As the number of discriminating characteristics increases, there is a decrease in the incremental differentiation produced by adding characteristics.

Proof. We are considering here situations of the form shown in the graph below.



Once again, p is the actor connected by positive paths to T , and the assumption of consistency entails that all his paths are positive. Let the lengths of the paths connecting p with T via the first discriminating characteristic be $\ell_1, \ell_2, \dots, \ell_m$ ($m \geq 2$ and will depend on the particular structure). Because each discriminating characteristic is equally relevant to the task outcome, each such characteristic will have paths of length ℓ_1, \dots, ℓ_m between p and the states of T . Hence, if there are n^* discriminating characteristics in situation S^* , we have from Assumption 4:

$$\begin{aligned} e_p^* &= \{1 - [1 - f(\ell_1)]^{n^*} [1 - f(\ell_2)]^{n^*} \dots [1 - f(\ell_m)]^{n^*}\} \\ &= 1 - \prod_{i=1}^m x_i^{n^*}, \quad \text{where} \quad x_i = [1 - f(\ell_i)]. \end{aligned}$$

Similarly, $e_o^* = -e_p^*$, and

$$D^* = e_p^* - e_o^* = 2 \left(1 - \prod_{i=1}^m x_i^{n^*} \right). \quad (1)$$

If we increase the number of discriminating characteristics to n^A , keeping them equally relevant to the task outcome, then in such a situation S^A ,

$$D^A = 2 \left(1 - \prod_{i=1}^m x_i^{n^A} \right).$$

Then $D^A > D^*$ follows from $0 < x_i < 1$ and $n^A > n^*$.

We now have to show that for a fixed strength of relevance, the addition of each further characteristic has a decreasing incremental effect. Consider three situations, S^1, S^2, S^3 , for which $n_2 = n_1 + 1$, $n_3 = n_2 + 1$. Then from the general form of (1),

$$\begin{aligned}(D^2 - D^1) - (D^3 - D^2) &= 2 \prod_{i=1}^m x_i^{n_1+2} - 4 \prod_{i=1}^m x_i^{n_1+1} + 2 \prod_{i=1}^m x_i^{n_1} \\ &= 2 \prod_{i=1}^m x_i^{n_1} \left(\prod_{i=1}^m x_i^2 - 2 \prod_{i=1}^m x_i + 1 \right) \\ &= 2 \prod_{i=1}^m x_i^{n_1} \left(1 - \prod_{i=1}^m x_i \right)^2.\end{aligned}$$

Since $0 < x_i < 1$, this is positive, and hence $D^3 - D^2 < D^2 - D^1$, which is the desired result.

C. The Relation between Status Inconsistency and Equality

Our third theorem, the "inconsistency-equality" result, is possibly the most basic of our differentiation theorems. This theorem argues that there is a fundamental relation between the degree of status inconsistency and the degree of differentiation. Specifically, the claim is made that, given a set of discriminating status characteristics possessed by actors in the group, increasing the degree of inconsistency of these characteristics will decrease their differentiation effect; in other words, it will increase the degree of actor equality in the system. Assume, for example, that p and o are discriminated on sex and a performance ability that is a specific status characteristic. These status elements will produce the least amount of actor inequality when the male is believed to have the low state of the performance ability and the female is believed to have the high state, which is the status inconsistent situation. These status elements will generate the greatest amount of inequality when the male is believed to have the high and the female the low state of the performance ability. Similarly, imagine that p and o are discriminated on sex (one is male and the other female) and occupational class (one is a semiskilled worker and the other professional) and that traditional evaluations and expectations obtain for these status distinctions. Also, assume that sex and occupational class are directly connected to the task ability: males and professionals are expected to possess the high state of the ability, while females and semiskilled workers are expected to possess the low state. In this case, if the male is the semiskilled worker and the female is the professional, this inconsistent status configuration will generate the least amount of differentiation from this set of two discriminating characteristics. On the other hand, if the male is the professional and the female is the semiskilled worker, this consistent status pattern will give

The Status Characteristics Formulation

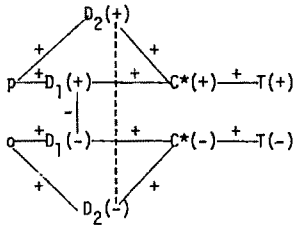
rise to the greatest degree of differentiation to be expected from this set of status characteristics (see fig. 1, in which two dimensionality lines have been dotted for general clarity).

Formulating this inconsistency-equality relation for the general case and in the form of an abstract theorem, we have:

Theorem 3 (status inconsistency and equality). Given a fixed number of possessed states of discriminating status characteristics that are equally relevant to the task outcome, the greater the degree of inconsistency of these status characteristics, the less the degree of differentiation.

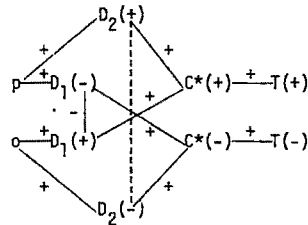
Proof. As usual, we prove the theorem from a p -centric perspective, where p is the actor with the expectation advantage. We are concerned with going from situation S^* to situation S^A , where S^A has a greater degree of inconsistency among the discriminating characteristics. Although it is possible to formulate a specific function for the degree of inconsistency, all we require here is the following. Let A_1, \dots, A_n be the states of discriminating characteristics possessed by p . Then the degree of status inconsistency of the characteristics is a decreasing function of $|n_1 - n_2|$ where n_1 is the number of A_i 's connected to the task outcome via positive paths and n_2 is the number of A_i 's connected to the task outcome by negative paths.

Consistent Status Situation



p is a professional male and o is a semi-skilled female. Sex and occupational class are relevant to task ability. Consistent status characteristics generate greatest amount of inequality between p and o .

Inconsistent Status Situation



p is a semi-skilled male and o is a professional female. Sex and occupational class are relevant to task ability. Inconsistent status characteristics generate least amount of inequality between p and o .

$D_1(\pm)$ = states of occupational class

$D_2(\pm)$ = states of sex characteristic

$C^*(\pm)$ = states of task characteristic or ability

$T(\pm)$ = outcome states of task

FIG. 1

Because the characteristics are discriminating, the degree of inconsistency will be the same for those states possessed by o .

Each discriminating characteristic is equally relevant to the task outcome, in other words, the lengths of the paths connecting a characteristic to the task outcome are the same for each characteristic. Let the lengths of these paths be $\ell_1, \ell_2, \dots, \ell_m$. Hence the strength of relevance for any given positively relevant state is $1 - \prod_{i=1}^m x_i$, by Assumption 4, where $x_i = [1 - f(\ell_i)]$. Thus we have

$$e_p^+ = 1 - \prod_{i=1}^m x_i^{n_1}$$

and

$$e_p^- = - \left(1 - \prod_{i=1}^m x_i^{n_2} \right);$$

hence

$$e_p = \prod_{i=1}^m x_i^{n_2} - \prod_{i=1}^m x_i^{n_1} = \prod_{i=1}^m x_i^{n_2} \left[1 - \prod_{i=1}^m x_i^{(n_1 - n_2)} \right].$$

In the discriminating situations considered, o possesses n_1 states of characteristics which are negatively relevant to T , and n_2 which are positively relevant. Hence

$$e_o = \prod_{i=1}^m x_i^{n_1} - \prod_{i=1}^m x_i^{n_2},$$

and thus

$$e_p - e_o = 2 \prod_{i=1}^m x_i^{n_2} \left[1 - \prod_{i=1}^m x_i^{(n_1 - n_2)} \right]. \quad (2)$$

Consider now situations S^* and S^Δ , where the degree of status inconsistency in S^Δ is greater than in S^* . Thus we have

$$|n_1^* - n_2^*| > |n_1^\Delta - n_2^\Delta|. \quad (3)$$

Because p 's degree of inconsistency will increase to a maximum and then decrease as the number of negative characteristics possessed by p increases, we are concerned here only with cases in which p possesses at least as many positive as negative states. An exactly similar proof can be constructed for an actor having an initial expectation disadvantage diminished by increasing the degree of inconsistency. In both cases the actors will become more nearly status equals with increased inconsistency. Thus we have $n_1^* - n_2^* > n_1^\Delta - n_2^\Delta$. We now have to show that $n_2^* < n_2^\Delta$. Suppose not, that is, suppose $n_2^* \geq n_2^\Delta$. Then because the total number of discriminating characteristics is fixed, $n_1^* + n_2^* = n_1^\Delta + n_2^\Delta$, and so $n_1^* - n_1^\Delta \leq 0$. From $n_1^* - n_2^* > n_1^\Delta - n_2^\Delta$ we have $n_2^* - n_2^\Delta < n_1^* - n_1^\Delta \leq 0$, giving $n_2^* <$

n_2^Δ contrary to our assumption. Thus from $n_1^* - n_2^* > n_1^\Delta - n_2^\Delta$, $n_2^* < n_2^\Delta$, and $0 < x_i < 1$, using equation (2) for S^* and S^Δ , we have $e_p^* - e_o^* > e_p^\Delta - e_o^\Delta$, which is the required result.

D. The Relation between Assignment and Relevance Structures

Our two final theorems are "equivalency theorems." They are concerned with demonstrating that fundamentally different types of information structures are in fact closely related to each other and under certain conditions can be substituted for each other without basically changing the power and prestige relations among the actors in the system.

Within the present formulation of the status characteristics theory, there are a number of different types of structures through which actors may be connected to the states of the instrumental task characteristic, C^* . We shall be concerned with examining three such structures which we designate respectively as *assignment*, *relevance*, and *associational* structures. In Theorem 4 we concern ourselves with the relation between assignment and relevance structures, and in Theorem 5 with the relation between relevance and associational structures. These three structures certainly do not exhaust the ways by which p and o can be connected to the states of the task characteristic in status situations, but they do represent some of the most basic and frequently encountered situations.

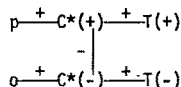
How may actors be connected to states of the instrumental characteristic? First, the actors may actually possess states of the instrumental task characteristic. In such a situation we say that the states of the task characteristic have been *assigned* to p and o , and p and o behave as if they have direct information about their ability to succeed at the task. Assume, for example, that p and o know that p possesses the high and o the low of state C^* . In this situation the information structure is such that p and o behave as if p is competent and o is incompetent with respect to the ability required for their task. In an assignment structure the information about task abilities is noninferential and immediate: p has the ability or does not have the ability.

There is a second basic way actors may be connected to the instrumental task characteristic: p and o may be linked to the states of C^* via a number of other status characteristics, either specific or diffuse. That is, p and o may possess states of these characteristics, which, in turn, are directly relevant to the states of C^* . In such a situation we say that there is a *relevance structure* connecting p and o to the states of the task characteristic. In this case p and o behave as if they have indirect information about their relative abilities to succeed at the task. Imagine, for example, the following situation: p possesses the high states of three specific status characteristics, $C_1(+)$, $C_2(+)$, $C_3(+)$; and o possesses the low states of these characteristics. Further, assume that the states of these characteristics are

relevant in a consistent manner to the states of the task ability. In this situation the information structure is such that p and o behave as if they expect that p will have high and o will have low task ability on the basis of the specific status characteristics they possess. In a relevance structure the information about task ability is nonimmediate and inferential. It is information about anticipations and expectancies: p expects that he has the ability or he expects that he does not have the ability to succeed at the task. (See fig. 2, in which two of the dimensionality lines are dotted for ease of reading.)

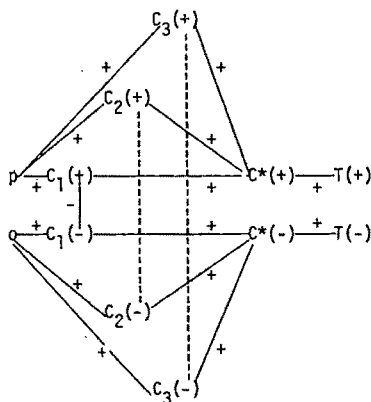
From an informational standpoint, an assignment structure is a structure representing the task abilities attributed to actors, while a relevance structure is a structure of expectancies that the actors hold about their task

Assignment Structure



p possesses high and o low state of task ability. In this situation p behaves as if he is competent and o incompetent on task ability.

Relevance Structure



p possesses the high states of three status characteristics. o possesses the low states of these characteristics. In this situation p behaves as if he expects, on the basis of information in these status characteristics, to be competent and o to be incompetent on task ability.

$C*(\pm)$ = states of task ability

$T(\pm)$ = outcome states of task

$C_i(\pm)$ = states of specific status characteristics

FIG. 2

abilities. We now argue that there exists a crucial relation between these types of structures: for certain basic types of assignment structures there exist corresponding basic types of relevance structures that are, in behavioral terms, approximately equivalent. More precisely, if p and o are status unequals in the given assignment structure, their status inequality will be at least as great in the relevance structure; and if they are status equals in the assignment structure, their status equality will be maintained in the corresponding relevance structure. Put another way, the claim is that, for given basic structures of attributed task abilities, it is possible to specify and construct structures of task expectancies that are, in power and prestige terms, near equivalents.

Sociologists have long held that structures of expectancies are related to structures of task assignments. For example, in an earlier formulation of the status characteristics theory, Berger, Cohen, and Zelditch assumed that under specified conditions, structures of task expectancies are transformed into structures of task assignments (see Berger, Cohen, and Zelditch [1972], in particular, p. 246, assumptions 2 and 3). Our object now is to derive from the more basic principles in this formulation a fundamental relation between these types of structures. To do so we must first introduce some technical concepts which will become part of our "relevance-assignment" theorem.

Definition 1. A *basic assignment structure* is a structure in which the only relevance bonds are those between the states of C^* , the instrumental characteristic, and the respective outcome states of T , the task. Further, there are no referent actors in the structure, and each actor possesses a state of C^* .

Definition 2. A *basic relevance structure* is a structure in which the following conditions are satisfied: (1) there are no referent actors and (2) there is exactly one relevance bond between a possessed admissible element and a state of the instrumental characteristic C^* .

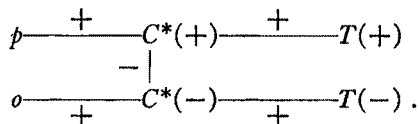
Examples of basic assignment and relevance structures are given in figure 2. We use the term "basic" here just because relevance structures with shortest paths of length 3 are the simplest cases in which explicit expectancies occur in the path of task relevance. In fact, these basic relevance structures cover a large proportion of the socially significant situations where power and prestige orders are established solely on the basis of explicit task expectancies.

The question now is: given a basic structure of representing task abilities (an assignment structure), can we stipulate and construct a corresponding structure of task expectancies (a relevance structure) that is behaviorally equivalent? Theorem 4 states that this is indeed the case.

Theorem 4 (relevance and assignment). Given a basic assignment structure, there is a basic relevance structure in which the degree of differentiation

between the actors is at least as great as it was in the assignment structure. In particular, if the actors are status equals in the assignment structure, there is a relevance structure in which their status equality is maintained.

Proof. There are only three possible basic assignment structures, and for each we can construct the appropriate relevance structure. Case 1:



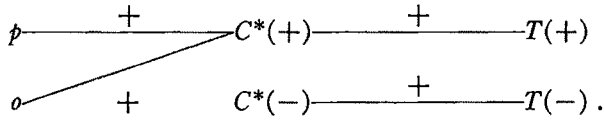
There is one positive path of length 2, and one positive path of length 3 connecting p with task outcome; o has similar negative paths. Now consider a basic relevance structure which is just like the one given in figure 2 but has ' k ' status characteristics, rather than 3, where ' k ' is the smallest integer which is greater than or equal to k , the path combination constant. (Again, we remind the reader that the status characteristics may be diffuse or specific.) For example, if $k = 2.5$, ' k ' = 3. If $k = 4$, ' k ' = 4. We now have ' k ' paths of length 3 and ' k ' paths of length 4 linking p to the task outcome. All of these paths are positive. By the parallel-paths assumption which we discussed at the end of the section The Status Characteristics Theory, k paths of length ℓ running in parallel are equivalent to one path of length $\ell - 1$. Then we have $[1 - f(2)] = [1 - f(3)]^k$, and $[1 - f(3)] = [1 - f(4)]^k$. Hence, as ' k ' $\geq k$, we have $[1 - f(3)]^{[k]} \leq [1 - f(3)]^k = [1 - f(2)]$ and $[1 - f(4)]^{[k]} \leq [1 - f(4)]^k = [1 - f(3)]$. Using Assumption 4 concerning the aggregated expectation state for paths of like signs, we obtain

$$\begin{aligned}
 e_p^{[R]} &= 1 - \{[1 - f(3)]^{[k]}[1 - f(4)]^{[k]}\} \\
 &\geq 1 - \{[1 - f(3)]^k[1 - f(4)]^k\} \\
 &= 1 - \{[1 - f(2)][1 - f(3)]\} \\
 &= e_p^{[A]}.
 \end{aligned}$$

(The superscript $[R]$ indicates quantities connected with the relevance structure; $[A]$ indicates those connected with the assignment structure.) Similarly, as all o 's paths are negative, $e_o^{[R]} \leq e_o^{[A]}$. Hence $e_p^{[R]} - e_o^{[R]} \geq e_p^{[A]} - e_o^{[A]}$.

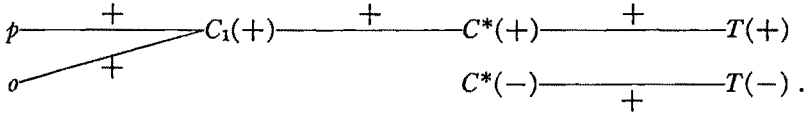
We note two things here. First, the proof makes the standard assumption that k , the path combination factor, is constant. In fact the theorem holds even when k is a function of a path length i , as long as $k(i) \geq 1$. In particular, if $k(i)$ decreases with increasing i , then taking ' k ' = ' $k(3)$ ' in the proof will give the result. Second, it is obvious that status characteristics can only be possessed one by one, whereas k is not necessarily integer valued; this difference leads to the lack of *exact* equivalence when we go from the

assignment to the relevance structure. Case 2:

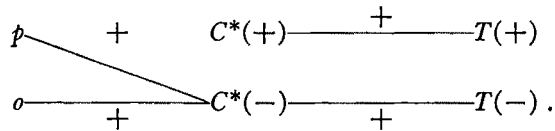


In this case, p and o are undifferentiated, since they both have one positive path connecting them to the outcome.

The following relevance structure will clearly result in the same undifferentiated behavior:



Case 3:



This is dealt with in a way similar to case 2.

E. The Relation between Relevance and Associational Structures

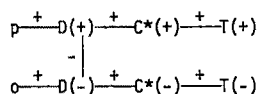
Our fifth theorem is concerned with the relation between relevance structures and associational structures. We have already observed that there are a number of different structures by which actors may be connected to the task characteristic, C^* : in an assignment structure the actors each possess a state of the task characteristic, while in a relevance structure, they possess states of other characteristics that are, in turn, relevant to the states of the task characteristic. We now consider a third basic way in which actors in a status situation may be connected to the states of C^* : they may be linked to those states through referent actors. Specifically, there may exist in the situation referent actors who possess the states of a status characteristic that is possessed by p and o and who also possess the states of the task characteristic. If so, the characteristics the actors possess and the task characteristic are *associated* with each other by virtue of the fact that states of these characteristics happen to be jointly possessed by referent actors in the situation.

Now let us consider two different situations. Situation one is a relevance situation in which p is a male, $D(+)$, and o is a female, $D(-)$, and both expect that p will possess the high state, $C^*(+)$, and o the low state, $C^*(-)$, of the task ability because of the sexual characteristic state they possess. Situation two is an associational structure. In this situation also p is a male,

$D(+)$, and o is a female, $D(-)$. However, there exist six referent actors: o_1 , o_2 , and o_3 are males known to possess the high state of the task ability, $C^*(+)$; and o_4 , o_5 , and o_6 are females known to possess the low state of the task ability, $C^*(-)$. The information that p and o have relating them to the task ability is that, as a matter of contingent fact in their relevant social world, being a male or female is associated with different levels of task ability. What p knows about the relation between sexual status and task ability is: there are others who, like me, are male and are competent and there are others who, like my partner, are female and are incompetent at the task. Thus, in an associational situation, p and o 's structure provides information on which status elements are jointly possessed by actors, and how status elements as a matter of social fact happen to be associated with each other (see fig. 3).

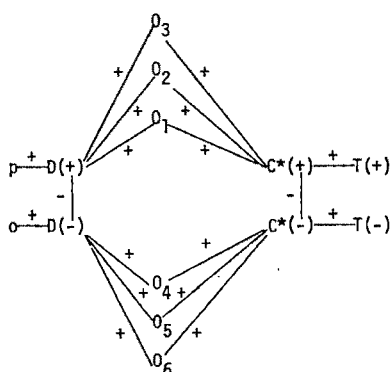
We have said that a relevance structure is a structure of expectancies that the actors hold about task abilities, while an associational structure is a structure of actual status-task ability associations that the actors know exist in their immediate social world. We now argue that these two types of

Relevance Structure



p is a male and o is a female. p expects himself to have high and o to have low task ability on the basis of their sexual status.

Associational Structure



p is a male and o is a female. p knows as a matter of fact that there are three referent males who have high task ability, while there are three other referent females who have low task ability.

$D(\pm)$ = states of sex characteristic

$C^*(\pm)$ = states of task ability

$T(\pm)$ = outcome states of task

o_i = referent actors

FIG. 3

structures can also be shown to be behaviorally related to each other: for certain types of relevance structures there are associational structures that are, in behavioral terms, near equivalents. Again, this is to be understood to mean that if p and o are status unequals in the given relevance structure, their status inequality will be at least as great in the associational structure, and if they are status equals in the relevance structure, their equality is maintained in the corresponding associational structure.

Again it is correct to note that sociologists have previously argued (in one form or another) that structures of actual associations between particular status elements are often related to and give rise to structures of expectancies involving these elements. (For an example of one such argument as it applies to status characteristics and rewards, see Berger, Zelditch, Anderson, and Cohen [1972].) In the present context our objective is again to derive a relation between structures, in this case, between basic relevance and associational structures.

Before stating and proving our "association-relevance" theorem, some further preliminaries are in order.

Definition 3. A *basic associational structure* is a structure in which there is at least one referent actor, the only relevance bonds are between states of C^* and T , and there are no states of activated or induced elements in the structure.

One way of obtaining basic associational structures, which is employed extensively in the proof of Theorem 5, is to replace all relevance bonds between states of characteristics by pairs of possession bonds linking referent actors to those states. Let us call any associational structure gained from a relevance structure by such a method a *derived* associational structure. For example, in figure 3, the associational structure is derived from the given relevance structure by just such a method. Further, that associational structure is symmetric and consistent in the sense of the following two definitions.

Definition 4. A structure is *symmetric* if each path from p to the task is either (1) the same as the path that connects o to the task or (2) matched by a similar path from o differing only in having oppositely evaluated elements.

Definition 5. A structure is *consistent* if it is true for every actor that each of his effective paths to a task outcome state has the same sign.

We restrict the statement of Theorem 5 to consistent, symmetric structures. Examples of the types of structures to which this theorem is applicable were given in figure 3. Although we believe the theorem also holds under other conditions, it has still to be shown for the fully general case.

Theorem 5 (association and relevance). Given a basic relevance structure that is consistent and symmetric, there is an associational structure in which the degree of differentiation between p and o is at least as great as it was in

the relevance structure. In particular, if the actors are status equals in the relevance structure there is an associational structure in which their status equality is maintained.

Proof. Because this is an existence proof, all we have to do is to find *some* associational structure which satisfies the theorem. The construction technique used here has the advantage that it can be extended to more complex situations than basic relevance structures. Thus, although in certain cases it may not give the "minimal" derived associational structure, its generality justifies its use here.

All paths in the type of basic relevance structure considered here are of length 3 or 4. Let the number of paths of length ℓ connecting p to the task outcome be denoted by n_ℓ . By the symmetry of the situation, the number of paths of length ℓ connecting o to T will also be n_ℓ . The actor p is taken as having the expectation advantage. Hence all paths connecting p to T are positive, and all paths connecting o to T are negative. From the definition of a basic relevance structure, there are n_3 paths of the form

$$p \xrightarrow{+} C_i(+) \xrightarrow{+} C^*(+) \xrightarrow{+} T(+)$$

connecting p and T . Replace each relevance bond between $C_i(+)$ and $C^*(+)$ by

$$[k]^1 + \left(\frac{n_4}{n_3}\right)^1$$

referent actors. Each of these actors possesses both $C_i(+)$ and $C^*(+)$. (Here $[x]^1$ is again, for any real number x , the smallest integer greater than or equal to x , and in the situation considered, $n_3 = n_4$.) There will now be at least $kn_3 + n_4$ positive paths of length 4 connecting p and T . By the parallel paths assumption, these have the same effect as n_3 paths of length 3 and n_4 paths of length 4. The aggregated expectation of p in the partially completed associational structure is already at least as great as it was in the relevance structure. Further, it is clear that the resulting structure is still consistent. Thus extra paths induced by new dimensionality bonds being formed, or previous paths for p being lengthened by alterations in o 's path to T , cannot decrease p 's expectation. Thus for p , $e_p^{[Ass]} \geq e_p^{[R]}$, where $[Ass]$ refers to the associational structure. An exactly similar construction is used for paths between o and T . Here, by the addition of extra paths, the expectation of o is made at least as negative as it was in the relevance structure. Thus $e_o^{[Ass]} \leq e_o^{[R]}$. We thus have $e_p^{[Ass]} - e_o^{[Ass]} \geq e_p^{[R]} - e_o^{[R]}$, which is the desired result. For the special case in which p , o are status equals in the relevance structure, it is sufficient to replace each relevance bond between states of C_i and C^* by a single referent actor who possesses the states of C_i and C^* which were previously relevant. Then p , o are clearly still status equals. This completes the proof.

CONCLUSION

Our task in this paper has been to present and prove five theorems that can be derived from the latest version of the status characteristics theory. These theorems are of two types. The first three, our differentiation theorems, are concerned with describing the way status characteristics, the relations between status characteristics, and the relation between status characteristics and the task affect the degree of equality and inequality that obtains among the members of a task group. The last two, our equivalency theorems, are concerned with describing specific interpersonal structures and the relations of these structures to each other. In all cases, our theorems have been formulated in highly general and abstract terms.

One motive for formulating and proving these theorems is to facilitate the task of testing, elaborating, and refining the general theoretical ideas concerned with the operation of status organizing processes from which these theorems are derived. This is in accord with the methodological position that theorem proving can play a major role in the development of theories and theoretical research programs.

Aside from their utility in developing status theory, these theorems are of considerable substantive interest. Not only do they provide us with a deeper understanding of status organizing processes, but they also have fairly obvious social engineering implications. Theorem 3, for example, is the basis for most of the highly effective "expectation training" procedures that have been developed to "overcome" the operation of status characteristics such as race and sex in biracial and mixed-sex groups. In most of these intervention situations, specific status information that is inconsistent with the race and sex assignments is introduced to modify the effect of the diffuse status characteristic (see, e.g., Cohen and Roper 1972; Pugh and Wahrman 1978; and Webster and Driskell 1978). While Theorems 4 and 5 have not up to now been as heavily involved in intervention research, it is clear that they have definite engineering potentialities. Theorem 4, for example, provides us with information on how to produce structures of task assignments among actors by creating structures of expectancies that are behaviorally equivalent. Similarly, Theorem 5 tells us how to produce structures of expectancies among actors by creating structures of status-task associations that are behaviorally equivalent. These structure theorems may provide us with as powerful engineering principles as the inconsistency-equality principles that we have used up to now. Further, our work shows that these are all theoretically derived principles.

One final observation about the logical nature of our theorems. A standard conception of sociological theorems is that they assert a functional connection between variables—that a change in one quantity is associated with a particular type of change in a second. Our differentiation theorems, in fact, are of this form. There is, however, another important type of claim

that a theory can make, and that is an *existence* claim. Our equivalency theorems are of this form. When dealing with interpersonal structures it is important that a theory concerned with such structures be able to make claims concerning the types of structure to which it is committed and the relations between these structures. The fact that this can be done, as demonstrated in this paper, is certainly of specific significance to the status characteristics theory. Moreover, given the widespread concern among sociologists with accounting for the presence of different types of social structures and with describing relations between these structures, the very existence of our results is also of general theoretical significance.

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On the Microfoundations of Macrosociology¹

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Detailed microsociological studies of everyday life activity raise the challenge of making macrosociological concepts fully empirical by translating them into aggregates of micro-events. Micro-evidence and theoretical critiques indicate that human cognitive capacity is limited. Hence actors facing complex contingencies rely largely upon tacit assumptions and routine. The routines of physical property and organizational authority are upheld by actors' tacit monitoring of social coalitions. Individuals continuously negotiate such coalitions in chains of interaction rituals in which conversations create symbols of group membership. Every encounter is a marketplace in which individuals tacitly match conversational and emotional resources acquired from previous encounters. Individuals are motivated to move toward those ritual encounters in which their microresources pay the greatest emotional returns until they reach personal equilibrium points at which their emotional returns stabilize or decline. Large-scale changes in social structure are produced by aggregate changes in the three types of microresources: increases in generalized culture due to new communications media or specialized culture-producing activities; new "technologies" of emotional production; and new particularized cultures (individual reputations) due to dramatic, usually conflictual, events. A method of macrosampling the distribution of microresources is proposed.

Microsociology is the detailed analysis of what people do, say, and think in the actual flow of momentary experience. Macrosociology is the analysis of large-scale and long-term social processes, often treated as self-subsistent entities such as "state," "organization," "class," "economy," "culture," and "society." In recent years there has been an upsurge of "radical" microsociology, that is to say, empirically detailed and/or phenomenologically sophisticated microsociology. Radical microsociology (Garfinkel 1967; Cicourel 1973), as the detailed study of everyday life, emerged partly from the influx of phenomenology into empirical sociology and partly from the application of new research techniques—audio and video recordings—which have made it possible to study real-life interaction in second-by-second detail. This has led to the close analysis of conversation (Sacks, Schegloff, and Jefferson 1974), of nonverbal interactions (Goffman 1971, pp. 3–61), and of the construction and use of organizational records

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(Cicourel 1968; Clegg 1975) and hence to a view of how larger social patterns are constructed out of micro materials.

This radical microsociology, under such labels as "ethnomethodology," "cognitive sociology," "social phenomenology," and others, cuts in a number of different directions. The direction that I would argue is most promising for the advance of sociology as an empirical science is not the phenomenological analysis of concepts but the emphasis upon ultradetailed empirical research. This detailed micro-analysis offers several contributions to the field of sociology in general. One is to give a strong impetus toward translating all macrophenomena into combinations of micro-events. A micro-translation strategy reveals the empirical realities of social structures as patterns of repetitive micro-interaction. Microtranslation thus gives us a picture of the complex levels of abstraction involved in causal explanations.

Another contribution of radical microsociology is its discovery that actual everyday-life microbehavior does not follow rationalist models of cognition and decision making. Instead, social interaction depends upon tacit understandings and agreements not to attempt to explicate what is taken for granted. This implies that explanations in terms of norms, rules, and role taking should be abandoned and that any model of social exchange must be considerably modified. These are large departures from accepted sociological traditions. But these traditions have not been very successful in advancing explanatory principles. I would contend that this is because they have an incorrect model of the actor. What we need, instead, is a micro-mechanism that can explain the repetitive actions that make up social structure such that interactions and their accompanying cognitions rest upon noncognitive bases.

Such a mechanism, I will attempt to show, is provided by *interaction ritual chains*. Such chains of micro-encounters generate the central features of social organization—authority, property, and group membership—by creating and recreating "mythical" cultural symbols and emotional energies. The result of microtranslating all social structure into such interaction ritual chains should be to make microsociology an important tool in explaining both the inertia and the dynamics of macro structure.

THE TIME-SPACE TABLE

It is useful to visualize the empirical basis of micro and macro categories by a time-space table (see table 1). On one dimension are laid out the amounts of time considered by the sociologist, ranging from a few seconds through minutes, hours, days, weeks, months, and up to years and centuries. On the other axis are the numbers of people in physical space one might focus on: beginning with one person in a local bodily space, through small groups, large groups, and aggregates, and up to an overview of all

TABLE 1
TIME AND SPACE AS LEVELS OF SOCIOLOGICAL ANALYSIS

SPACE SCALE	TIME SCALE					
	Seconds (10 ⁰ -1 sec)	Minutes/Hours (10 ⁻¹ -1 sec)	Days (10 ⁰ sec)	Weeks/Months (10 ⁰ sec)	Years (10 ¹ - ² sec)	Centuries (10 ² sec)
One person (1-3 sq ft).....	Cognitive/ emotional processes	Meaningful events Work Repetitive and intermittent behaviors	Careers, life histories	Genealogies
Small group (3-10 ² sq ft).....	Eye-contact studies Microconversational analyses	Rituals Group dynamics Exchanges Bargaining Crowd behavior
Crowd/organization (10 ² -10 ⁴ sq ft)....	Formal organizations	Organizational histories	...
Community (10 ² -10 ⁶ sq ft).....	Social movements	Communities Political, economic, demographic, and stratification patterns (mobility rates, etc.) "cultures"	Long-term social changes
Territorial society (10 ⁴ -10 ⁶ sq ft).....

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the people across a large territory. I have filled in the cells of the table with the kinds of analyses that sociologists make of that particular slice of time and space.

It is clear that the distinction between micro and macro is one of degree and admits of at least two dimensions. All levels of analysis in this table are more micro than those below and to the right of them, and all levels are more macro than those above and to the left. Micro and macro are relative terms in both time and space, and the distinction itself may be regarded as a pair of continuous variables. Moreover, one can see that microanalysis in sociology has recently shifted its level: symbolic interactionism, for example, has traditionally been concerned with situations (although sometimes with more long-term processes—e.g., Becker 1963; Bucher and Strauss 1961; Dalton 1959) located generally on the minutes-to-hours level. Radical microsociologies such as ethnomethodological analysis of conversation or micro-ethological studies of eye movements have shifted the focus to the seconds level (e.g., Schegloff 1967); and phenomenological sociology, in its extreme formulations, verges upon Platonism or mysticism because of its focus on the instantaneous “now” at the left edge of the table.

The strict meaning of “empirical” refers to the upper left-hand corner of the table. You, the reader, sitting at your desk or in your car, or standing by your mailbox, etc., are in that microsituation (or possibly also slightly further down the left-hand column), and it is impossible for anyone ever to be in any empirical situation other than this sort. All macro-evidence, then, is aggregated from such micro-experiences. Moreover, although one can say that all the vertical cells in the far left-hand column are empirical in the (slightly different) sense that they all exist in the physical world of the present, the cells horizontally to the right must be regarded as analysts’ constructs. In the few seconds it takes to read this passage, you the reader are constructing the reality of all those macrocategories insofar as you think of them. This is not to say that they do not also have some empirical referent, but it is a more complex and inferential one than direct micro-experience.

Everyone’s life, experientially, is a sequence of microsituations, and the sum of all sequences of individual experience in the world would constitute all the possible sociological data. Thus the recent introduction of audio- and videotapes by radical microsociologists is a move toward these primary data.

MICROTRANSLATION AS A STRATEGY

There are several advantages in translating all sociological concepts into aggregates of microphenomena. The first point is epistemological. Strictly

speaking, there is no such thing as a "state," an "economy," a "culture," a "social class." There are only collections of individual people acting in particular kinds of microsituations—collections which are characterized thus by a kind of shorthand. This can easily be seen if one examines empirically how researchers go about studying macrosubjects. Researchers themselves never leave their own microsituations; what they do is compile summaries by a series of coding and translating procedures until a text is produced which is taken as representing a macroreality, standing above all the microsituations that produced it (Garfinkel 1967; Cicourel 1975). This is true whether the researcher is relying on conversation with informants or on closed-item questionnaires, or even on direct personal observation. In each case there are a series of tacit summaries between the actual life experiences and the way in which they are finally reported. The same is true to an even larger degree when historical materials are used; such materials are usually constructed from previous written accounts, which even in their original form contain numerous glosses upon the actual flow of minute-by-minute experience.

It is strategically impossible for sociology to do without this kind of macro summary. It would take too much time to recount all the micro-events that make up any large-scale social pattern, and a total recounting in any case would be tedious and unrewarding. Nevertheless, we need not reconcile ourselves to the complete loss of information of the truly empirical level, satisfying ourselves with remote abstractions. For if macrophe-nomena are made up of aggregations and repetitions of many similar micro-events, we can sample these essential microcomponents and use them as the empirical basis of all other sociological constructions.

The significance of the first point, then, is: *Sociological concepts can be made fully empirical only by grounding them in a sample of the typical micro-events that make them up.* The implication is that the ultimate empirical validation of sociological statements depends upon their microtranslation. By this standard, virtually all sociological evidence as yet presented is tentative only. This of course does not mean that it may not be a useful approximation, although this is not always the case. Success at some degree of microtranslation, I would suggest, is the test of whether the macro statement is a good approximation or a misleading reification.²

² To cite a recent example: the controversy over the reputational and decision-making models of community power is a debate over the merits of a more macro and a more micro model. The decision-making model focuses on particular micro-events and claims greater empirical realism. Its advocates criticize the reputational model for taking the hypostatizations and illusions of commonsense discourse as if they were reliable pictures of social realities. Advocates of the reputational method, on the other hand, criticize the decision-making model for missing the larger pattern, and especially that part of it which is hidden by focusing only on actual decisions, ignoring decisions that are never raised, including institutional arrangements which are never challenged

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A second implication is that the *active* agents in any sociological explanation must be microsituational. Social patterns, institutions, and organizations are only abstractions from the behavior of individuals and summaries of the distribution of different microbehaviors in time and space. These abstractions and summaries do not *do* anything; if they seem to indicate a continuous reality it is because the individuals that make them up repeat their microbehaviors many times, and if the "structures" change it is because the individuals who enact them change their microbehaviors.

This is not to say that a causal explanation is totally microsituational. In another paper (Collins, in press), I have attempted to show that the microtranslation of a large body of causal principles leaves, in addition to a number of pure microprinciples, a residue of several types of macroreferences. Individuals within microsituations make macroreferences to other situations, as well as to abstract or reified social entities; the effects of microsituations upon individuals are often cumulative, resulting from repetition of micro-experiences; outside analysts cannot establish microprinciples without comparing across microsituations. There are also three pure macrovariables: the dispersion of individuals in physical *space*; the amount of *time* that social processes take (including temporal patterns of intermittent and repeated behaviors); and the *numbers* of individuals involved. In other words, there are some irreducible macrofactors, but there is only a limited set of them. All varieties of macro structures or events can be translated into these kinds of aggregations of micro-events.

If causality involves stating the conditions under which particular social processes happen, it is apparent that both the independent and dependent variables, "the conditions" and "the social processes which happen," are composite terms. Both, at a minimum, refer to an analyst's selection of repetitive micro-events. Both independent and dependent variables may be further composites in the sense of including a spatial-temporal arrangement of a number of different micro-actors. In addition, more macro samples—"control variables"—must be compared by the analyst to establish any given causal statement.

In any empirical instance, then, to account fully for the behavior observed involves the analyst in comparisons with a wide range of nonpresent situations and with statements linking behavior in one situation with be-

but are implicitly defended by being taken for granted (Backrach and Baratz 1962). The macro theory here promises a greater range of explanatory power but is empirically weaker. Yet it is salvageable by translating it into an aggregate of micro terms. A move in this direction has been accomplished by Laumann, Marsden, and Galaskiewicz (1977), who show a key link between the crude macrosummary of actions involved in reputational power and the actual exercise of that power by demonstrating that there are networks of reputed influentials who actually discuss political matters informally among themselves and thus tend to arrive at a general line of behavior which presumably includes taken-for-granted routines as well as explicit decisions.

havior in other situations. For example, an individual's situational behavior is conditional upon the overall distribution of behaviors in other times and places that can be referred to metaphorically as an organizational "network." But to show such a pattern (and I believe we have shown a number of such patterns, cryptically summarized under such statements as "social class background affects attitudes about *x*" [e.g., Collins 1975, pp. 73–75]) is not yet to show its dynamics; it is only to refer to an observed correlation between behavior in certain kinds of repeated situations and behavior in other situations. We still need to produce the *mechanism* by which conditions—certain arrangements of microsituations—motivate human actors to behave in certain ways. This mechanism should explain both why they behave as they do in specific situations and why they maintain certain dispersions of microbehaviors among themselves, across time and space, thereby making up the macropatterns of social structure. Such a mechanism, moreover, should be able to produce, by different states of its variables, both repetitive behaviors—static or regularly reproduced social structure—and structural changes.

The second implication, then, comes down to this: *the dynamics as well as the inertia in any causal explanation of social structure must be micro-situational; all macroconditions have their effects by impinging upon actors' situational motivations.* Macro-aggregates of microsituations can provide the context and make up the results of such processes, but the actual energy must be microsituational.

It remains to produce such a micromechanism. Here, the substantive research of radical microsociology provides further leads.

THE MICROCRITIQUE OF RATIONALISTIC COGNITIVE AND EXCHANGE MODELS

Much of the classic ethnomethodological research was oriented toward showing that the basic everyday life stance is to take it for granted that meaningful activities are going on. Garfinkel's (1967) breaching experiments indicate that to question or violate the usually tacit aspects of behavior upsets people. They assume there are aspects of life which they should not have to explain. There is also a deeper reason for this reaction: it is in fact impossible to explicate all the tacitly understood grounds of any social convention, and the effort to do so quickly shows people the prospects of an infinite regress of discussion. Cicourel (1973) has shown some of the bases of the "indexicality" of social communications. Many elements of communication involve nonverbal modes which cannot be completely translated into words, and the *activity* of talking itself (as opposed to the content of talk) has a structure that results in verbalizations but is not itself verbalizable. These results imply that meaningful cognitions do not

ultimately guide social behavior; rather, cognitive meaning is usually given to events retrospectively, when some difficulty has arisen which is to be remedied by offering an "account" (Scott and Lyman 1968).

This perspective undercuts a number of conventional explanations of social behavior. Values and norms become dubious constructions. Ethnomethodological research indicates that people are rarely able to verbalize many social rules guiding their behavior. This is especially true at the deeper levels of tacit understanding, such as the circumstances under which particular kinds of surface rules are appropriate (Cicourel 1973). Normative concepts are observed mainly in retrospective accounts or as analysts' constructs; there is no first-hand evidence that they guide actors' spontaneous behavior (see Deutscher 1973; Cancian 1975). Nor is it possible for individuals to operate cognitively simply by matching external situations to mentally formulated rules.³

Similar considerations cast doubt on the adequacy of assuming that behavior is guided by the definition of the situation or by role taking. These concepts imply that behavior is determined cognitively by well-defined verbal ideas. But if the most common stance is to *assume* normalcy as much as possible, even in the absence of discernible meaning, and if meanings are mainly imputed retrospectively as part of some other conversational situation, then immediate situations do not have to be explicitly defined in order for people to act in them. Moreover, if there is an irreducibly tacit element in cognition and communication, situations and roles never *can* be fully defined. What guides interaction, then, must be found on another level.

These difficulties arise again in the case of exchange theories. For the micro-evidence does not show that the usual cognitive stance is one in which actors calculate possible returns; on the contrary, most people most of the time operate on the basis of an assumed normalcy which is not subject to conscious reflection. Comprehensive samplings of conversations in work settings, for example, show that the prevailing tone of most interactions is to take organizational routine for granted; bargaining relations are confined largely to external contacts, as between business heads and clients (Clegg 1975). More fundamentally, the ethnomethodological findings imply that, even where exchanges do take place, they must occur against a background of tacit understandings which are not challenged or even raised

³ Of course, one may rescue the norms or rules as nonverbalizable or unconscious patterns which people manifest in their behavior. But such "norms" are simply observer's constructs. It is a common, but erroneous, sleight-of-hand then to assume that the actors also know and orient their behavior to these "rules." The reason that normative sociologies have made so little progress in the past half century is that they assume that a description of behavior is an explanation of it, whereas in fact the explanatory mechanism is still to be found. It is because of the potential for this kind of abuse that I believe that the terminology of norms ought to be dropped from sociological theory.

to consciousness. Durkheim ([1893] 1947) made a similar point in criticizing social contract theories: any contract, he pointed out, involves one in further obligations not bargained for, such as an implicit obligation to uphold the contract.

Analogous difficulties have arisen within exchange theory itself (Heath 1976). There are certain kinds of calculations which actors cannot make on a purely rational basis. They cannot choose rationally among amounts of two or more alternative goods if there is no common metric; and this is frequently the case in everyday life, as in dealing with such goods as status, comfort, or affection, which have no simple monetary equivalent. The problem is even more acute when one must calculate the expected value of different courses of action, which involves multiplying the probability of attaining a good times its relative desirability; here there are two incommensurable scales to be combined. Yet another difficulty is that the probabilities of attaining one's ends are impossible to calculate for a particular situation in the absence of knowledge of the objective distribution of outcomes. There are further limitations on the applicability of an exchange model: many exchanges, such as those among members of organizational positions, or among persons who have established a bond of repeated gift exchanges, leave no room for bargaining, having excluded alternative partners after a once-and-for-all agreement. The applicability of a model of exchange, then, seems very restricted.

The findings of empirical microsociology and the self-critiques of exchange theories are equivalent and point to the same underlying conditions. If cognition is limited to a few relatively uncomplex operations, then people cannot follow a chain of thought very many steps, either forward to its consequences or back to its premises. Most courses of action must be taken for granted. In March and Simon's (1958) neorationalist reformulation, the only feasible strategy for an actor monitoring a number of complex actions (as in managing an organization) is to "satisfice" in most areas, that is, to ignore most chains of actions, as long as they meet a certain routine level of satisfaction, and concentrate instead on the most unpredictable and irregular area. This is essentially the same procedure that ethnomethodologists find in people's conversational practices. People do not question the truthfulness or pursue the full meaning of most utterances unless severe misunderstandings or conflicts occur, and then they "troubleshoot" by offering retrospective accounts.

Williamson (1975) has drawn some of the consequences for economic theory. Like the ethnomethodologists, he proposes that human rationality is limited and hence that any complex or potentially conflictual negotiations can become exceedingly long and costly—conceivably even interminable—unless there is some tacit or nonnegotiable basis for agreement. Hence, in many circumstances open markets for labor and for goods give way to or-

ganizations, that is, to repeated exchanges at conditions negotiated on a once-only basis. These are economically more efficient than continually re-negotiating relations among workers, or among suppliers and manufacturers, when there are tasks of any degree of complexity to be carried out. This argument is tantamount to claiming that the structural consequence of the cognitive features documented by microsociologists is to replace open-market exchanges with taken-for-granted routines in organizational networks.

Nevertheless, substituting organizations for markets does not eliminate the problem of showing the microfoundations of social structure. Granted that limited rationality makes people rely on routine rather than on bargaining in many areas of life, the question still remains: Why does any particular form of organizational routine exist, and to what extent will it be stable? Any organization involves authority, the power of certain people to give and enforce orders which others carry out. The basis of authority is a chain of communications. The ultimate sanction of a lower-level manager over a worker is to communicate to others in the management hierarchy to withhold the worker's pay; the sanction in a military organization is to communicate orders to apply coercion against any disobedient soldier. The civilian case is founded on the military one; control chains based on pay or other access to property are ultimately backed up by the coercive power of the state. Thus the microbehaviors that make up any organizational routine must involve some sense of the chains of command that can bring sanctions to bear for violating the routine.

Carrying out a routine, then, cannot be a matter of complete obliviousness to possible contingencies. Moreover, there is a good deal of evidence from observational studies of organizations that struggles to exercise or evade control go on among workers and managers, customers and salespersons; that managers negotiate coalitions among themselves; that staff and line officials struggle over influence; that promotions and career lines are subject to considerable maneuver (Roy 1952; Lombard 1955; Dalton 1959; Glaser 1968). Given the nature of power, this is not surprising. Sanctions tend to be remote and take time to apply, and the very conditions of limited cognitive capacities in situations calling for complex coordination or involving uncertainty leave room in the routine for negotiation. Routine may be cognitively desirable, but it is not always forthcoming. When breakdowns occur, prior routine cannot prevent individual actors from negotiating *which* further routines are to be established.

Even when sanctions are applied, the negotiable nature of power itself again becomes apparent. The ultimate basis of property and of private authority is political authority, backed up by the power of the military. Political and military authority, however, are based upon a self-reinforcing process of producing loyalty or disloyalty. A political leader, even of dicta-

torial power, relies upon others to carry out orders; this includes using subordinates to enforce discipline over other subordinates. Hence a leader is powerful to the extent that he or she is widely believed to be powerful, most essentially among those *within* the organizational chain of command (see Schelling 1963, pp. 58–118). For less dictatorial leaders and for informal negotiations at lower levels within organizations, power is even more obviously dependent upon the accumulated confidence of others (Banfield 1961).

Organizational authority, then, is based on shared orientations among the members of a group, directed toward the extent of shared orientation itself. Organizational members monitor what each is feeling toward the other and especially toward those in authority. The ultimate basis of routine is another level of implicit negotiation.

Here we come to the crux of the issue. Both neorationalist self-criticisms and microsociological evidence agree that complex contingencies cannot be calculated rationally, and hence that actors must rely largely on tacit assumptions and organizational routine. But the actual structures of the social world, especially as centered on the networks upholding property and authority, involve continuous monitoring by individuals of each other's group loyalties. Since the social world can involve quite a few lines of authority and sets of coalitions, the task of monitoring them can be extremely complex. How is this possible, given people's inherently limited cognitive capacities?

The solution must be that negotiations are carried out implicitly, on a different level than the use of consciously manipulated verbal symbols. I propose that the mechanism is *emotional* rather than cognitive. Individuals monitor others' attitudes toward social coalitions, and hence toward the degree of support for routines, by feeling the amount of confidence and enthusiasm there is toward certain leaders and activities, or the amount of fear of being attacked by a strong coalition, or the amount of contempt for a weak one. These emotional energies are transmitted by contagion among members of a group, in flows which operate very much like the set of negotiations which produce prices within a market. In this sense, I will attempt to show that the strengths of a market model for linking micro-interactions into macrostructures can be salvaged without incorporating the weaknesses of traditional exchange theories.

SOCIAL STRUCTURE AS MICROREPETITION IN THE PHYSICAL WORLD

From a microviewpoint, what is the "social structure"? In microtranslation, it refers to people's repeated behavior in particular places, using particular physical objects, and communicating by using many of the same symbolic expressions repeatedly with certain other people. The most easily identifi-

able part of this repetition, moreover, is physical: the most enduring repetitions are those around particular places and objects. Most of the repetitive structure of economic organization takes place in particular factories, office buildings, trucks, etc. The most repetitive behaviors that make up the family structure are the facts that certain people inhabit the same dwelling places day after day, that the same men and women sleep in the same beds and touch the same bodies, that the same children are kissed, spanked, and fed. The "state" exists by virtue of there being courtrooms where judges repeatedly sit, headquarters from which police leave to ride in the same squad cars, barracks where troops are repeatedly housed, and assembly halls where congresses of politicians repeatedly gather.

Of course, there is also symbolic communication which goes on among these people, and this bears some relation to the "structuredness" of society. But what I am contending is that the repetitiveness is not to be explained primarily by the *content* of this symbolic communication. The social structure is not a set of meanings that people carry in their heads. I believe that this is borne out by the findings of the empirical microsociology of cognition. The structure is in the repeated *actions* of communicating, not in the contents of what is said; those contents are frequently ambiguous or erroneous, not always mutually understood or fully explicated. People do not always (or even usually) have a very accurate idea of the political state to which they defer, the organization in which they work, or the family or circle of friends with whom they associate. But if the structuredness of society is physical, not cognitive, these disabilities do not prevent us from carrying out a great deal of orderly repetition. No one needs to have a cognitive map of the whole social structure, or even of any organization; all one needs is to negotiate a fairly limited routine in a few physical places and with the particular people usually encountered there.

The limitations upon human cognition documented by the ethnomethodologists show why social order must necessarily be physical and local for all participants. Although this may seem paradoxical in view of the philosophical and antimaterialist themes associated with this intellectual tradition, it is consonant with the main examples of "indexical" statements which ethnomethodologists have cited (Garfinkel 1967): such terms as "you," "me," "here," and "this" are irremediably bound to the specific context, because people's activities always occur at a particular physical location and at a particular time. The inexpressible context upon which everybody depends, and upon which all tacit understandings rest, is the physical world, including everyone's own body, as seen from a particular place within it.⁴

⁴ A phenomenologist would object that individual persons and particular situations cannot be seen simply as physical moments in time and space, because they are always

Again, it is plain that this *physical* social world is not static. People do come and go; homes are formed and dissolved; workers move to new factories and offices; politicians are replaced; new friends meet while others cease to see one another. Nor are the patterns historically constant; indeed, much of what we mean by "structural change" in history is shifting patterns of physical organization: separation of workplaces and armaments places from homes, shifting numbers and shifting rates of turnover of people in political places, and so on. My point here is simply that the micro-reality of any "social structure" is some pattern of repetitive associations among people in relation to particular physical objects and places, and that this must be so because human cognitive capacities do not allow people to organize in any other way.

These cognitive capacities do not prevent individuals from systematically misperceiving the nature of their social order by making claims about it on a symbolic plane. How this is done will be suggested below.

The question now arises: Why is it that people repeatedly inhabit the same buildings, use the same tools, talk to the same people? Part of the answer has already been given: routine occurs because the world is too complex for us to have to renegotiate all of it (or even very much of it) all the time. Most of the time it is easier to stay where one is familiar. But this is only the beginning of an answer. We still need to know why those particular people occupy those particular places. And since they do not stay there forever, we need to know why they move when they do and where they will go. Moreover, the mechanism that explains when they will move (and by the same token, when they will stay) should also be the mechanism that explains just what they will do, both in action and in communication, with the people they repeatedly encounter in their usual places.

From a macroviewpoint, one way to gloss these microrepetitions is to refer to them as property or authority. This brings in the notion of possible sanctions against violating a particular pattern of repetitive behavior. The person who goes into someone else's factory or takes someone else's car stands the risk of being arrested and jailed; the person who fails to carry out a boss's orders risks being fired. Nevertheless, from the view-

defined by a cognitive structure which transcends the immediate situation. In other words, we do not know who the individual is or what the situation is without using some situation-transcending concept. Here again (as in n. 2 above) I believe we encounter a confusion of the theorist-observer's viewpoint and the actor's viewpoint. It is the outside theorist who wants to characterize the individual as a "citizen" or a "husband," or the situation as a "home" or a "workplace." What I am contending is that most of the time actors do not think about such concepts at all; they simply are physically in certain places, carrying out certain actions, including the action of talking to other people. It is only when this physical and emotional routine is disrupted that people rise to the level imputed to them by phenomenological theorists and begin to offer macroconceptual "accounts" of themselves and their settings.

point of strict microtranslation, we must ask: To what extent do people actually think of these contingencies from moment to moment in their lives as they act either to respect property and authority or to violate them? The reality of sanctions upholding property and authority cannot be doubted, as micro-events that *sometimes* occur; but they do not occur very often in relation to the sheer number of micro-events that actually take place. Moreover, the general model of human cognition suggested above is that people do not calculate contingencies or refer to explicit rules most of the time; they act tacitly, and only consciously think of these formalities when an issue arises. Not that people cannot formulate rules or calculate contingencies, but there is no conscious rule about *when* people must bring up the rules and no conscious calculation of when one should calculate and when not (see Cicourel 1973).

What we have instead, I suggest, is a pattern in which people act toward physical objects and toward each other in ways that mostly constitute routines. They do not ordinarily think of these routines as upholding property and authority, although an analytically minded outside observer could describe them as fitting that pattern. People follow routines because they feel natural or appropriate. Moreover, routines may be quite variable with respect to what an observer may describe as property and authority; people can rigidly avoid stepping on someone else's front lawn or they may take the office stationery home, in both cases without consciously thinking about it; they may nervously jump to a boss's request or slough it off behind the boss's back, again without consciously invoking any general formulations of rules or roles. This variation may, of course, also extend to instances where people *do* become property-conscious, rules-conscious, authority-conscious; what I am arguing is that we need an explanation of why this symbolic consciousness occurs when it does. That explanation is again in the realm of feeling: people invoke conscious social concepts at particular times because the emotional dynamics of their lives motivates them to do so.

The underlying emotional dynamics, I propose, centers on feelings of membership in coalitions. Briefly put: property (access to and exclusions from particular physical places and things) is based upon a sense of what kinds of persons do and do not belong where. This is based in turn upon a sense of what groups are powerful enough to punish violators of their claims. Authority is similarly organized: it rests upon a sense of which people are connected to which groups, to coalitions of what extensiveness and of what capacity to enforce the demands of their members upon others. Both of these are variables: there is no inherent, objective entity called "property" or "authority," only the varying senses that people feel at particular places and times of how strong these enforcing coalitions are. There may also be membership groups that make few or no claims to property or authority—purely "informal" or "horizontal" groups, like friends and

acquaintances, whose solidarity is an end in itself as far as its members are concerned.

The most general explanation of human social behavior encompasses all of these variations. It should specify: What makes someone a member of a coalition? What determines the extensiveness of a coalition and the intensity of bonds within it? How do people judge the power of coalitions? The answers to these questions, I am suggesting, determine the way in which groups of friends and other status groups are formed, the degree to which authority and property routines are upheld, and who will dominate others within these patterns. The basic mechanism is a process of emotional group identification that may be described as a set of interaction ritual chains.

A THEORY OF INTERACTION RITUAL CHAINS

From a microtranslation viewpoint, all processes of forming and judging coalition memberships must take place in interaction situations. The main activity in such situations is conversation. But no one situation stands alone. Every individual goes through many situations: indeed, a lifetime is, strictly speaking, a chain of interaction situations. (One might also call it a chain of conversations.) The people one talks to have also talked to other people in the past and will talk to others in the future. Hence an appropriate image of the social world is a bundle of individual chains of interactional experience, crisscrossing each other in space as they flow along in time. The dynamics of coalition membership are produced by the emotional sense individuals have at any one time, due to the tone of the situation they are currently in (or last remember, or shortly anticipate), which in turn is influenced by the previous chains of situations of all participants.

The *manifest* content of an interaction is usually not the emotions it involves. Any conversation, to the extent that it is taken seriously by its participants, focuses their attention on the reality of its contents, the things that are talked about (Goffman 1967, pp. 113-16). This may include a focus on practical work that is being done. What is significant about any conversation from the point of view of social membership, however, is not the content but the extent to which the participants can actually maintain a common activity of focusing on that content. The content is a vehicle for establishing membership. From this viewpoint, any conversation may be looked upon as a ritual. It invokes a common reality, which from a ritual viewpoint may be called a "myth": in this case, whether the conversational myth is true or not is irrelevant. The myth, or content, is a Durkheimian sacred object. It signifies membership in a common group for those who truly respect it. The person who can become successfully engrossed in a conversational reality becomes accepted as a member of the group of those

who believe in that conversational entity. In terms of the Durkheimian model of religious ritual (Durkheim [1912] 1954; see also Goffman 1967), a conversation is a cult in which all believers share a moral solidarity. In fact, it *creates* the reference point of moral solidarity: those who believe are the good; defense of the belief and hence of the group is righteousness; evil is disbelief in, and even more so attack upon, the cognitive symbols that hold the group together. The cognitive symbols, however banal, particularized, or esoteric the conversational content may be, are important to the group and defended by it because they are the vehicle by means of which the group is able to unify itself.

Not all conversations, however, are equally successful rituals. Some bind individuals together more permanently and tightly than others; some conversations do not come off at all. Among those conversations that do succeed in evoking a common reality, some produce a feeling of egalitarian membership among the conversationalists, while others produce feelings of rank differences, including feelings of authority and subordination. These types of variability are, in fact, essential for producing and reproducing stratified social order. Conversational interaction ritual, then, is a mechanism producing *varying* amounts of solidarity, varying degrees of personal identification with coalitions of varying degrees of impressiveness.

What, then, makes a conversational ritual succeed or not, and what kinds of coalitions does it invoke? I suggest the following ingredients. (1) Participants in a successful conversational ritual must be able to invoke a common cognitive reality. Hence they must have similar *conversational* or *cultural resources*.⁵ A successful conversation may also be inequalitarian, in that one person does most of the cultural reality invoking, the others acting as an audience; in this case we have a domination-and-subordination-producing ritual. (2) Participants must also be able to sustain a common emotional tone. At a minimum, they must all want to produce at least momentary solidarity. Again, the emotional participation may be stratified, dividing the group into emotional leaders and followers.

These two ingredients—cultural resources and emotional energies—come from individuals' chains of previous interactional experience and serve to reproduce or change the pattern of interpersonal relations. Among the most important of the patterns reproduced or changed are feelings about persons' relationships to physical property and to the coercive coalitions of authority. How individuals are tied to these coalitions is the crucial determinant of which are dominant or subordinate.

Conversational resources.—Particular styles and topics of conversation imply memberships in different groups. At any time, the previous chain of

⁵ Bourdieu (1977; Bourdieu and Passeron 1977) proposes a similar concept, "cultural capital," although this refers more specifically to the culture legitimated by the dominant class in a society.

interaction rituals which have been successfully negotiated has made certain conversational contents into symbols of solidarity. The range of these contents has been discussed elsewhere (Collins 1975, pp. 114-31). For example, shop talk invokes membership in occupational groups, political and other ideological talk invokes contending political coalitions, entertainment talk invokes groups with various tastes, general discussion invokes different intellectual and nonintellectual strata, while gossip and personal talk invoke specific and sometimes quite intimate memberships. Again, it is not important whether what is said is true or not, but whether it can be said and accepted as a common reality for that moment—that is what makes it an emblem of group membership.

Conversational topics have two different implications for reproducing the social structure. Some conversational topics are *generalized*: they refer to events and entities on some level of abstraction from the immediate and local situation. Talk about techniques, politics, religion, and entertainment is of this sort. The social effect, I would suggest, is to reproduce a sense of what may be called status-group membership: common participation in a horizontally organized cultural community which shares these outlooks and a belief in their importance. Ethnic groups, classes to the extent that they are cultural communities, and many more specialized cultural groups are of this type. Successful conversation on such topics brings about a generalized sense of common membership, although it invokes no specific or personal ties to particular organizations, authority, or property.

Other conversational topics are *particularized*: they refer to specific persons, places, and things. Such talk can include practical instructions (asking someone to do something for someone at a specific time and place), as well as political planning about specific strategies (as in organizational politics) and gossip and personal narration. Some of this particularized talk serves to produce and reproduce informal relations among people (friendships). But particularized talk, paradoxically enough, is also crucial in reproducing property and authority, and hence organizations.⁶ For, as I have argued above, property and authority structures exist as physical routines whose microreality consists of people taking for granted particular people's rights to be in particular buildings, giving orders to particular people, and so on. In this sense, property and authority are reenacted whenever people refer to *someone's* house, *someone's* office, *someone's* car, as well as whenever someone gives an order to do a particular thing, and the listener ac-

⁶ This is contrary to the emphasis in Bernstein's (1971-75) theory of linguistic codes, in which restricted (particularized) codes are seen as the communication mode of the lower classes, while the middle and upper classes use primarily an elaborated (generalized) code. Bernstein's theory focuses only on class cultures and misses the role that particularized talk plays in enacting specific organizations. The higher classes do engage in more generalized talk than the lower classes, but they also engage in particularized talk that is, in fact, crucial for enacting the organizations they control.

knowledges the reality, at least for that moment, of that order. Again, it is worth pointing out that orders are not always carried out, but it is the situation in which the communicative ritual occurs that is crucial for maintaining the structure as a real social pattern, not the consequences for practical action.⁷

Of course, as indicated, even the degree of ritual compliance is a variable, and we must inquire into the conditions which make people respect and enact organizational communications less or more enthusiastically or even rebel against them. This brings us to the second ingredient of rituals, emotional energies.

*Emotional energies.*⁸—Emotions affect ritual membership in several ways. There must be at least a minimal degree of common mood among interactants if a conversational ritual is to succeed in invoking a shared reality. The stronger the common emotional tone, the more real the invoked topic will seem to be and the greater the solidarity in the group (see Collins 1975, pp. 94–95, 153–54). Emotional propensities are thus a prerequisite for a successful interaction. But the interaction also serves as a machine for intensifying emotion and for generating new emotional tones and solidarities. Thus emotional energies are important results of interactions at any point in the ritual chain. Emotional solidarity, I would suggest, is the payoff that favorable conversational resources can produce for an individual.

If successful interactional rituals (IRs) produce feelings of solidarity, stratification both within and among coalitions is a further outcome of emotional flows along IR chains. As noted, conversational rituals can be either egalitarian or asymmetrical. Both types have stratifying implications. Egalitarian rituals are stratifying in that insiders are accepted and outsiders rejected; here stratification exists in the form of a coalition against excluded individuals, or possibly the domination of one coalition over another. Asymmetrical conversations, in which one individual sets the energy tones (and invokes the cultural reality) while the others are an audience, are internally stratified.

The most basic emotional ingredient in interactions, I would suggest, is a minimal tone of positive sentiment toward the other. The solidarity sentiments range from a minimal display of nonhostility to warm mutual liking and enthusiastic common activity. Where do such emotions come from? They originate in previous experiences in IR chains. An individual who is successfully accepted into an interaction acquires an increment of positive

⁷ This, I believe, is the significance of Goffman's (1959) concept of frontstage behavior in organizations. Enunciation of *rules*, then, is a special type of frontstage enactment; its significance is not that the organizational rules directly cause behavior but that rules are *conversational topics* that are sometimes invoked as crucial tests of feelings of members toward authority coalitions in organizations.

⁸ Some alternative theories of emotion are given in Kemper (1978), Schott (1979), and Hochschild (1979).

emotional energy. This energy is manifested as what we commonly call confidence, warmth, and enthusiasm. Acquiring this in one situation, an individual has more emotional resources for successfully negotiating solidarity in the next interaction. Such chains, both positive and negative, extend throughout every person's lifetime.⁹

Let us consider the variations possible within this basic model. The main conditions which produce emotional energy are these:

a) Increased emotional confidence is produced by every experience of successfully negotiating a membership ritual. Decreased emotional confidence results from rejection or lack of success.

b) The more powerful the group within which one successfully negotiates ritual solidarity, the greater the emotional confidence one receives from it. The power of a group here means the amount of physical property it successfully claims access to, the sheer number of its adherents, and the amount of physical force (numbers of fighters, instruments of violence) it has access to.

c) The more intense the emotional arousal within an IR, the more emotional energy an individual receives from participating in it. A group situation with a high degree of enthusiasm thus generates large emotional increments for individuals. High degrees of emotional arousal are created especially by IRs that include an element of conflict against outsiders: either an actual fight, a ritual punishment of offenders, or, on a lower level of intensity, symbolic denunciation of enemies (including conversational griping).

d) Taking a dominant position within an IR increases one's emotional energies. Taking a subordinate position reduces one's emotional energies; the more extreme the subordination, the greater the energy reduction.

INTERACTIONS AS MARKETPLACES FOR CULTURAL AND EMOTIONAL RESOURCES

Why will a particular person, in any given interactional situation, achieve or fail to achieve ritual membership? And why will particular persons dominate or be subordinated in an IR? The answers lie in a combination of the emotional and cultural resources of all the participants in any encounter. These in turn result from the IR chains that each individual has previously experienced. Each encounter is like a marketplace in which these resources are implicitly compared and conversational rituals of various degrees of solidarity and stratification are negotiated. Each individual's "market" po-

⁹ This does not imply an infinite regress in the past; it points to the important fact that human children are born into an *emotional* interaction and that successive emotional states build upon the initial one.

sition depends upon the emotional and cultural resources acquired from previous interactions.

The several kinds of emotional and cultural resources interact. Since emotional energies result from success or failure in previous IRs, having high or low cultural resources also contributes to high or low emotional energies. To a lesser extent there is an effect in the opposite direction: the more emotional energy (confidence, social warmth) one has, the more one is able to gain new cultural resources by successfully entering into new conversations, whereas a person with low emotional confidence may be "tongue-tied," unable to use even what cultural resources he or she has.

Both cultural and emotional resources change over time. But they change in different rhythms. Generally speaking, I would suggest that emotional energies are much more volatile than cultural resources and that they can change in both positive and negative directions. If one encounters a series of situations in which one is highly accepted or even dominating, or in which the emotions are very intense, one's emotional energy can build up very rapidly. The rhythms of mass political and religious movements are based upon just such dynamics. On the other hand, if one goes through a series of ritual rejections or subordinations, one's energies can drop fairly rapidly.

Cultural resources, however, are fairly stable, and they change largely in a positive direction. But here we must pay attention to the distinction between generalized and particularized cultural resources. Generalized resources usually grow over time and at a slow rate. Individuals may forget some of the generalized information they possess, but since it is often reproduced as common conversational topics in their usual encounters with other people, loss of generalized cultural capital is probably confined to those occasions in which someone leaves a habitual milieu of conversational partners for a long time. And even so, there is a considerable lag; the power of memory makes generalized cultural resources a stabilizing force in social relations.

Particularized cultural resources, on the other hand, are potentially more discontinuous. Particularized conversational actions (giving a specific order, asking practical advice, negotiating a strategy regarding a particular issue in organizational politics, joking with friends, etc.) are evanescent. The bonds they enact are permanent only to the extent that those actions are frequently reproduced. Particularized cultural resources are especially important as the microbasis of property, authority, and organization, as well as of close personal ties. The relationship of people to particular physical objects that constitute property is enacted over and over again in ordinary and taken-for-granted encounters, in IRs which have a particularized content. The same is true of the microreproduction of authority and of organizations.

Particularized conversational resources differ from generalized conversational resources, and from emotional resources as well, in that they not only are acquired in one's *own* conversations but also circulate independently of oneself. When other people talk particularistically about some individual, they are constituting her or his reputation. One's reputation, then, is a particularized conversational resource that circulates in other people's conversations. For the microtranslation of macrostructures, the most important kind of reputations that circulate are simply the parts of talk which identify someone by a particular title ("the chairman," "his wife") or organizational membership ("he is with G.E."), or which tacitly give someone a reputation for certain property and authority ("I went into his office," "She sent out a memo directing them to . . ."). Particularized conversation, both as enacted and as circulated secondarily as reputations of other people, is what principally constitutes the social structure of property and authority.

Compared with generalized conversation, particularized conversation is potentially quite volatile, although much of the time it simply reproduces itself and hence reproduces social routines. Most of the time, the same people are placed into organizational and property-maintaining routines by both the particularized conversational rituals in which they take part and those in which they are conversational subjects. But this flow of particularistic cultural resources *can* shift quite abruptly, especially on the reputational side. On a small and local scale, this happens frequently: a new person enters a job, a familiar one leaves a place—the old round of particularized conversational enactments and reputations suddenly stops and a new particular social reality is promulgated. Most of the time these particularized items of conversation reinforce the bedrock of physical routine, which human cognitive capacities require us to rely upon to such a great extent. But by the same token, the particular structure of organizational behavior, including very large organizational aggregates such as the state, is potentially very volatile: it is not upheld by generalized rules or generalized culture of any kind, but by short-term, particularized interaction rituals, and these can abruptly take on a new content. This microbasis of property and authority implies that these routines alternate between long periods of relatively stable microreproduction and dramatic episodes of upheaval or revolution.

If we ask, then, what causes the variations in this pattern—when will particular individuals move in or out, and when will the whole pattern of property and authority be stable or shift—we find a market-like dynamic. Particular individuals enact the property and authority structure because their previous IR chains give them certain emotional energies and cultural resources, including the resource of the reputation for belonging in certain authority rituals and particular physical places. The relative value of these

resources may shift from encounter to encounter, as the combinations of individuals vary. If one begins to encounter persons whose emotional and cultural (including reputational) resources are greater or less than what one is used to, one's own capacity to generate ritual membership and conversational dominance will shift up or down. Hence one's emotional energies will undergo an increase or decrease. If these energy shifts reach the point at which one is motivated and able to shift physical and ritual position in the pattern of property and organizational authority, one's reputation and other particularized conversational resources will abruptly shift. Generalized cultural resources, finally, may build up across a long series of interactions, but this occurs relatively slowly.

Although IR situations are market-like, it is worth stressing that the mechanism by which individuals are motivated by their market positions is not one of rational calculation. As noted above, a fundamental difficulty in rationalist social exchange models is that there is no way for individuals to compare disparate goods having no common metric, nor is it possible to multiply these values by the different metric of a scale of probabilities of attaining various goods. But if individuals are motivated by their emotional energies as these shift from situation to situation, the sheer amount of emotional energy is the common denominator deciding the attractiveness of various alternatives, as well as a predictor of whether an individual will actually attain any of them. Individuals thus do not have to calculate probabilities in order to feel varying degrees of confidence in different outcomes. Disparate goods do not have to be directly compared, only the emotional tone of situations in which they are available.¹⁰ Nor do actors have to calculate the value of their various cultural resources (generalized and particularized) in each situation. These resources have an automatic effect upon the conversational interaction, and the outcomes are automatically transformed into increments or decrements of emotional energy.

The fundamental mechanism, then, is not a conscious one. Rather, consciousness, in the form of cultural resources, is a series of inputs into each situation which affects one's *sense* of available group memberships of varying degrees of attractiveness. It is possible, of course, for individuals sometimes to reflect consciously upon their social choices, perhaps even to become aware of their own cultural and emotional resources vis-à-vis those of their fellows. But choices consciously made, I would contend, would be the

¹⁰ There may be occasions, of course, in which individuals find themselves among disparate sources of attraction or repulsion which are evenly balanced. In those cases, the IR chain theory predicts that their behavior will in fact be immobilized—they will remain in whatever physical routine they are in at that time, until the flow of IR energy combinations with other actors motivates them to leave that routine.

same as choices made without reflection.¹¹ One's sense of "choice" or "will" rests upon the accretion of energies—one's degree of self-confidence—which is the product of a larger dynamic.

Another long-standing difficulty of social exchange theories is solved by the IR chain model: Why do people repay a gift? Self-interest is not a sufficient explanation, as an exchange is rewarding only to the extent that individuals *already* know there will be reciprocity. Hence theorists have felt it necessary to fall back upon such claims as "what is customary becomes obligatory" (Blau 1964) or to invoke an alleged "norm of reciprocity" (Gouldner 1960; see also Heath 1976). Both formulations beg the explanatory question: in both cases, the customariness of the behavior is just what remains to be explained, and to call this customariness a "norm" is merely to describe it. The IR chain model proposes that feelings of solidarity within a social coalition are fundamental. If two individuals feel a common membership, they will feel a desire to reciprocate gifts, because the gift and its reciprocation are emblems of continuing their common membership. This model has the advantage of making gift giving and reciprocation into a variable instead of a constant: individuals will reciprocate to the extent that the emotional dynamics of a particular coalition membership is attractive to them. Similarly, they will feel like giving gifts or not because of the same range of circumstances. Hence the variables described above should account for the degree to which reciprocity is actually practiced.

The aggregate of IRs, then, may be described as market-like. What happens in each encounter is affected by what has happened in the recent series of encounters in each participant's IR chain, and what happened in those encounters in turn was affected by the recent experiences of *their* participants, and so on. This larger aggregate of encounters produces what may be described as a series of cultural and emotional "prices" at which individuals can negotiate IRs of different degrees of solidarity and domination with one another. I say a *series* of prices because only certain combinations of individuals can successfully create a ritual, and different combinations will settle upon deals at different prices.

There are several different markets of this kind operating simultaneously. At one level, there is a relatively slow-moving market for organizational

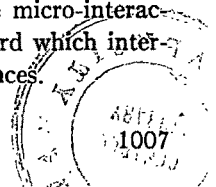
¹¹ Hochschild (1979) shows that people do sometimes reflect upon their emotions and try to make themselves feel in particular ways that are appropriate to the situation. The fact that they do *not* automatically feel the "right" way is explainable, I would argue, by the market attraction or repulsion of various alternative situations in their IR chains. What Hochschild is describing, then, may be situations in which individuals are torn between two different forms of resources or are getting very mixed payoffs from their immediate interactions. Such situations may arise when an individual's market position is shifting away from a previous equilibrium point and a new equilibrium has not been established (see discussion below).

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ritual repetitions ("positions") and for other property enactments. There is a great deal of repetition in the microrituals that make up the reproduction of such structures; yet individuals do try to move in or out of positions. Their motivations to stay put or to move, and the chances of being accepted when they attempt these actions, are determined by the aggregate of IR chains with which their lives physically intersect. Informal shifts within organizational relations are similarly determined—shifts in which bosses gain or lose influence, informal allies win or lose, workers show greater or less enthusiasm and compliance. At another level, there are markets for personal friendships, for horizontal coalitions among different organizational executives, etc., which are not tied to the direct enactment of property and authority relations between the participants. These markets are capable of moving much more quickly and continuously than those in which organizational "structures" are enacted, because informal conversational partners do not have to change the more complex and particularity-embedded ties of property and organizational position.

Both types of markets, however, operate by a similar mechanism. In the "organizational position market," individuals will be motivated to press for more domination within the organizational routine or to leave that routine to find a better one to the extent that their aggregate of experiences in IR chains is emotionally positive. Similarly, in markets for horizontal alliances (whether personal or business/political), individuals who experience relative surplus of emotional energy over those in their usual encounters will be motivated either to seek more domination or to move to a different set of encounters. But such individuals will eventually tend to reach the limiting situations to which their resources will take them: situations in which their partners are equal or higher in resources, hence stabilizing or reversing their emotional surplus.

From a very abstract viewpoint, one can imagine an equilibrium point in such markets at which all individuals have settled on the particular people to interact with ritually, so that all emotional and cultural resources are statically reproduced. Such an equilibrium point may be a useful concept, but only if we see it as merely one tendency of aggregate interactional markets which is modified by a number of other processes. The situation is constantly being destabilized, whenever any individuals anywhere experience new increments (or decrements) of cultural resources and emotional energies. A particular boss who is losing emotional energies (through ill health, let us say, or a shift in family interactions) will bring about small increments in energies among the workers he or she routinely dominates, which in turn may increase their influence in other encounters. Such effects will cause at least local destabilization of the micro-interactional equilibrium. The equilibrium point is a pattern toward which interactions will tend again and again, subject to these disturbances.



Many of these disturbances will be local and temporary; their outcomes do not change the pattern of macroorganization. Others, however, may be large-scale and pervasive in their consequences. In the following section, I consider what kinds of aggregate microprocesses can cause either gross reproduction or gross change in macrostructures.

MACROSTRUCTURAL EFFECTS

The preceding model suggests that large-scale social changes are based on micromechanisms of one or more of the following kinds: large-scale changes in the amount or distribution of (a) generalized cultural resources, (b) particularized cultural resources, or (c) emotional energies.

a) The *generalized cultural resources* across a large population can shift because of the introduction of new technologies of communication or because more individuals specialize in the production and dissemination of generalized culture. Writing implements, mass media, and educational and religious organizations of varying size have introduced new cultural resources, or increased their distribution, in societies at various times in history. One can picture at least two kinds of resultant structural effects. First, the distribution of the expanded culture may be concentrated in particular populations; hence these will be able to raise their level of success in IRs at the expense of others, forming new organizational ties and thereby eventually developing emotional and reputational advantages. A second kind of effect occurs when the whole population uniformly receives an increase in generalized cultural resources; the sheer degree of mobilization, of efforts to negotiate new IR connections, should increase throughout the society. Although no one gains relative to others, the overall process should increase the amount of organization building generally in that society. It can be suggested that early phases of this process contribute to economic booms and to the growth of political and/or religious movements; later phases, however, if generalized cultural currency continuously expands, may involve a devaluation of the cultural currency, with ensuing contraction of political and economic activity (Collins 1979).

b) *Particularized cultural resources* define individuals relative to particular physical properties and authority coalitions. What can change the whole structure of these resources? The volatile aspect of particularized culture, I would suggest, is especially important for the reputations of the individuals who ritually enact the most powerful coalitions. Most reputational talk, as indicated, is local and repetitive. But rapid upheavals in personal reputations characterize important shifts in political and religious power. Persons become powerful (or "charismatic") when a dramatic event, usually involving success in a conflict, makes large numbers of people focus on them. The widespread and rapid circulation of their new reputation gives

them the self-reinforcing power of commanding the largest, and therefore dominant, coalition in that society. Conversely, powerful persons usually fall because of dramatic events—scandals or defeats in conflicts—which suddenly circulate their *negative* reputation.

The movement of such particularized cultural resources has several implications for the dynamics of social change. Such changes are discontinuous and alternate with periods of routine. They depend upon dramatic events that are highly visible to many people. The most dramatic events, I would contend, are conflicts, and especially violent ones. It is for this reason that wars are so important in mobilizing revolutions and other rapid social changes (see Skocpol 1979). Politics itself is a master determinant of the property system, and so many other routine aspects of social life, because politics consists of continuously organized coalitions mobilized to engage in conflicts. These coalitions gain their power from broadcasting the dramatics of their conflicts in ways favorable to themselves, thereby creating particularized reputations for various individuals as powerful, villainous, or impotent. Politics, as the struggle over reputation, rests upon control of the means of reputation management.

c) *Emotional energies* form the most crucial mechanism in all of these processes. Shifts in both generalized and particularized cultural resources have effects upon people's actions in microsituations because they affect their emotional energies. The reputation shift of a political leader, for example, is truly effective only when the rumors carry an emotional impact, a contagion of feelings throughout the society about which is now the dominant coalition. Hence the market attractiveness of that coalition increases, all the more so to the extent that it spreads fear of its threat to those people who remain outside it. Conflict, war, and politics can be regarded as quintessentially emotion-producing situations. The stronger the conflict, the more emotional energy flows through the networks of micro-interaction constituting the macrostructure. Periods of rapidly changing reputational resources become particularly important for the organization of social networks to the extent that such networks are vehicles for strong emotional contagion.

There are also conditions that change the entire level of emotional energy in a society. Parallel to the introduction of new communications technology and generalized-culture-producing specialists, one can think of the historical introduction of new emotion-producing "technologies," including shifts in the number of emotion-producing specialists. From this viewpoint, changes in material conditions are most important because they change the number of people who can assemble for ritual purposes or because they change people's capacities for impression management or dramatization (Collins 1975, pp. 161–216, 364–80). Such technologies of dramatization have ranged from the massive architecture and lavish religious and politi-

cal ceremony of the pharaohs through the various styles of political display of today. Similarly, the history of religions can be seen as a series of inventions of new social devices for generating emotions, ranging from the shaman's magic ritual, to congregational worship, to individual meditation and prayer. In this perspective, shifts among tribal, patrimonial-feudal, and bureaucratic forms of organization are shifts among diverse sources of emotional impression management. The various combinations of these emotional technologies available at any given time, and their degree of concentration or dispersion among the populace, are crucial factors in the struggle for power in any particular historical society.

An overall picture of the statics and dynamics of macrostructures emerges, at least in general outline. There are relatively slow processes of macrochange, fueled by new emotional "technologies" or by stepped-up production of either generalized cultural currency or emotional energies. There are also episodic shifts in particularized cultural resources—especially the reputations of persons who ritually enact the most powerful political, military, and religious coalitions—which occur at times of dramatic conflict. The slow processes, which may spread either to certain privileged groups or more uniformly throughout the society, bring about long periods of organization building and personal mobilization which alter both the structure of the society and its degree of fluidity and conflict. The rapid, episodic processes bring about revolutionary shifts in which dramatic conflicts focusing attention on a new dominant coalition can bring about massive changes in the patterns of property and organization and in the particular distribution of persons in them.

CONCLUSION

The preceding model has been presented in very abstract form. It does not attempt to describe the detailed variants of ritual interaction or the complexities of conversational negotiations and emotional energies. Integrating these variants into the general model should greatly increase its explanatory power. On the macro level, as well, there are many ramifications to be worked out in translating all macropatterns into micro-interactional "markets" of generalized and particularized cultural resources and emotional energies.

Even at this degree of imprecision, I hope that the model conveys some of the advantages of integrating micro and macro descriptions into a common explanatory framework. It suggests, for example, that "entities" that have been located in individuals, such as "personality" or "attitudes," are instead *situational* ways of acting in conversational encounters, and that "personalities" and "attitudes" are stable only to the extent that individuals undergo the same kinds of repeated interactions. Charismatic person-

alities, by this account, are simply individuals who have become the focal point of an emotion-producing ritual that links together a large coalition; their charisma waxes and wanes according to the degree to which the aggregate conditions for the dramatic predomination of that coalition are met. On a smaller scale, one may hypothesize that upwardly mobile individuals are those whose cultural resources lead them through a sequence of IR experiences that build up their emotional energies, hence their confidence and drive; when they reach IR matchups that no longer give a favorable emotional balance, this advantage disappears, and they cease to rise further. To mention one more area of application, the growth of a productive economy as well as its cycles of boom and depression should be to an important degree determined by shifts in emotional energies throughout the working population in general, or possibly among entrepreneurs in particular.

Such explanations of specific phenomena need to be elaborated from both the micro and the macro sides. I would also suggest that the connection between the two levels can be made empirical by a new form of research. Generalized and particularized conversational resources exist simply as things people say in conversations; emotional energies exist in the rhythms and tones with which people say them. Accordingly, one may take a macro-sample of the distribution of microresources by sampling conversations across a large number of different social groups and taking repeated samples over time. Such a method moves away from the predominant emphasis of contemporary conversational research, which performs detailed analyses of single conversations in isolation. The proposed method resembles sample surveys, but instead of tapping attitudes of self-reports by interviewer questions, it would sample natural conversations by audio or video recordings. Technical devices may make it possible to characterize the emotional energies of conversational tone and rhythm from tape recordings or by expressive postures in video recordings. Generalized and particularized conversational resources may be characterized by the same data, by classifying verbal contents. With these kinds of data, it should be possible to show the actual operation of IR chains, their effects upon individuals' situational behavior, and their aggregate effects upon social stability and social change.

SUMMARY

The following principles have been suggested to construct an explanatory theory of macrostructures as aggregates of microsituations:

1. Sociological concepts can be made fully empirical only by translating them into a sample of the typical micro-events that make them up.
2. The dynamics as well as the inertia in any causal explanation of social structure must be microsituational; all macroconditions have their effects by impinging upon actors' situational motivations.

3. Human cognitive capacity is limited, hence actors facing complex contingencies of social coordination rely largely upon tacit assumptions and routine.

4. Any individual's routine is organized around particular physical places and objects, including the physical bodies of other persons. The sum of these physical routines, at any moment, makes up the microreality of property.

5. Authority is a type of routine in which particular individuals dominate micro-interactions with other individuals.

6. What particular routines are to be adhered to is subject to self-interested maneuver and conflict. Both adherence to routines and changes in them are determined by individuals' tacit monitoring of the power of social coalitions.

7. Conversations are rituals creating beliefs in common realities that become symbols of group solidarity. Individual chains of conversational experiences over time (IR chains) thus re-create both social coalitions and people's cognitive beliefs about social structure.

8. Conversational topics imply group membership. Generalized conversational resources (impersonal topics) reproduce horizontal status-group ties. Particularized conversation enacts individuals' property and organizational positions and further reinforces this concrete social structure by circulating beliefs about it, including the reputations of particular individuals.

9. An encounter is a "marketplace" in which individuals tacitly match conversational and emotional resources acquired from previous encounters. Individuals are motivated to enact or reject conversational rituals with particular persons to the extent that they experience favorable or unfavorable emotional energies from that interaction, as compared with other IRs they remember in their recent experiences.

10. Individuals' acceptance or rejection in an IR respectively raises or lowers their emotional energies (social confidence). Similar effects are produced by experiencing domination or subordination within an IR. These emotional results are weighted by the intensity of emotional arousal in each IR and by the power of the membership coalition it invokes (its control over property and force).

11. Several different ritual markets operate simultaneously: a slow-moving market of persons shifting in and out of particular property and organizational positions, more rapidly changing markets for informal solidarity within organizations and among individuals outside organizational relations, and very long-term markets for the growth and decline of organizations as a whole.

12. In each market, individuals sense their personal opportunities via their degree of emotional energy. They move toward more advantageous ritual exchanges until they reach personal equilibrium points at which their

cultural and emotional resources are matched by equal or greater resources of their partners.

13. Social structure is constantly changing on the microlevel, but it tends to an aggregate stability if individual fluctuations of emotional and cultural resources are local and temporary.

14. Large-scale changes of social structure occur through changes in any of the three types of microresources: (a) Increases in generalized cultural resources, produced by new communications media or increased activity of religious and educational specialists, increase the size of group coalitions that can be formed and hence the scope of organizational structure. (b) Particularized cultural resources change, for a whole society, when dramatic (usually conflictual) events focus many people's attention on particular individuals, thereby creating rapid shifts in their reputations and shifting the organizational center of power coalitions. (c) New "ritual technologies," including shifts in the materials of impression management and in the typical density and focus of encounters, change the quality of emotions throughout a society. Such shifts bring about changes in the nature of social movements and in the dynamics of political and economic action.

15. Conversational resources and emotional energies may be directly measured by sampling conversation through time and across populations; the cultural resources are found in conversational topics and the energy levels in the tone and rhythm of talk.

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The Focused Organization of Social Ties¹

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Sociologists since Simmel have been interested in social circles as essential features of friendship networks. Although network analysis has been increasingly used to uncover patterns among social relationships, theoretical explanations of these patterns have been inadequate. This paper presents a theory of the social organization of friendship ties. The approach is based upon Homans's concepts of activities, interactions, and sentiments and upon the concept of extra-network foci organizing social activities and interaction. The theory is contrasted with Heider's balance theory. Implications for transitivity, network bridges, and density of personal networks are discussed and presented as propositions. The focus theory is shown to help explain patterns of friendships in the 1965-66 Detroit Area Study. This paper is intended as a step toward the development of integrated theory to explain interrelationships between networks and other aspects of social structure. Implications for data analysis are discussed.

Sociologists have long recognized the importance of patterns in networks of relations that connect individuals with each other. Simmel (1955) described modern society as consisting of loosely connected social circles of relationships. Granovetter (1973) has indicated the general significance of these social circles for communication, community organization, and social conflict. Various studies have supported this picture of the essential patterns in social networks, including Moreno's sociometry (1953), Milgram's "small world" experiments (1967), and Kadushin's observations (1966).

Unfortunately, the study of social networks has often been carried out without concern for the origins in the larger social context. Most network analysis ends with description and labeling of patterns; and when explanations of patterns are offered, they frequently rely upon inherent tendencies within networks to become consistent, balanced, or transitive. As a consequence of such atheoretical and/or self-contained network theoretical approaches, data are collected and data analysis techniques are devised for

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social ties alone, without taking into account individual characteristics or other social structures.

Recent studies have taken the important step of beginning to investigate the extra-network sociological bases for friendship ties (e.g., Fischer et al. 1977; Verbrugge 1977). However, in order to explain patterns in social networks, we need not look at all causes of friendship but should concentrate our attention on those aspects of the extra-network social structure that systematically produce patterns in a social network. My purpose in this paper is to present the basic structure of a theory that can begin to explain the origins of the ubiquitous loosely connected social circles.

The theory is based upon the idea that the relevant aspects of the social environment can be seen as foci around which individuals organize their social relations. A focus is defined as a social, psychological, legal, or physical entity around which joint activities are organized (e.g., workplaces, voluntary organizations, hangouts, families, etc.). As a consequence of interaction associated with their joint activities, individuals whose activities are organized around the same focus will tend to become interpersonally tied and form a cluster. The task of the network analyst is the investigation of those social structural characteristics that serve to organize the activities underlying the social ties of a network. Such analysis requires information about each individual's relations to extra-network foci. Without such contextual information, conclusions about networks and their consequences are likely to be incomplete and even misleading.

In this paper, I will explicate the theory and demonstrate how it may be applied. The paper is organized as follows: (1) the present theoretical approach will be described and contrasted with balance theory; (2) the nature of foci will be considered; (3) the process by which loosely connected clusters of ties arise from focused social interaction will be discussed; (4) focus theory implications for transitivity, local bridges, and density of personal networks will be described; (5) formal definitions, assumptions, and propositions will be specified; (6) focus theory predictions will be examined using the analysis of the 1966-67 Detroit Area Study data presented by Fischer et al. (1977); and (7) a focus theory approach to network data analysis will be presented.

THE SOCIOLOGICAL PROCESS OF FOCUSING SOCIAL NETWORKS

In the focus theory approach, a social context can be seen as consisting of a number of different foci and individuals, where each individual is related to some foci and not to others. A group's activities are organized by a particular focus to the extent that two individuals who share that focus are more likely to share joint activities with each other than two individuals who do not have that focus in common.

The Focused Organization of Social Ties

The present theory is built upon Homans's (1950) social elements of activity, interaction, and sentiments. Activities are organized around foci and, consequently, so are interactions and sentiments. It is not new to suggest that ties are organized around extra-network characteristics (e.g., Laumann 1973; Kadushin 1966). However, there has been no general consideration of the nature of foci and the processes through which relations to these foci become patterns in networks.

Davis (1963) recognized that interpersonal attraction theorists, exchange theorists, and others shared a basic abstract notion of the social organization of ties that implies clustering at the level of group structure. He suggested that balance theory could be used as a single approach to integrate propositions derived from these diverse sources. In some ways, the present theory is similar to Heider's (1946) original formulation of balance theory, which emphasized that sentiments among individuals tend to become consistent with the relations that the individuals have to other objects. However, balance theory and the present theory are based upon different conceptions of the underlying process. Balance theory is essentially psychological: the process takes place within the heads of the actors. The present theory is essentially sociological: the process depends upon the behaviors and interactions of individuals in a social context.

Two specific comparisons should be emphasized. (1) Although Heider himself intended balance theory to include a broad range of types of relationships to "objects" (including similarity, proximity, and membership), Newcomb (1961), Davis (1963), and most subsequent balance theory researchers have emphasized only the affective relations, that is, relations in which two individuals are jointly favorable or unfavorable toward an object, where the object is specifically an attitude or another person. My concept of focus is similar to Heider's original formulation of relations to a social object which could include "working in a place," "belonging to a group," etc. The nature of these relations to objects vary; yet they are abstractly similar in that they may be considered relations to generally defined foci; and they therefore have similar implications for group structure. (2) Heider and subsequent balance theorists have suggested that changes in sentiments are the direct result of cognitive pressures toward making sentiments consistent with relations to objects. Relying upon Homans's (1961) behaviorism in the context of the social structures of foci, I am suggesting that shared relations to foci create positive sentiments indirectly through the generation of positively valued interaction (i.e., shared relations to foci bring people together in a mutually rewarding situation which encourages the development of positive sentiments). The focus theory's structural/behavioral process does not require that the participants have any understanding whatsoever of the underlying focusing structures and processes.

The psychological approach of balance theory has sometimes led researchers to erroneous conclusions. They have tried to explain the extent of clustering in a network as a consequence of the strength of pressures toward cognitive consistency (e.g., Leinhardt 1972; Hallinan 1974). I suggest that clustering will most often be a result of the tendency of the foci to organize exclusive social interaction. Accordingly, in order to understand the patterns that are found in a social network, it is necessary to investigate: (1) the sociological nature of the foci, (2) the distribution of the individual relations to the foci, and (3) the degree to which the foci organize valued social interaction among the individuals. For example, in an elementary school class where reading groups organize activities, one would need to determine the number of members of each reading group and the extent to which these groups organize activities.

THE NATURE OF FOCI

It is important to emphasize that foci tend to produce patterns of ties, but all ties do not arise from foci. A relationship between two individuals does not necessarily arise from activities that are organized around a focus. People may meet "by chance" or as a result of adjacency along some continuum; neither of these situations includes a focus. The central point of the focus theory is that no matter what proportion of ties arise from foci, the focused organization has structural significance.

Foci may be many different things, including persons, places, social positions, activities, and groups. They may actively bring people together or passively constrain them to interact. In Homans's (1950) original formulation, similarity brings individuals together in interaction and sentiments; but similarity is not sufficient to account for the clustered arrangement of ties that we are trying to explain. This can be seen by considering that if individuals are distributed along a continuum, then similarity leads to a chain of interaction in which each individual interacts with those adjacent on either side. If there are foci, on the other hand, then all those related to a particular focus tend to form a separate cluster. So the present theory is dependent upon the existence of such discrete entities to explain the clusters that are found. (As discussed later, the loose connections between clusters may be based upon less constraining foci, or not based upon foci at all.)

In this respect, the present theory bears some resemblance to the discussions of Lazarsfeld and Merton (1964) in their consideration of friendship formation, and Blau (1978) in his theory of social structure; these discussions stress the importance of the similarity of discrete categories of attitudes, attributes, and social positions in the formation of social ties. Certainly, where frequency of interaction is uniform, such similarities may

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lead to the selective development of ties. However, the point of the current paper is that interaction is typically focused rather than uniform, and unless the similarities of attitudes, attributes, and social positions are translated into the structuring of focused interaction, their selective effects on tie formation will be overwhelmed by structural features that do focus the interaction. Similarities need not lead to focused interaction, and focused interaction can exist apart from similarities of individual characteristics. The present theory stresses the focused organization of the social context rather than similarities of individual characteristics.

Variations among Foci

By the definition of a focus, it is always the case that two individuals who share a focus are more likely to share joint activities than two random people. However, all individuals who share a focus do not necessarily interact with each other very much or very often. For foci where everyone is forced to interact much and often (e.g., families), all of the individuals associated with that focus will be tied to each other; but for foci that are less constraining on interaction (e.g., city neighborhoods), only a slightly higher proportion of individuals will be tied than would be tied in the general population. In general, the more constraining a focus, the greater is the likelihood that two individuals associated with that focus will be tied. A focus may involve very little constraint, but where there is no constraint at all, there is no focus.

Although all foci organize the activities of a limited number of people, they vary in size. Small foci organize the activities of very few people, while large foci organize the activities of many people. In general, larger foci will be less constraining, because it is difficult to arrange for many people to have frequent joint activities. However, there may be small foci that involve little constraint and large ones that involve much.

The structure of a network is dependent upon the constraint and size of the underlying foci. Highly constraining foci will create close-knit clusters of various sizes depending upon the size of the foci.

Developing New Foci

In order to understand network structure fully, it is important to remember that the formation of social networks and the relations to foci are interdependent. Once there is a tie between two individuals, these individuals will tend to find and develop new foci around which to organize their joint activity. The structural approach underlying the focus theory suggests that the more severe the restrictions on time, effort, and emotion, the more individuals will experience pressures to combine their interactions with vari-

ous members of their network by finding and developing new foci around which to bring more of them together. This will be facilitated if the foci upon which the original ties are based are more "compatible," that is, involve similar types of activities and social interactions (e.g., neighborhood and family are typically more compatible than workplace and childhood neighborhood). The more compatible the foci, the more likely it is that the individual can find or invent some focus that can organize joint activities.

Where there are reasonably compatible foci underlying ties among many individuals, such loose-knit sets of people will tend to develop new foci that organize activities among themselves. Thus, *ceteris paribus*, the more ties within a set of individuals, the more likely it is that a common focus will be developed, and, consequently, previously untied pairs within the set will become tied.

Balance theorists offer a cognitive explanation of the tendency for indirect ties to lead to the development of direct ones. They suggest that psychological tendencies toward consistency lead individuals to bring members of their network together. Balance theory thus implies that the factors determining whether direct ties will develop are psychological characteristics of the individuals. The focus theory suggests that the factors determining whether direct ties will develop are characteristics of the social situation and the compatibility of the foci underlying the indirect connections.

THE FOCUSED ORGANIZATION AND LOOSELY CONNECTED CLUSTERS

There can be a range of complexity in the focused organization of social ties, and I will begin with consideration of the simplest. A "simply focused" situation is an ideal type in which there are multiple foci, but each individual is related to a single focus. In this type of situation, interactions and sentiments tend to be within clusters organized around each focus. Any interactions and sentiments between individuals associated with different foci are not based upon foci and provide "random" links between the clusters.

Sociometric studies of relatively small groups frequently show clustered arrangements of ties that suggest the simple focus model. Some of these situations can provide clear examples of types of foci. Consider the finding of Festinger, Schachter, and Back (1950) that friendships were largely confined to spatially segregated courts in a housing project, leading to the clustered arrangement of the simple focus model. In this case, the architects designed the courts to be the foci of activity and interaction. However, even if places of residence are continuously arranged, neighborhoods are often foci separated by large streets, railroad tracks, etc.; and barriers may be solidified through legal divisions separating school districts, water districts, and other jurisdictions (Logan 1978).

The Focused Organization of Social Ties

Most situations are more complex than simply focused situations, because each individual's activities and interactions are organized around a number of different foci simultaneously. The interpenetration of clusters can have important consequences for conflict and cooperation (Coleman 1957), and the patterns of interpenetration may be the direct result of the complex, focused organization of ties. The particular purpose of this paper is to explain the loose connections typically found between clusters. Any focus is likely to include individuals who are associated with other foci, and thus ties associated with one focus may serve as links in a chain between individuals associated with other foci. The number of alternative paths between these other foci will depend upon the nature of the linking focus. The larger and more constraining the linking focus, the more alternative paths there will be. Therefore, "loose" connections between clusters (i.e., few alternative paths) are expected wherever a connecting focus is relatively small and/or weakly constraining. In the extreme case, where a tie is not based upon any focus, it is most likely to be the unique path between separate clusters.

The situation may be represented using a variation of Ptolemy's circle-upon-circle model of planetary motion: here each circle contains all of the individuals associated with a particular focus. Each individual alternates between participation in multiple orbits, and so each individual is the intersection of orbits. As shown in figure 1, a social path from one individual to any other may be traced by following one of the orbits of the first person (A) to another person (B), and then following another orbit to another individual (C), etc., until the target individual (E) is reached. (Note that fig. 1 represents a simple situation. For many situations more dimensions and oddly shaped orbits may be required to show both that three or more orbits may intersect at one individual and that two orbits may intersect at three or more different individuals.) Milgram (1967) suggests that for

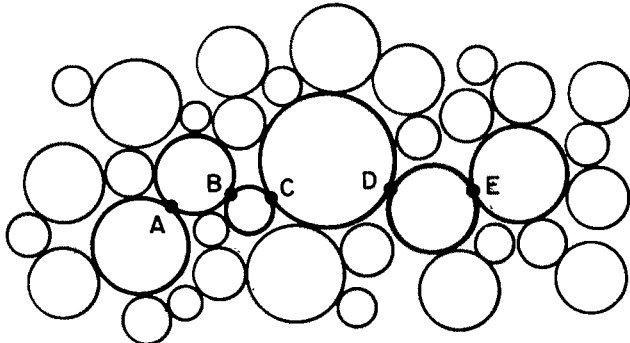


FIG. 1.—A schematic representation of social circles with individuals as the intersections.

any initiator and target in the United States, an average of only about five such orbits would need to be followed.

The following discussion considers the implications of the focused organization of social ties from three perspectives: (1) The extent of clustering is equivalent to the extent of "transitivity" of mutual relationships (Davis 1967; Holland and Leinhardt 1971). (2) Local bridges are the extreme form of critically important connections between clusters (Granovetter 1973). (3) The density of personal networks indicates the extent to which individuals are contained within clusters or are intersections between clusters. The network position of individuals can have important implications for each person (e.g., Bott 1957).

FOCUS THEORY IMPLICATIONS

Transitivity

Researchers have documented the tendency for two individuals who are both tied to a third to also be tied to each other. This is called a tendency toward transitivity. Transitivity has sometimes been more broadly defined as a constraint upon asymmetric as well as symmetric ties (Holland and Leinhardt 1971), but the present discussion concerns mutual relationships, and Davis (1967) has shown that the condition of transitivity of symmetric ties is identical to the structure of clusters (i.e., where all clusters are completely connected, and no clusters are connected with each other).

The focus theory suggests the conditions under which transitivity should be expected, and thereby the conditions under which clusters are formed. The theory suggests that two individuals who are both tied to a third may share a focus with the third; and if they share the same focus with the third person, then they share that with each other and are likely to be tied to each other. The more foci that they share with the person, and consequently with each other, the more likely it is that they will be tied with each other. The more constraining are the foci that they share with the person, and consequently with each other, the more likely it is that they will be tied with each other. Thus, the focus theory implies that the main causes of transitivity are the number and types of preexisting foci underlying the relationships.

In general, each individual who is related to two or more foci can expect that many of his or her ties will be to others who are not tied to one another. As discussed in an earlier section of the paper, when an individual is confronted with the typical situation of ties to disconnected others, he or she may seek to change this situation by creating and/or finding a new focus around which to organize his or her joint activities with the others. In this way, individuals create transitivity over and above the transitivity

induced by focused sources of ties. Individuals are most likely to engage in such creative network manipulation in situations where relationships involve a high proportion of their time, effort, and emotion, and where the relationships are based upon compatible foci.

Previously, transitivity has been studied as a consequence of balance theory, whereby researchers have suggested that tendencies toward transitivity were the result of individuals seeking to make their ties cognitively consistent. The main focus theory prediction is that transitivity will be present specifically where ties are based upon highly constraining shared foci and/or where structural pressures lead actors to create additional transitivity.

Local Bridges

Granovetter (1973) has explained how ties which connect two individuals who do not share ties to other individuals are important for communication and community organization; such ties are called local bridges. Where there is perfect transitivity, there can be no local bridges, because transitivity requires that every two individuals who are tied to each other must also be tied to all of the same others. Local bridges will be likely where transitivity is unlikely.

By definition, the more others to whom two people share ties, the less bridging is a tie between them. Ties based upon foci are less bridging than other ties, because the two people are likely to share ties to others associated with that focus. If a tie is based upon a focus, then the larger the focus, the more ties to others will be shared. If a focus of a given size underlies the tie, then the more constraining the focus, the more others will be tied to both of the individuals. The more foci the two individuals share, the more other people are likely to be tied to both of them. In summary, the fewer, less constraining, and smaller the foci underlying a tie, the more bridging is the tie; and ties not based upon foci are most bridging. Over time, a tie may lose its structural significance as a bridge if one or both of the individuals find or develop a focus that organizes their joint activities with others, and so leads the two individuals to share ties to others. A tie is most likely to remain bridging if it involves little time, effort, and emotion (so the individuals will feel little pressure toward combining their activities with others), and if the underlying focus is incompatible with other foci (e.g., a married man will be unlikely to introduce his friend from a singles bar to his family or to his work associates).

Granovetter describes the counterintuitive finding that highly significant bridging ties tend to be "weak" ties. He offers a number of definitions of strength/weakness based alternatively upon emotional content, interaction, and functions of a tie. To the extent that Granovetter offers any expla-

nation of his finding, he suggests that it is the nature of a strong tie, involving much emotional interaction, that tends to bring both partners in the tie into joint contact with others. The focus theory also implies that ties involving high degrees of interaction and emotion are likely to be those that encourage the finding and development of new foci. However, the focus theory emphasizes that many ties are based upon preexisting foci. The more constraining is an underlying focus, the stronger a tie is likely to be by any of the available definitions. As discussed above, the more constraining is an underlying focus, the less bridging is a tie. It follows that those weak ties that are based upon less constraining foci or upon no foci will be found to be bridging. Thus, the focus theory provides theoretical explanations of the "bridgingness" of weak ties in terms of their focused origins. The theory allows prediction of which weak ties will be most bridging, based upon the underlying foci.

Density of Personal Networks

The density of personal networks is the extent to which the associates of a particular individual are tied to one another. If there is perfect transitivity, then all of an individual's associates know one another, and the personal network is completely dense. On the other hand, if every tie to an associate is a local bridge, then none of the individual's associates know one another, and the density is zero. If an individual has a completely dense network, then the individual is contained in a cluster of individuals; but if the network has low density, then the individual provides a link between others who are otherwise disconnected and so is an intersection of otherwise disconnected social circles. Bott's (1957) research finding that the density of personal networks affects conjugal roles has stimulated interest in this area (e.g., Cubitt 1973; Kapferer 1973). Nevertheless, the causes of the density of personal networks are not well understood. The focus theory provides some clear guides to particular types of factors that make it likely that personal networks will be dense. If individuals associate with many different foci, then it is unlikely that their networks will be dense, because the individuals drawn from different foci will be unlikely to know one another. Boissevain (1968) has described the focused organization of personal networks in terms of each individual having his or her networks segmented according to "activity fields." Cubitt (1973) has empirically investigated some bases of these sectors. The focus theory explicates this basic idea.

If associates are drawn from the same focus, then the more constraining the focus, the greater will be the density of the individual's personal network. If an individual shares many foci with each associate, then it is likely that those associates will share at least some foci with each other and conse-

quently be tied to each other. The number of foci that two people share is sometimes referred to as the "multiplexity" of a relationship. However, some researchers use the word "multiplexity" to indicate the multifaceted nature of the exchange relationships between two people (see Verbrugge 1979). A pair of individuals who share many foci are also likely to have multifaceted exchange relationships, but an analytical distinction should be maintained. The focus theory suggests that the primary aspect of multiplexity affecting density is the sharing of foci of activity and interaction, rather than merely having multifaceted exchange relationships.

However, multifaceted exchange relationships may involve a large amount of time, effort, and emotion. The focus theory implies that where relationships involve a high proportion of an individual's time, effort, and emotion, that individual will try to develop foci that bring his or her associates together in a dense personal network. The individual will be most successful in developing such foci where the original foci are compatible with one another.

FORMAL DEFINITIONS, ASSUMPTIONS, AND PROPOSITIONS OF THE FOCUS MODEL

The focus model not only accounts for the frequently observed patterns of clustering in networks, but also specifies a process through which the patterns arise and are maintained.

In order to clarify the assumptions and implications of the theory, it may be helpful to express them more formally. It is impossible to capture the full implications of a theory in a short list of propositions; so the following list is intended only to indicate the nature of a formal presentation of the theory with a few examples of applications.

First, I will provide definitions and assumptions, and then a list of derivations from the theory. Some of these will overlap, and others will contradict derivations from other theories.

Definition 1.—A "focus" is any social, psychological, or physical entity around which joint activities of individuals are organized.

Definition 2.—A focus is "constraining" to the extent that it leads each pair of individuals to devote time and energy to participating in joint activities associated with that focus.

Definition 3.—Two foci are "compatible" with each other to the extent that the types of activities and interactions that they involve are similar.

Definition 4.—A focus is "smaller" (larger), the smaller (larger) the number of people who share it.

Assumption 1.—There exist foci in the social world.

Assumption 2.—(Borrowed from Homans) The more frequently indi-

viduals have valued social interaction with each other, the more likely it is that they will develop positive sentiments toward each other.

Assumption 3.—Individuals can find or invent a focus around which to combine activities of various others with whom they are tied. A schematic representation of the dynamic process described by the theory is presented in figure 2.

I will list five basic focus theory propositions, predictions of the likelihood of ties between dyads based upon the number and types of foci that they share. Then the same five propositions will be adapted to make predictions concerning transitivity, bridging, and density of personal networks. These propositions have been informally discussed and explained in the preceding sections and are formally stated here to avoid theoretical ambiguity and to allow for empirical testing.

Basic Propositions

Proposition 1.—Two individuals who are related to the same focus are more likely to be tied than two people not so related.

Proposition 2.—If two individuals are related to the same focus, the more constraining the focus, the more likely it is that they will be tied.

Proposition 3.—The more different foci that two individuals share, the more likely it is that they will be tied.

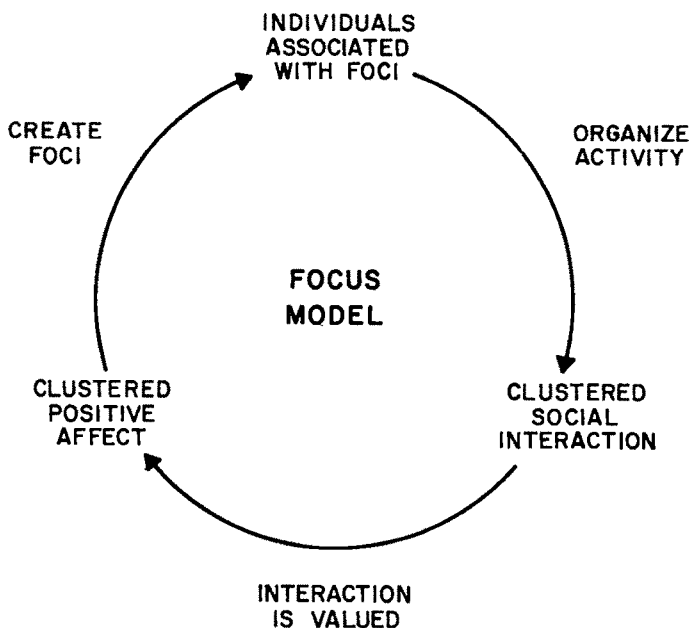


FIG. 2.—The dynamics of the focus model

The Focused Organization of Social Ties

Proposition 4.—Where two individuals are each tied to a third, based upon a different focus, the more compatible are these foci, the more likely it is that the two individuals will be tied to each other.

Proposition 5.—Where two individuals are each tied to a third, based upon a different focus, the more time, energy, and emotion that these ties involve, the more likely it is that the two individuals will be tied to each other.

Transitivity Propositions

Definition 5.—If *A* and *B* are tied, and *B* and *C* are tied, then the triad consisting of *A*, *B*, and *C* is transitive if *A* and *C* are tied.

Proposition 1T.—If *A* and *B* are tied, and *B* and *C* are tied, then transitivity is more likely if *AB* and *BC* are based upon the same focus.

Proposition 2T.—If *A* and *B* are tied, and *B* and *C* are tied, based upon the same focus, then transitivity is more likely the more constraining the focus.

Proposition 3T.—If *A* and *B* are tied, and *B* and *C* are tied, then transitivity is more likely the more foci that *B* shares with both *A* and *C*.

Proposition 4T.—If *A* and *B* are tied, and *B* and *C* are tied, based upon different foci, then transitivity is more likely the more compatible the two different foci.

Proposition 5T.—If *A* and *B* are tied, and *B* and *C* are tied, based upon different foci, transitivity is more likely the greater the proportion of time, energy, and emotion that these ties involve for *B*.

Bridging Propositions

Definition 6.—The smaller the number of others with whom *A* and *B* share ties, the more bridging is a tie between *A* and *B*.

Proposition 1B.—Ties based upon foci are less bridging than other ties.

Proposition 2B.—Ties based upon foci are more bridging the less constraining and the smaller the foci underlying the ties.

Proposition 3B.—Ties based upon foci are more bridging the fewer foci underlying the ties.

Proposition 4B.—Ties based upon foci are more bridging the less the underlying foci are compatible with any other foci.

Proposition 5B.—Ties are more bridging the smaller the proportion of the individuals' time, effort, and emotion that they involve.

Personal Network Density Propositions

Definition 7.—The density of an individual's personal network is the proportion of pairs of the individual's associates who are tied to one another.

Proposition 1D.—The fewer foci from which an individual draws associates, the denser will be the personal network.

Proposition 2D.—The more constraining are the foci from which associates are drawn, the denser will be the personal network.

Proposition 3D.—The more foci that the individual shares with each associate, the denser will be the personal network.

Proposition 4D.—The more compatible with one another are the various foci from which an individual draws associates, the denser will be the personal network.

Proposition 5D.—The greater the proportion of the individual's time, effort, and emotion that the ties involve, the denser will be the personal network.

EMPIRICAL SUPPORT FOR THE FOCUS THEORY

The purpose of this section is not to provide critical tests of specific propositions of the focus theory, but to use a set of data to illustrate that the focus theory can provide accurate predictions concerning the density of personal networks.

Because researchers are usually interested in either the structure of networks or the origins of ties, but not both, data that are appropriate for testing predictions from the focus theory are rare. Fortunately, data from the 1965–66 Detroit Area Study contain information on the origin of ties and the structure of networks. However, these data have other shortcomings. First of all, they were collected on separate networks of individuals by taking all of the information about origins and structure from one individual without any corroboration from the others. Second, each individual reported “three best friends,” while most networks contain many more friends as well as many other types of ties to others. Third, each individual was asked to report which of the best friends knew each other well, which is only an indicator of the existence of a tie between the others. Nevertheless, these data provide some preliminary results in support of the focus theory. Laumann (1973) analyzed these data originally, and Fischer et al. (1977) did extensive reanalysis. I will rely upon Fischer et al.'s published analysis.

Specifically, I will examine the structure of personal networks and show that the focus theory provides accurate predictions about this structure. The data, consisting of information on the three best friends of each respondent, include the predominate source of the ties (i.e., family, neighborhood, work, voluntary organizations, etc.) and the number of different sources from which an individual draws the friends (from one to three). The set of ties is also characterized by the average intimacy and the average frequency of contact with the individual. Finally, the data also include

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the number of interrelations among the three friends—the “density” of the individual’s network. As Laumann reports, there are four possible patterns of interrelations, which are shown and labeled in figure 3. The propositions concerning density of personal networks will be examined in order.

Proposition 1D.—The fewer foci from which an individual draws associates, the denser will be the personal network. In these data, Fischer determined the number of different sources from which each individual’s three best friends were drawn and found that this was related to density as expected ($\gamma = -.48$). In addition, he reported some of the information from the cross-tabulation, as shown in table 1. Single-source networks were much more likely to be fully dense (57%) than three-source networks (17%). Fischer also reported that whether the three friends “get together as a group” was highly related to the density of the network, as would be intuitively expected and specifically predicted by the focus theory. Since “sources” are causally prior to density, the relationship between number of sources and density provides strong evidence that the focused organization of the personal network is a determinant of the density of the network. The relationship between getting together as a group and density could arise from two complementary causal processes: sharing of foci leading to

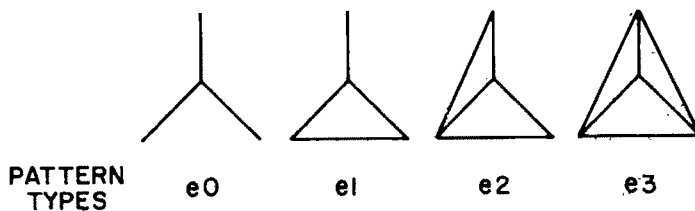


FIG. 3.—Patterns in personal networks

TABLE 1
DENSITY OF PERSONAL NETWORKS
RELATED TO NUMBER OF
DIFFERENT SOURCES

NETWORK TYPE	N DIFFERENT SOURCES (%)	
	1	3
$e_0, e_1, \text{ or } e_2$	43	83
e_3^*	57	17

NOTE.—Gamma = $-.48$ for the uncollapsed table. Percentages total 100; this is as complete as it is possible to reconstruct from the statistics reported in Claude S. Fischer, R. M. Jackson, C. A. Stueve, K. Gerson, and L. M. Jones, *Networks and Places: Social Relations in the Urban Setting* (New York: Free Press, 1977).

* Fully dense.

density, and density leading to sharing of foci. These findings provide strong support for the central proposition of the focus theory and could not have been predicted on the basis of balance theory or the strength of weak ties.

Proposition 2D.—The more constraining are the foci from which associates are drawn, the denser will be the personal network. Unfortunately, there are no independent measures of the constraint involved with each type of focus. However, it is reasonable to suggest that family and work involve frequent joint activities for most people, and so these two would generally be highly constraining foci. Wherever two of the three best friends were drawn from the same focus, Fischer characterized the personal network by its "primary source." Table 2 shows that personal networks with family and work as primary sources were more often fully dense than personal networks with other primary sources. It is interesting to note that this finding could not be anticipated merely on the basis of frequency of contact with the individual; in these data, neighbors and childhood friends had the greatest frequency of contact with the individual, and yet networks with these primary sources did not have as high density as family and work networks.

Proposition 3D.—The more foci that the individual shares with each associate, the denser will be the personal network. Although Fischer measured the multiplexity of ties according to the number of foci that the tie involved, there are no reported associations between density and multiplexity.

Proposition 4D.—The more compatible with one another are the various foci from which an individual draws associates, the denser will be the personal network. One could obtain some tentative support for this proposition by speculating about the compatibilities of foci (e.g., family and childhood would be more compatible than family and work), but Fischer did not analyze the data in such a way that densities of personal networks with various mixes of foci were presented. Consequently, not even an approximate

TABLE 2
DENSITY OF PERSONAL NETWORKS
WITH EACH DOMINANT SOURCE

Dominant Source	Type e_3 (%) [*]
Family.....	44 (23)
Work.....	39 (211)
Childhood.....	34 (167)
Association.....	34 (41)
Neighborhood.....	29 (212)

SOURCE.—Claude S. Fischer, R. M. Jackson, C. A. Stueve, K. Gerson, and L. M. Jones, *Networks and Places: Social Relations in the Urban Setting* (New York: Free Press, 1977).

NOTE.—Figures in parentheses = N .
^{*} Fully dense.

test is available. To test this proposition properly would require independent measures of compatibility of pairs of foci, which can be used to predict the densities of personal networks containing each pair of foci.

Proposition 5D.—The greater the proportion of the individual's time, effort, and emotion that the ties involve, the denser will be the personal network. Density was related to average intimacy ($\gamma = .13$) and to average frequency of contact ($\gamma = .24$) for the personal networks in these data; this provides direct support for the prediction that individuals tend to bring together friends with whom they relate intimately and frequently. As Fischer points out, it is also possible that when friends know one another, it is easier for the individual to relate closely and frequently with each of them. I have no doubt that the process operates in both causal directions. Certainly, if friends' knowing one another facilitates close and frequent contact, then an individual who is or wants to be close to two friends is more likely to introduce them to each other.

Thus, the data provide both strong evidence for the central focus theory proposition and suggestive evidence for two other propositions. Further analysis could be made to examine the two remaining propositions.

APPROACHES TO DATA ANALYSIS

The focused organization of social ties implies that a researcher should understand and measure relations to foci in order to understand the structure of a network. The patterns of relations to foci are likely to be crucial for explaining clusters and interrelations among clusters in a network, and ignoring these extra-network structural bases of ties may lead one to misinterpret (e.g., in terms of cognitive pressures toward consistency) patterns that are found among ties.

Taking foci into account requires collection of appropriate data and application of appropriate methods for analysis. Although a complete discussion of methodology would be inappropriate here, I will suggest the basic structure of data collection and analysis.

The focus theory directs the researcher to look for the particular foci that organize the activities and interactions of individuals in a situation. In order to find them, the researcher will ordinarily need to understand the major activities that organize the interactions of the individuals. If one were studying the children of a community, one would probably expect to find that schools were the major centers of activity and interaction. The boundaries for each school district circumscribe the residences of the children associated with that focus. If we wanted to understand better, we should look to specific classes as foci. In addition, there may be physical barriers (e.g., major roads dividing an area into focal places), and there may be religious centers which serve as foci.

Every situation is unique, and the particular foci must be determined. The researcher should ascertain which individuals are associated with which foci and investigate the constraint, size, and compatibility of the important foci. Constraint of a focus may be indicated by the amount of time that pairs of individuals typically spend in activities associated with that focus. Once the boundaries of a focus have been specified, it is relatively easy to measure its size (e.g., the membership of a church or the population of a neighborhood). To determine the compatibility of pairs of foci, one can list the activities that are associated with each focus and ask objective judges to estimate the similarity of the lists. On the basis of all this information, the researcher can project a hypothesized model of the network of the community. It will not be possible to predict exactly which individuals will be tied to which others, but it should be possible to predict the major patterns in the social network.

To determine whether foci can account for the patterns, the analysis requires a definition of the relevant "patterns." Qualitatively, one can draw a picture of the pattern of ties that would be expected to arise from a specified set of foci and see whether the network data can be organized in a sociogram with predicted clusters and connections among clusters.

For researchers who conceive of social structure in terms of clusters in a cluster analysis or "blocks" in a blockmodel analysis (White, Boorman, and Breiger 1976), a confirmatory type of factor analysis can be used to decide how well a structure based upon specified foci "fits" the data.

An alternative method of structural analysis is to define the extent of the social structure as the size of a relationship that exists between variables. The "focus variable" is then introduced to determine how much of the relationship is "explained." In particular, one may treat a pair of individuals as the unit of analysis. Pairs are characterized by two variables: (1) being tied to each other or not and (2) the number of others to whom both are tied. In any situation, some pairs will be tied, and some will share many ties to others. If the situation is unstructured, the pairs that are tied will share no more ties to others than do other pairs. However, wherever there is a clustered structure, there will be a tendency for tied pairs to share many more ties than other pairs. The association between the two variables can be measured, and the strength of the association taken to indicate the amount of focused structure (clustering). Pairs may be characterized by a third variable indicating whether a pair shares any of a set of specified foci (or how many foci are shared). Using elaboration or partial correlation methods, one can determine the extent to which this third variable "explains" the association between the first two variables. The extent to which the focus variable can explain the association can be said to be the extent to which the specified foci explain the structure.

This methodology can illustrate how it is possible for a set of foci to

account for important aspects of social structure while explaining only a relatively small proportion of the variance in social ties. Consider the possibility that the original correlation between being tied and number of others to whom both are tied is .35. Also assume that the correlation between sharing a focus and being tied is .5, and the correlation between sharing a focus and number of others to whom both are tied is .7. Based upon the correlation between sharing a focus and being tied, the focus variable explains only $(.5 \times .5)$.25 of the variance in ties. However, using approximate methods of partial correlation, it can be seen that the focus variable can completely explain the relationship between being tied and number of others to whom both are tied $(.5 \times .7 = .35)$. Consequently, the foci may be said to explain the clustered structure completely, while explaining only a small proportion of the variance in ties. Thus, although other factors may explain more of the variation in ties than foci do, foci may nevertheless explain the major structures of relationships in a group.

CONCLUSIONS

The purpose of this paper has been to explain the patterns found in social networks, particularly patterns of clusters and connections among clusters, by determining the underlying foci and individual relations to those foci that cause the relations among the individuals. I have argued that the focused organization of social ties is important under practically all circumstances, and that the nature of the foci will vary in important ways depending upon the values and activities of the group. The theory explains the presence of most transitivity in networks as a function of the organization around foci rather than as an inherent tendency or as a function of the cognitive needs of individuals.

As a structural rather than a cognitive theory, the focus theory may be applicable to the social organization of ties among entities that do not have the ability or inclination to "think" like individual persons. For example, social networks have been studied in science, corporations, and international relations.

In science, authors of articles have been considered tied if their articles are jointly cited in subsequent articles (Cole 1975). These ties may be focused by subject matter, school of thought, university affiliation, etc. The structures of disciplines may be essentially different as the result of different types of foci. These foci create personal and professional interactions among authors.

A tie between corporations has been defined to exist wherever two corporations share a director (Mintz and Schwartz 1979). Such interlocks may be focused by location (e.g., northeastern corporations are more often interlocked), common ownership (e.g., major holdings by families), or de-

pendence upon particular financial institutions. The nature of these foci may reflect the underlying structure of capitalist enterprise in a country.

Finally, ties of alliance among nations may be focused by location, membership in formal group alliances (NATO, the Warsaw Pact), common resources (OPEC), ethnic identity (black Africa and the Moslem world), form of economy or government (socialism, communism, free market capitalism, fascism, etc.), or relations to the superpowers (those under U.S. vs. USSR domination). The patterns change over time to reflect changing political, economic, and social realities.

In all three examples, once one understands the focused organization, one can predict that transitivity will occur around the foci, and bridges will be ties based upon weakly constraining foci or not based upon foci at all. For example, authors may be tied on the basis of a maverick article of an unaffiliated author; or an interlock may be based upon the common use of the eminence of a particular former cabinet member on boards; or ties between nations can be based upon nothing more than immediate convenience (e.g., the peculiar short-lived alliance between Israel and Uganda).

The focus theory will not be applicable under all circumstances. There may be situations where there are no foci, and there may be situations where other processes (e.g., based upon similarities or upon deliberate manipulations by the actors) override the effects of foci. However, where the focus theory does apply, it should provide a step toward understanding the relationship between the structure of social networks and other aspects of social structure.

In this paper, I have emphasized causes of patterns of networks. It is equally important to understand the consequences of such patterns. Through further development of integrated theory and of data analytic techniques that can simultaneously analyze network and other structural data, it should be possible to develop a better understanding of social structure as a whole.

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The Price and Value of Children: The Case of Children's Insurance¹

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Qualitative analysis of historical data on the development of children's insurance in the United States between 1875 and the early decades of the 20th century explores the complex relationship between the human and the market values of children, specifically the emergence of the economically "worthless" but emotionally "priceless" child. This paper focuses on the legislative struggle between child insurers and child savers who opposed the insuring of children. I show that although monetary compensation for the death of a child was initially justified by the pecuniary loss for the parents, the final success of the insurance industry was based on more than economic rationality. It made its appeal primarily as burial insurance for poor children. This is a measure of the emerging "sacralization" of children's lives.

On March 14, 1895, the *Boston Evening Transcript* stated: "No manly man and no womanly woman should be ready to say that their infants have pecuniary value." The paper was attacking the widespread contemporary practice of insuring children. For 3 cents a week, for instance, a one-year-old child could be insured for \$10, a 10-year-old for \$33. To this day, the concept of making money out of the life or death of a child seems mercenary and morally repugnant to most people. Yet a major national survey sponsored by the American Council of Life Insurance in 1976 reveals that 57% of all American children under 15 have some kind of life insurance coverage. Surprisingly, children's insurance has been largely ignored in the voluminous life insurance literature. Despite its commercial success, it remains mostly unadvertised and unpublicized, and it is difficult to obtain information on the subject.

The development of children's insurance offers the possibility of probing into the complex relationship between human values and the market. The single most profound and extensive analysis of this issue is found in Simmel's *Philosophy of Money* (1978), where he traces the historical

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dichotomization of money and personal values. Simmel attributes this polarization to a dual process which transformed both the value of a person and the value of money. While an earlier, relativist concept of human life legitimated its quantification, Christianity sacralized and absolutized human existence, setting life above financial considerations. Paradoxically, the growing inadequacy of money is also a consequence of its success. The "sacred dignity" of primitive money disappears in modern society as money expands into the "colourless and indifferent" equivalent of everything and anything (Simmel 1978, pp. 365-66). Money's successful leveling of all qualitative distinctions into quantitative measurement clashes with a powerful "ideal of distinction" which stresses the uniqueness of human values.

Thus, Simmel posits a radical contradiction and a necessary tension between money and human values in modern society, which limit or at least obstruct the expansion of the market into all areas of exchange. His arguments may be examined with empirical data. In my previous work, based on the development of adult life insurance, I analyzed the powerful cultural resistance in the earlier part of the 19th century against profaning the sanctity of human life and death with commercial considerations. Gradually, the capitalization of the value of adult life and the monetary indemnification of that value became acceptable. However, the monetary evaluation of death did not desacralize it. Far from "profaning" life and death, money became ritualized by its association with them. Life insurance took on symbolic values quite distinct from its utilitarian function, emerging as a new form of ritual with which to face death (Zelizer 1978, 1979).

Ironically, as men's lives became more and more entangled with market considerations, children's lives were gradually severed from market ties. The radical transformation in the value of children which began in the 20th-century United States—specifically, the emergence of the economically "worthless" but emotionally "priceless" child—offers another empirical possibility of examining the interaction between the market and personal values. The case of adult life insurance suggests that commercialization of those aspects of the social order, such as life and death, that are defined as above financial relationships involves cultural as well as economic processes. Similarly, exclusion from the market—as in the case of children—implies changes in values, not only in social structure.²

This paper will argue that the removal of children from the "cash

² Economists and sociologists have expressed concern over the negative social, economic, and even moral consequences of a limitless expansion of the market (see, e.g., Titmuss 1971; Blau 1967; Hirsch 1978). There is little analysis, however, of specific processes by which activities, relations, or products are excluded from or introduced into the market sector.

nexus" at the turn of the past century was part of a cultural process of sacralization of children's lives. I use the term "sacralization" in the sense of objects being invested with moral and religious meaning. While in the 19th century the market value of children was culturally acceptable, the new normative ideal of the child as an exclusively emotional and moral asset precluded instrumental or fiscal considerations. The primacy of children's qualitative, intrinsic value was affirmed by forsaking any immediate quantitative money value. Historical data on the controversial development of children's insurance between 1875 and the early decades of the 20th century are analyzed as a specific measure of the radical transformation in the cultural meaning of childhood.

THE PRICE AND VALUE OF CHILDREN: LIMITATIONS OF EXISTING INTERPRETATIONS

It is now a documented fact that a major shift in the value of children took place at the turn of the past century: from "object of utility" to object of sentiment and from producer asset to consumer good. Economically, a child today is worthless to his or her parents. He is also expensive. The total cost of raising a child—combining both direct maintenance costs and indirect opportunity costs—was estimated to average between \$100,000 and \$140,000 in 1980 (Espenshade 1980). In return for such expenses, a child is expected to provide love, smiles, and emotional satisfaction but no money. In fact, the contemporary parent-child relationship in America can be considered a prototype of a non-market-exchange relationship. Parents cannot even expect significant public support for their expenses. While in all other major industrial countries a system of family allowances grants children at least partial monetary value, in America income-transfer programs remain inadequate and mostly restricted to female-headed, single-parent households below a certain income level. The actual value of a \$750 tax deduction for each dependent child is determined by the family's tax bracket. For low-income families, the deduction is minimal (Keniston 1977; Kamerman and Kahn 1978).

In sharp contrast to our conceptions of the value of children, the birth of a child in 18th-century rural America was welcomed as the arrival of a future laborer and as security for parents later in life. By the mid-19th century, however, the construction of the economically worthless child was completed among the urban middle class. Fiscal rewards were postponed as concern shifted to children's education as the determinant of future marketplace worth. Yet the economic value of the working-class child increased. Rapid industrialization after the 1860s introduced new occupations for children, and according to the 1870 census about one out of every eight children was employed. Child labor laws, compulsory ed-

ucation, and strict legal regulation of "baby farming" gradually destroyed the class lag. By the early decades of the 20th century, lower-class children joined their middle-class counterparts in the new nonproductive world of childhood. Its new sanctity and emotional value made a child's life industrially taboo. To make profit out of children, declared Felix Adler in 1905, was to "touch profanely a sacred thing" (quoted in Brenner 1971, p. 653).³

Although the shift in children's value is indisputable, Kett notes that "precise characterization of this change has remained elusive" (1978, p. S196). Its sociological import has never been systematically explored. Research on the value of children has been dominated by demographers, and more recently by psychologists and economists, all similarly concerned with parental motivation for childbearing and its relation to fertility patterns (Easterlin 1969; Schultz 1973; Hoffman and Hoffman 1973; Becker 1976; Sawhill 1977; Arnold et al. 1975).⁴ Although these studies are significant contributions to the understanding of children's value, they remain limited by a primarily individualistic and utilitarian framework and by an ahistorical perspective. Microeconomic theorists, for instance, conclude that shifts in children's value are dictated by market shifts in price. Accordingly, children became consumption goods when they ceased to be profitable as economic investment goods. Value preferences "are assumed not to change substantially over time, nor to be very different between wealthy and poor persons, or even between persons in different societies and cultures" (Becker 1976, p. 5).

American historians, on the other hand, seem to be more intrigued with the social creation of adolescence than the changing status of younger, pre-adolescent children (Kett 1977; Katz and Davey 1978; Elder 1980). Existing historical interpretations are psychologically oriented (DeMause 1974) or else focus mostly on the impact of structural change. The success of industrial capitalism at the turn of the past century is assigned primary responsibility for putting children out of work and into schools to satisfy the growing demand for a skilled, educated labor force (Minge-Kalman 1978). Huber suggests that the new economic system also triggered a

³ Class differences in the value of children have not disappeared completely; the economic worth of a child is still a concern in rural areas and sometimes among the urban lower class. The National Child Labor Committee estimates that at least 300,000 migrant children are at work. A recent study suggests, however, that even parents of economically productive children are reluctant to discuss their children as if they were commercial goods (Arnold et al. 1975, p. 43).

⁴ Indeed, since the 1930s, the study of children has been excessively psychological in orientation. The sociology of childhood remains mostly an undeveloped specialty. Significantly, while the first edition of the *International Encyclopedia of the Social Sciences* (1930) included some 12 essays on social aspects of dependent childhood, the new *Encyclopedia* (1968) has only two listings under child: child development and child psychiatry.

conflict of interest between age groups (1976). In an agrarian economy, as in early stages of industrialization, the labor of "little work people" was a welcome alternative which freed men for agriculture (*Niles' Register* [June 7, 1817], p. 226). By the turn of the century a cheap juvenile labor force threatened to depress adult male wages.

Changes in the family also are linked with the shift in children's value. Increasing differentiation between economic production and home transformed the basis of family cohesion. As instrumental ties weakened, the emotional value of all family members—including children—gained new saliency (Smelser and Halpern 1978; Hareven 1977; Zaretsky 1976). In addition, the specialization of women into efficient full-time motherhood, while in reality restricted to the middle class, nevertheless made the widespread labor of children awkward (Rothman 1978; Degler 1980).

Demographic theories contend that the new emotional value of children is best explained by falling birth and mortality rates in the 20th century. In two landmark studies of childhood in Europe and England, respectively, Ariès (1962) and Stone (1977) suggest that in periods of high mortality parents protect themselves against the emotional pain of a child's death by remaining affectively aloof. From this perspective it is "folly to invest too much emotional capital in such ephemeral beings" (Stone 1977, p. 105). A similar cost-benefit accounting explains why falling birth-rates and smaller family size increase the emotional value of each individual child. The economic equation of longevity or scarcity with value remains highly speculative. For instance, Demos submits that in 17th-century Plymouth a high death rate may have encouraged a special concern for and tenderness toward infants (1978; see also Slater 1977).

With some important exceptions (Smelser and Halpern 1978; Greven 1977; Wells 1978; Boli-Bennett and Meyer 1978), the independent impact of cultural variables redefining the value of children in the United States has received much less attention in the literature.⁵ This paper will focus on the cultural dimension, namely, the sacralization of children.

MARKETING CHILDREN'S INSURANCE: A BRIEF BACKGROUND

Ordinary life insurance companies, which had operated in the United States since the latter part of the 18th century, had confined their business to husbands and fathers of middle-class families. In 1875, for the first time, John F. Dryden of the Prudential Life Insurance Company began insuring the lives of children under 10. It was part of industrial insurance, a major marketing innovation aimed at the rapidly expanding work-

⁵ In his explanation of changing family types in England (1500-1800), Stone relies primarily on a cultural explanation, contending that the rise of "affective individualism" was the determining factor (1977).

ing-class population. Besides accepting the insurance of children and women, industrial life policies were available in small units—the average face value of a policy was about \$100.⁶ Industrial agents went into the homes of prospective buyers, usually insuring their lives without medical examination and collecting premiums weekly on a house-to-house basis. They were effective. In four months, one thousand lives had been insured in Newark, New Jersey, including 329 children under 10. After one year of operation, the Prudential had received about \$14,000 in premiums. In 1879 two more companies, Metropolitan Life and the John Hancock, began selling industrial insurance. Perhaps because premiums were as low as 5 or 10 cents per week, the business grew beyond all expectations. By 1895 \$268 million of insurance was in force and the Prudential alone had received over \$33 million in premiums. The company now employed 10,000 agents (Dryden 1895, p. 22; Huebner 1921, p. 276).

As a crucial component of that prosperity, children's insurance became firmly established; one and a half million children were insured in 1896. The greatest growth of the business took place between 1882 and 1902; at the end of that period over 3 million children were insured (Hoffman 1903, p. 4). It was also during those years that the trouble started. Between 1889 and 1902 there were at least 80 legislative attempts across the country to prohibit or restrict the insurance of children's lives as being against public policy and the public interest. In 1884, as sales multiplied, the legislative battle began when Governor Benjamin Butler of Massachusetts included in his address to the legislature the first official suggestion to prohibit the insurance of children. In 1889, Governor Beaver of Pennsylvania revived the attack in his annual message to the legislature, and a bill was introduced to make the insurance of children unlawful in the state. Although the bill was defeated, the antagonism persisted and additional bills were presented—and rejected—in Pennsylvania in 1897, 1903, and 1907. This relentless attack on the industry that insured children was nationwide. Every few years bills were introduced in New York, Ohio, Illinois, Massachusetts, Wisconsin, and 13 other states. In Colorado the opposition was finally successful: in 1893 the insurance of children was declared illegal in that state.

Insuring children was expensive to the parents. Although premiums were low, so were benefits. The industry had to guard itself against the higher mortality of the class insured and particularly of children. Costs were also increased by the need to subsidize an army of industrial agents in their

⁶ These policies were primarily burial insurance. Before the Prudential, urban working-class families joined friendly societies and mutual aid groups. The unsound financial methods used by these associations, however, resulted in frequent failures and little assistance for bereaved families. In addition, any assistance was for the adult dead; few arrangements existed for the death of poor children.

weekly door-to-door collections. Industrial policies had little or no surrender value to their owners, and the lapse rate was extremely high. The battle against children's insurance, however, had little to do with these economic issues. It was a moral crusade by child savers on behalf of poor children. An examination of the arguments and rationales used by the equally relentless opposition and defense in their testimonies to state legislatures, in the press, and other publications reveals a struggle over the changing value of the lower-class child.

THE OPPOSITION: CHILD SAVERS VERSUS CHILD INSURERS

In 1874, only one year before the child-insuring business started, upper- and middle-class groups organized the New York Society for the Prevention of Cruelty to Children, the first and one of the leading institutions of a national child-saving movement. The almost twin births were more than an unrelated coincidence. In different ways, both organizations became involved in the changing status of the lower-class child. The primary goal of child savers, pursued through a wide variety of programs, was to enforce a new respect for the sanctity of poor children's lives, creating for them a special nonproductive world.⁷ To many child savers, child insurers were the enemy. The insurance of poor children stood as an offensive symbol of the prevalent materialistic orientation toward childhood; they opposed it as a form of commercial exploitation even more sordid than most since it speculated on a child's death.

To its detractors, "child-insurance companies"—as they dubbed the industrial insurance business collectively—had "child-blood" on their hands (Waugh 1890, p. 59). Newspapers across the nation carried sensational articles on the dangers of making a child's death profitable. The *Trenton True American*, for instance, suggested that children's insurance be declared invalid as a "dangerous incentive to murder": "It is not only the inducement which inhuman parents . . . find in insurance on their children to ill-treat them or put them out of the way, but it is the tendency to cause them to neglect their children in their sickness and . . . the demoralizing effect produced by parents speculating on the lives of their children" (March 26, 1878). Even within the insurance community some leaders warned that one "should shrink with horror from the ungodly speculation" (Wright 1873, p. 65).

The genealogy of the business did little to dispel fears. Children's insurance began as outright bets among 16th-century European businessmen on the birth and lives of boys and girls (Huizinga 1970, p. 73; de Roover 1945, p. 196). The evidence suggests that in England the insurance of

⁷ On the different programs of the child-saving movement, see Brenner (1972) and Platt (1974).

children by burial clubs, which started in the 1830s and 1840s, was often a sordid affair. In *Past and Present*, Carlyle relates an 1840 case of a mother and father arraigned and found guilty of poisoning three of their children to collect insurance money from a burial society. He adds, "The official authorities . . . hint that perhaps the case is not solitary, that perhaps you had better not probe farther into that department of things" (1918, pp. 4-5). Until the 1870s, government regulation in England was weak and seldom enforced. Baby farmers, for whom the death of one of their children was already a pecuniary gain, made it doubly profitable by insuring the children they "adopted," often in more than one company. A case of this "unspeakable trade partnership between the insurance touter and the ghoulish baby farmer" was presented before the Royal Commission on Insurance in 1875; a child had been insured for the benefit of his guardian in eight societies for a total of £30. During another investigation that same year, a Manchester lawyer testified that already in the 1840s "it was publicly said in the town that the children had been purposely killed [for insurance money]; it was a regular trade" (Campbell 1902, p. 283). Comparative data from France reveal similar accusations against child insurance for dangerously raising the price of children above their emotional value. Anecdotes circulated of mercenary *nourrices* and heartless parents grieving over the recovery of a sick child because it meant the loss of insurance benefits. Indeed, sales booklets popular in both France and Belgium enticed customers quite candidly with the potential profits to be gained from a dying child. One popular pamphlet, for example, included a conversation between two working-class fathers with one outdoing the other in marveling over how much money they had received from the insurance company after the death of one of their children (Goupil 1905, p. 101; François 1906, p. 10; Quiquet 1906).

The available evidence is not conclusive enough to prove beyond doubt that child insurance and child murder were related. However, taking into account the high rates of infanticide in England as in Europe in the 18th and early 19th centuries, the relationship seems likely. If the death of a child, particularly an unwanted child, was a lesser event, the possibility of making money out of it could well become a tempting incentive.⁸

Until the 1880s the opposition to child insurance in the United States remained sporadic and unorganized. But in 1893 the national American Humane Society for the Prevention of Cruelty to Animals and Children passed a resolution that "the practice of insuring the lives of children under 10 years of age, under any pretext, is against public policy." Member organizations were urged to secure prohibitive legislation making it a

⁸ On infanticide, see McKeown (1977); Langer (1972); DeMause (1975). On the mercenary orientation of French wet nurses, see Shorter (1975) and Donzelot (1979).

criminal offense to insure "or in any way offer a reward upon the death of a child" (Read 1895, pp. 19-20). The legislative battle was on. The fiercest fight took place in 1895, when the Massachusetts Society for the Prevention of Cruelty to Children introduced a bill to the legislature to prohibit the insurance of children under 10. For weeks the opposition and the defense argued their cases, making sensational front-page news with their alarming statistics and emotional pleas. Clergymen, physicians, judges, and politicians took the stand to denounce the "diabolical" practice of insuring children. Concerned citizens urged legislators to stop "the traffic in the lives of children" and the "merciless temptation of making young death a profitable event" (McKenzie 1895, p. 28; James 1947, p. 122). Charity workers presented heart-rending accounts of families in extreme poverty whose sick children, often dying of starvation, received no care but whose insurance premiums were paid regularly. Insurance companies were accused of joining with mercenary parents to "feather [their] own nest at the cost of blood and tears and deaths of human beings" (Read 1895, p. 4). Charles C. Read, a vociferous critic, instructed legislators: "I do not know whether you have seen the building of the Metropolitan Company in New York. . . . Go up . . . to the directors' room where the floor is soft with velvet carpets and the room is finished in rich red mahogany . . . and there you will find these gentlemen who think what a beautiful thing this child insurance is. . . ." From every block of marble of that magnificent place peered "the hungry eyes of some starving child." Agents were also attacked as an unscrupulous "band of sharpers": "the more they hustle, the more they make" (Read 1895, pp. 39, 51; Fiske 1895, p. 4). English agents, often imported by American companies for their experienced background, were even suspected of "baby-baiting," paying parents for the death of an uninsured child to attract new customers. Benjamin Waugh, president of the English Society for the Prevention of Cruelty to Children, quoted alleged admissions of agents that "we bait our hook with a dead child" or "I am glad of a funeral, I look out for one. . . . I get business by funerals" (1890, p. 54). On October 15, 1895, the *New York Times* joined the ranks of outspoken critics, its columns exposing the child insurance business as an unconscionable "temptation to inhuman crimes" and insurance agents as "pests of society" (p. 23).

The moral condemnation of children's insurance, however, went beyond the arguments of possible child neglect or murder. For many child savers, it was not the danger of death but the profanation of the child's life that was at stake. They decried parents "ready to traffic in their offspring as if they were horses or goats" (*Insurance Monitor* [February 1881]). To them, the insured child was but another version of the working child, one earning through its death what the other earned by its labor. In both cases, the sanctity of a child's life was polluted by monetary considerations.

Critics asked: "Can any reflecting citizen . . . justify himself in assisting . . . the unnatural practice of child-insurance? . . . It is certainly not too much to claim that there should be no bargaining or trafficking in our Commonwealth under our auspices, in infant life, which has been held sacred" (letter by Chas. F. Donnelly, printed in Read [1895, p. 28]). Insurance agents were despised for making the death of a child a matter of economics, a routine business transaction by which, as the English put it, "you change the child for the pound" (Waugh 1890, p. 41). Regardless of individual motivation, the necessarily commercial involvement of agents with the life and death of little children was defined as morally deviant.

Despite its dramatic impact, the evidence against children's insurance was weak and unconvincing. In fact, the striking feature of the legislative hearings was not the cases of neglect but the intensity of the accusers' fears, based on unquestioned certainty that lower-class parents could be so cheaply bribed into destroying their children. In their moral outrage, and following the often distorted perceptions of middle-class observers, child savers assumed that the economic ties between working-class parent and child could easily become mercenary ones. If they were willing to profit from the labor of their children, parents would be equally willing to profit from their death. "Until fathers and mothers cease to be brutal and drunken," warned anxious critics, "it is not safe to put them in the way of such temptation as this child insurance" (Savage 1895, p. 28).⁹

After six weeks of heated debate, the Massachusetts bill to prohibit the insurance of children was defeated 149 to 23. With the exception of Colorado, all other states similarly endorsed the business. In some ways, the outcome was not surprising. It is true that powerful economic interests were at stake; the death of children had become big business not only for the insurance companies but for undertakers as well. Commercial insurance also saved cities, churches, and taxpayers money previously spent in subsidizing pauper burials. Yet the success of children's insurance cannot be reduced to the clever manipulations of capitalist entrepreneurs at the expense of poor people. The basis for its appeal among the working class is a clue to the changing value of their children.

THE DEFENSE: CHILD INSURERS AS CHILD SAVERS

As a recent insurance writer avows, the purpose of insurance on children has always been "somewhat dubious" (Vogel 1969, p. 60). Legally, life

⁹ Waugh claimed that at least a thousand children were murdered every year in England for insurance money. He insisted that insurance benefits be paid directly and only to undertakers, to stop parents who otherwise chose a "little funeral" for the sake of the "big drink" (1890).

insurance is justified only by the existence of an insurable interest, a reasonable expectation of gain or advantage in the continued life of another person, and no interest in his death. Against objections that child insurance was an illegal wager, 19th-century American courts ruled that the right of parents to the services and earnings of minor children gave them an insurable interest in their lives. Judicial approval thus rested entirely in the pecuniary bond between parent and child. In *Mitchell v. Union Life* the judge declared: "A father, as such, has no insurable interest resulting merely from that relation in the life of a child. . . . But the insurance in the present case was effected by a father upon the life of a minor son. . . . The father is entitled to the earnings of his minor child, and may maintain action for their recovery. . . . He has a pecuniary interest in the life of a minor child which the law will protect and enforce" (45 Maine 105 [1858]).¹⁰ In the late 19th century, the monetary value of even very young lower-class children, many of whom started working before they were 10, made their insurance legitimate. Walford's influential *Insurance Cyclopedia* explained: "Regarding the practice [of insuring children] it may find some defense in the manufacturing districts where every parent has an interest in the prospective earnings of his child" (1871). Insurance companies reminded state legislatures: "The industrial classes have the moral and legal right to insure their children, for it is well known that these children contribute to the support of their families at very early ages. . . . There is a just and reasonable expectation of advantage or benefit from the continuance of their lives and it logically follows that a proper justification inheres in the parents to protect that benefit" (statement by the representative of Prudential Life Insurance Company at the 1893 hearings before the Insurance Committee of the Colorado legislature [Hoffman 1900, pp. 198-99]).

The vice-president of Prudential Insurance Company explained that for identical premiums, eight- or nine-year-olds received higher benefits than one- or two-year-old children partly because "at the older ages, to speak in a commercial and what may seem a heartless way, there gets to be more of a money value in the life of the child" (Dryden 1895, p. 16). This economic appraisal of a parent's interest in his child's survival was not confined to the insurance business. Similarly, compensation for the wrongful death or injury of small children was settled by estimating the actual or prospective loss of earnings of a child from the time of death through his minority, deducting expenses for food and education. In one 1856 case, Hetty Downie, seven years old, was killed by the cars of the New York

¹⁰ See also *Loomis v. Eagle Life Co.* (6 Gray 396 [1856]). Although these landmark cases concerned minors over 14, they served as legal precedent for insuring younger children. The expectation of assistance in old age was another argument legitimating parents' insurable interest in their child's life (15 Hun 74 [1878]).

and Harlem River Company. The court awarded her mother \$1,300 on the basis that "the damages are to be assessed by the jury with reference to the pecuniary injuries sustained. . . . This is not actual present loss which the death produces . . . but prospective losses also" (*Oldfield v. New York, etc. R. Co.*, 14 N.Y. 310).¹¹

But while the law routinely recognized the monetary value of children, the insurance business did not press the issue outside the courts. Instead, insurance leaders themselves adopted their opponents' child-saving rhetoric, stressing the priceless emotional value of children above their cash value. Children's insurance was sold as a symbolic concern for the dying child; it was never marketed as insurance for the working child. This industry approach split child-saving organizations. While some members attacked insurance as a mercenary business, many others enthusiastically welcomed it as a new partner in the defense of childhood. In Massachusetts, for instance, the main spokesman for the opposition was Charles C. Read, attorney of the Massachusetts Society for the Prevention of Cruelty to Children, and yet John Long, vice-president of the same organization, represented insurers. Refuting accusations that "I am opposing the cause of little children," Long insisted that in pleading for their insurance, he was pleading for their welfare (1895, p. 1). Charges of neglect for the sake of insurance money were found to be unsubstantiated. Insurance commissioners from every state attested to the fact that no specific cases of abuse, cruelty, or murder of children for insurance money had ever been reported (extracts from official reports, 1880-1901). In an open letter to the *Philadelphia Evening Bulletin* (December 12, 1894), J. Lewis Crew, secretary of the Philadelphia Society for the Prevention of Cruelty to Children, confirmed the irreproachable record of the industry. In its periodic investigations the society had found no criminal uses of insurance even by baby farmers. Children were insured as "a token of love and affection" and not for profit. In hearings across the nation, leaders of child-welfare organizations, lawyers, politicians, policemen, and physicians testified in favor of the industry, while many clergymen praised it as a "grand blessing" (Long 1895, pp. 14, 35).

Ultimately, the debate over child insurance was a debate on the value of poor children, a public assessment of their emotional versus their economic worth. The director of the Philadelphia Organized Charities called it a "gross libel" to say that "parents in the laboring classes deliberately make away with their offspring for the sake of securing . . . thirty or forty dollars," noting that "among the poorer classes, parents are very fond of their children" (*Child Insurance as Regarded by Anti-Cruelty and by*

¹¹ The monetary value of children was already recognized legally in colonial times; see Speiser (1975) and *The Value of Children* (1882).

Charity Societies 1897). Attacking bills against insurance as discriminatory class legislation, insurance supporters rejected the "repugnant idea . . . that the poorer classes . . . do not have that natural affection for their offspring," insisting at every opportunity that "the natural love of parent . . . beats as strongly under the coarser vest as under the costliest" (report from the Insurance Commission of Wisconsin, June 1, 1904, in *Child Insurance in the Legislatures* [1909, p. 4]; *Weekly Underwriter* [July 24, 1880]). After working among the poor, a Boston minister concluded that "children insured are loved the most" (Long 1895, p. 35).

Sympathetic child savers saw child insurance as a symbolic recognition of the emerging sacred value of poor children's lives. It was not the hope of ready cash but the desire for a proper mourning ritual that prompted poor parents to invest their meager funds in premiums. The press noted: "Thousands of children are assured . . . and this is regarded in the light of a burial fund, to secure for the child in case of its death a decent Christian burial" (*Spectator* [March 13, 1890]). Defending children's insurance as a "grand institution for the poor," Jacob Huack, a Denver shoemaker, wrote Colorado legislators in 1893: "I lost a child which never was insured, and do you know where that poor thing is buried? Away out in the prairie in a place called Potter's Field and there, among unknown men and women, lies that child of mine. It makes my heart ache. . . . I take it as a personal insult when I hear people say that the poor would kill their little ones for a few pieces of silver" (Hoffman 1900, p. 279).¹²

Although child savers admitted that "for a parent to speculate a profit from the death of his offspring is repugnant to the natural feelings," they held that "a provision by insurance for the cost of sacred decencies to the relics and memory of the dead [children] is worthy and legitimate" (Fiske 1895, p. 24). Defending the "sentiment of respect for the body of a child" as a Christian sentiment, supporters elevated buying insurance to an act of piety: "If death should come into the family they want the household to be protected from harsh and profane influence and they want . . . a decent burial" (Fiske 1898, p. 12; *Bulletin of the Bureau of Labor* 1906, pp. 613-14).

Social historians have suggested that concern with the death and proper burial of little children is a telling indicator of their changing value. Although parents in colonial America were never indifferent to the death

¹² Unlike regular adult life insurance, which was rejected by customers in its early stages, children's insurance was opposed by middle-class critics but not by actual or potential buyers. Policyholders testified in defense of the business and signed petitions demanding the right to insure their children. The Boston Typographical Union sent a resolution to the 1895 Massachusetts hearing that "the passage of such a law . . . limiting the age of children [for insurance] would be a detriment to the interests of all working men and a reflection upon their intelligence."

of their children, studies detect a certain aloofness and emotional detachment from the child in the 17th and 18th centuries. Young death was lamented but passively accepted (see Stannard 1977; Walzer 1975; Illick 1975). In her perceptive analysis of American culture, Ann Douglas suggests that a change took place in the 19th century between 1820 and 1875. She describes the "magnification" of mourning during that period, specifically, the surge of concern among the middle class with the sorrow of a child's untimely death. The emotional pain of the bereaved father and mother became the subject of countless stories and poems. Mourners' manuals instructed parents how to cope with the tragedy of a "vacant cradle," while elaborate coffins were designed for the "small household saints" (Douglas 1978, pp. 243, 246, 251). The acceptance of children's insurance suggests that after 1875 lower-class parents adopted middle-class standards of mourning young children.

Insurance historians at the time were convinced that this new concern with children's burial was inspired by the excessive mortality in industrial areas during the 1870s. All demographic records indicate, however, that it was precisely during that period that children's mortality rates began a dramatic decline.¹³ Mortality records of insurance companies, based on the lives of their lower-class customers, show that the reduction cut across class lines. By 1896 the mortality of poor children had decreased to the extent that companies were willing to raise benefits without significantly increasing premiums. Insurance officials were quick to congratulate themselves for the demographic improvements, suggesting that their business lifted the poor to "a higher plane of life" (Dryden 1895, p. 6). Insured children had in fact lower mortality rates than the uninsured at comparable age levels. For example, between 1897 and 1901 the expected mortality for American children one to two years old was 46.6 per thousand, but mortality for children of that age insured by Prudential was only 31.6 per thousand. For ages five to nine the expected mortality during those years was 5.2 per thousand nationally but only 4.4 for those insured by Prudential (Hoffman 1903, p. 23; Jones 1893). Insurance companies did not require any medical examination for children, so the lower mortality was not a result of selected lives.

Since the concern with children's death was not due to rising mortality rates, the alternative demographic explanation is that children were

¹³ Vinovskis suggests that death rates remained stable between 1800 and 1860 (1972). Yasuba argues that death rates increased in the decades before the Civil War, particularly among urban children under 10 years of age (1962). Both agree, however, that mortality rates decreased significantly in the latter part of the 19th century. According to Gore, the mortality for children aged five to nine in cities fell from 14.1 per thousand in 1850-54 to 5.9 per thousand in 1895-1900 (1904). In Massachusetts, the death rate in 1865 of children aged five to nine was 9.6 per thousand. By 1900 it had decreased to 5.3 per thousand (*Historical Statistics* 1960, p. 30).

mourned more deeply as they began to live longer. It is difficult, however, to determine precise causal links between changing mortality rates and emotional attachment to children. Unidimensional explanations, perhaps theoretically satisfying, seldom capture the complexity of real social change. The surge of concern about the death of poor children in the late 19th century, which became the main sales appeal of the insurance business, was determined by the interaction of many variables, from demography to changes in the economic and occupational structures, and even by the marketing techniques of undertakers and life insurance agents who "sold" parents on giving their children a proper burial. It is my argument that one important variable was the cultural redefinition of the value of children. As children's lives became economically worthless but emotionally priceless, their deaths became a social problem.¹⁴

CONCLUSIONS: FROM A PROPER BURIAL TO A PROPER EDUCATION

The struggle over children's insurance reflects the transformation of lower-class childhood that began in the latter part of the 19th century. The business was caught in the transition. To its opponents, children's insurance was an extension of the old utilitarian view of childhood. Yet child insurers had an economic interest, if not a moral one, in the goals of the child-saving movement. The new respect for the sanctity of children's lives was good for business. Insurance companies became active partners in the effort to prolong young lives; they distributed free booklets instructing parents on proper care of their children and even sent visiting nurses to tend the sick or to assist new mothers.¹⁵ The union of child savers and child insurers was formalized in the Child Welfare Conference of 1909, at which the president of Metropolitan Life, a guest speaker, called it "one of the anomalies of both insurance history and of child welfare history, that . . . since the introduction of industrial insurance . . . well-meaning men and women have taken occasion to condemn the insurance of children" (Frankel 1909, p. 1).

The business continued its expansion, and by 1928, 37.4% of all policies

¹⁴ Ariès too stresses the cultural hypothesis, pointing out that the surge of sensitivity toward the value of children's lives between the 13th and 17th centuries in Europe preceded by more than a century any reduction in mortality. He attributes the change to the cultural impact of Christianity and its discovery that the child's soul is immortal (1962).

¹⁵ Companies did not insure babies in their first year of life until the 1920s when their mortality began to decrease significantly. Black children were not insured until 1881 and even then received lower benefits than white children. The discrimination was explained as a "commercial matter," namely, the higher mortality rate among blacks (Dryden 1895, p. 30).

issued by the three largest insurance companies were for children. Postwar sales increased beyond all expectations; in 1945 \$10 billion worth of life insurance was in force for children under 15. By 1950 the total was \$17 billion (Thornton 1940, p. 68; Scoins 1953, p. 122). The changing value of children's lives, however, transformed the legal foundations of the business after the 1920s. The strictly pecuniary standard of insurable interest became progressively inadequate to measure the worth of the economically worthless child. Already in the 1880s courts began recognizing the validity of ties of love and affection as a measure of the child's value. In a landmark decision, the judge in *Warnock v. Davis* declared: "It is not necessary that the expectation of advantage or benefit should be always capable of pecuniary estimation; for a parent has an insurable interest in the life of his child. . . . The natural affection in cases of this kind is considered as more powerful—as operating more efficaciously—to protect the life of the insured than any other consideration" (104 U.S. 775). Legal criteria used for recovery in the wrongful death of children shifted in the same direction, from estimating the economic loss of a child's earnings to weighing the loss of its love and companionship. In the 20th century, courts rejected the "child-labor" standards and the "bloodless bookkeeping imposed upon our juries by the savage exploitations of the last century" (*Wycko v. Gnodtke*, 361 Mich. 331 [1960]; 72 Misc. 2d 332). It was recognized that only in exceptional cases, such as child actors (Shirley Temple, for example, was insured for \$600,000 at the age of nine in 1936), did parents lose money when they lost a child. Indeed, sustaining the legal fiction of pecuniary loss would lead to the awkward conclusion that the average child had a negative worth and its death was a benefit for parents.

Other reasons for insuring children also emerged. Rituals for the dying child became less necessary as lower-class children began living longer. Instead, there were new pressures to subsidize the living unproductive child. By the 1930s, a family of moderate means with an average income of \$2,500 a year needed an average of \$7,766 to raise a child to age 18 (Dublin and Lotka 1930, p. 55). The marketing approach of insurance companies mirrored the new status of children as expensive consumer items; policies were now sold as "nest eggs" for children. Endowments that matured by the age of 16 or 21, creating funds for an education or a dowry, became the most popular policies. As it moved from burial coverage to education fund, children's insurance gradually became also a middle-class type of investment. While 19th-century insured children came from working-class families, today 74% of children in households with incomes of \$20,000–\$24,999 have some form of life insurance, compared with only 37% in households with incomes below \$6,000 (*Families and*

Their Life Insurance 1940, p. 61; Gregg 1964, p. 105; *The 1949 Buyer* 1950).¹⁶

The success of children's insurance cannot be understood simply in economic terms. Even insurance experts agree that funds for a child's education can be more profitably accumulated through investments other than a life policy. Mehr considers that arguments to sell life insurance for children are "more effective in making the sale than in solving the buyer's problems" (1977, p. 118). The 1974 *Consumers Union Report on Life Insurance* reports that children's insurance is irrational in economic terms. From the start, customers have been drawn by the symbolic appeal of a policy, a token of respect for the dead child in the late 19th century and one of love for the living child in the 20th century. Agents' manuals and insurance-selling booklets recognize the noneconomic appeal. One booklet admits: "There are few tangible advantages for you in buying life insurance on your son. . . . You would be buying life insurance to pay him back his love, his trust, his respect, his confidence, you would be paying back for all those wonderful unforgettable moments that only a little boy can share with his father." A 1951 manual instructs agents that the market for children's insurance is "as wide and as deep as the love of parents and grandparents for their children and grandchildren." Insurance advertisements have gradually hushed all reference to dying children and infant burials (*Give Your Son a Hand* 1958; Gravengaard 1951; Willard 1979).

This analysis of the history of children's insurance in the United States serves as an indicator of broader changes in the value of children's lives, specifically the emergence of the economically worthless but emotionally priceless child. The expulsion of children from the market at the turn of the past century, although clearly shaped by materialist concerns, ultimately exceeded a mere economic calculus. A shift in values redefined normative conceptions of childhood in America. As the emotional and moral uniqueness of children was stressed, pragmatic pecuniary equations of their value became increasingly inadequate. The new "sacred" child had to be kept off the market, useless but loving. The study of additional problem areas—as, for example, child labor, foster care, baby sales, and compensation for wrongful death and birth—will permit a better understanding of the complex relationship between the human and market values of children's lives.

¹⁶ While available records do not suggest major sex differences in the insurance of children by industrial companies in the early period, after the 1920s middle-class parents insured their sons much more often than their daughters (Vogel 1969).

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History and Career in a Bureaucratic Labor Market¹

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This paper presents a synthesis of theoretical statements on the determinants of socioeconomic achievement within bureaucratic (internal) labor markets and an empirical examination of the resultant predictions. The study focuses on careers in the U.S. civil service from 1963 to 1977, using official personnel records on a 1% sample of white collar federal employees, along with secondary survey data. The career is viewed as an outcome not only of individual attributes but also of both organizational and historical contexts. The empirical strategy used to disentangle the individual, organizational, and historical effects is cohort analysis. Five successive entering cohorts are differentiated, and for each a model of socioeconomic achievement is estimated which incorporates key characteristics of bureaucratic labor markets not heretofore considered in such models. The considerable power of the model to account for variations in occupational prestige and salary supports a Weberian view of bureaucratic labor markets as highly rationalized personnel systems, although ascriptive characteristics also affect career success in the federal service. The cohort analysis reveals important organizational and historical effect which are interpreted as demonstrating the influence of variations in opportunity structure on socioeconomic achievement. The paper concludes with a call for greater sociological attention to the interrelationship of individual biography and societal history.

At the intersection of societal history and individual biography is the career "The facts of contemporary history are also facts about the success and the failure of individual men and women. . . . Neither the life of an individual nor the history of a society can be understood without understanding both" (Mills 1959, p. 3).

For over a decade the sociology of career achievement has built on the Blau-Duncan (1967) model of the socioeconomic life cycle. However

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elaborations of the basic model have focused mainly on mechanisms of intergenerational status transmission, to the neglect of intragenerational career processes. In particular, status-attainment research has tended to ignore the structure and operations of formal organizations, the labor market context within which the career outcomes of individuals are most commonly determined. Nor have the effects of macrolevel historical forces on individual careers received much attention. However, theoretical and empirical work in organizational research, economics, and the status-attainment tradition itself has recently begun to place the socioeconomic career in its social context. I treat aspects of that literature below, highlighting key concepts and developing theoretical expectations about the organizational determinants of career success. The resultant predictions are then examined, in a cohort analysis of socioeconomic achievement in the U.S. civil service from 1963 to 1977.

Civil service systems appear to approximate the "internal" or, more precisely, the "bureaucratic" labor market in pure form (Caplow 1954). Thus, the determinants of career success in the largest such labor market in the American economy are of considerable theoretical and intrinsic interest (Taylor 1979). More broadly, the analysis demonstrates that an adequate understanding of socioeconomic achievement necessarily involves both the historical and the structural contexts in which careers occur. "Stratification theories seek to explain the features of social differentiation in a society by reference to the historical conditions that have produced them" (Blau and Duncan 1967, p. 3). Stratification research must do no less.

BUREAUCRATIC LABOR MARKETS

Spilerman (1977) has discussed the division of labor markets, and in turn careers, along occupational, industrial, and organizational lines. These dimensions of labor market segmentation tend to overlap, forming two distinct employment sectors. "Certain jobs (the primary sector) are organized around *internal* labor markets. Here . . . entrants are insulated from nonemployees (the *external* labor market) in competing for promotion and salary," while workers in the secondary sector are generally in occupations, industries, and organizations without strong internal labor markets (Spilerman 1977, p. 582). An internal labor market is an administrative unit within which labor allocation and wage determination are governed by administrative rules and procedures, at least partially shielded from the direct economic influences of the national, regional, or local labor market (Doeringer and Piore 1971). An "enterprise" internal labor market is contained within a single organization, while in a "craft" internal market, the administrative unit is a union hiring hall and workers are employed by

diverse firms. Both types may include either blue-collar or white-collar employees, but research has focused primarily on the former.

Closely related to the economic concept of an internal labor market is the bureaucratic labor market discussed by Caplow (1954). The bureaucratic labor market (hereafter abbreviated BLM) shares the main features of the enterprise internal labor market, including definite points of entry, stable employment within a single organization, recruitment of higher officials primarily from those lower in the hierarchy, and hiring and promotion based on established bureaucratic procedures. However, Caplow's more sociological concept is particularly relevant here, since Caplow follows Weber in emphasizing the bureaucratic organization of *white-collar* workers and in treating civil service systems as empirical approximations to the ideal type of BLM. The notion of a labor market confined to a single bureaucratic system also draws attention to the fact that position in the societal stratification hierarchy is increasingly dependent on a worker's position in an organizational hierarchy (Stinchcombe 1965).

Two major dimensions of socioeconomic achievement in a BLM are salary and occupational prestige. Within a given BLM, salary corresponds closely to organizational rank, though allowance is also made for seniority. The federal General Schedule pay system, for example, has 10 seniority steps within each of its 18 hierarchical grades. In part the correspondence to rank reflects the common assumption that authority relations will be undermined if subordinates are paid more than their nominal superiors (Doeringer and Piore 1971, p. 86). In addition, "the average official naturally desires a mechanical fixing of the conditions of promotion: if not of the offices, at least of the salary levels . . . from the lower, less important, and lower paid to the higher positions" (Weber 1946, p. 203). Since such desires for "orderly" careers (Spilerman 1977) are in accord with bureaucratic rationalization and with an organization's need for a stable, motivated work force, they are often realized in the salary structure of a BLM. In short, though personal income has customarily appeared in status-attainment and related "human capital" models, salary is of greater interest within a BLM because it is simultaneously a measure of individual economic reward and a formal attribute of an organizational position.

In a BLM, where a salary structure is established and administered rationally (in the Weberian sense), there are at least two straightforward mechanisms through which salary is affected by occupational prestige. First, "there are occasionally wage relationships that are dictated by the status of a job which management is forced to recognize" (Doeringer and Piore 1971, p. 87). That is, social definitions of appropriate remuneration may link occupational prestige with salary. Second, the salary structure of a BLM is based typically on elaborate methods of job evaluation and position classification (Caplow 1954; Doeringer and Piore 1971; Grandjean

1980). These methods depend on classifiers' ratings of jobs on rather abstract dimensions, such as complexity of the work or the level of knowledge required. Any bias in such ratings deriving from the halo of occupational prestige will create an additional linkage between occupational standing and salary (see Asch, Block, and Hertzman 1938).

THE BUREAUCRATIC CAREER

The process by which the rewards of salary and occupational prestige are achieved by individuals in a BLM has evoked considerable theoretical disagreement based on little evidence. Three major viewpoints, partially contradictory though at points complementary, underlie the present empirical analysis: (1) that the variables customarily used to explain socioeconomic achievement in the external labor market are largely *irrelevant* to careers in a BLM; (2) that, on the contrary, personnel practices in a BLM are in fact singularly *consistent* with the rationalistic assumptions of the usual models of achievement; and (3) that the main influence on success in a BLM is the *timing* of career events in relation to the history of the individual, the organization, and the society.

Career Lines and Career Success

The view that the usual predictors of socioeconomic achievement are of little consequence within a BLM has been expressed by Kaysen (1973, p. 149), who argues that "our implicit model of education and attainment is the model for professors, M.D.'s, engineers, and perhaps lawyers. . . . But the investment-in-education model applies much less readily to the business or civil service world, where stratified bureaucracies with formal job structures and internal promotion systems operate in a rather different way." The most relevant form of human capital is not education, general work experience, or even Becker's (1964) enterprise-specific experience but informal on-the-job training specific to a given cluster of related jobs in one BLM (Doeringer and Piore 1971). Hence, mobility tends to be confined within job clusters, each of which is connected to one or more low-level ports of entry into the BLM. Together with their respective entry ports, these clusters constitute career lines, the major avenues within which achievement occurs (Spilerman 1977). Once a worker enters the organization through a given port, "the key decisions about his [or her] career follow. The occupational decisions that he made earlier will be of relatively little significance" (Ginzberg 1972, p. 171).

If socioeconomic achievement depends primarily on differential opportunities for advancement inherent in various job clusters (rather than directly on education, prior occupational experience, or the like), access to one or

The Bureaucratic Labor Market

another career line is crucial. Administered labor markets "tend to establish rather stable channels of recruitment. Most important are the incumbent employees who refer friends and relatives to the internal market" (Doeringer and Piore 1971, p. 139) and perhaps to the most advantageous ports of entry as well. Consistent with the latter possibility is Granovetter's (1974) report that those of his respondents who had found their jobs through social contacts were on the average in better-paid positions than those who had not. In addition to friendship and kinship, social class, ethnicity, and sex seem to stream workers into various career lines (see Kanter 1977).

The same factors should have a continuing influence on career chances after entry. Some workers will have more information about opportunities for promotions or beneficial job shifts and better prospects for favorable action from decision makers when such opportunities arise. Ascribed attributes are used not only in hiring but also in decisions about salaries and work assignments, as presumptive proxies for a constellation of character traits—reliability, drive, congeniality—deemed valuable to the organization (Caplow 1954). Indeed, such attributes may be even more influential later in the career than during the initial screening for employment if attitudinal similarity is more strongly valued at higher levels in the organizational hierarchy, where peers are fewer and interactions correspondingly more intense.

Bureaucratic Rationality and Socioeconomic Achievement

A second, more Weberian viewpoint on the determinants of prestige and salary in a BLM holds that some of the usual human capital and status-attainment predictors of occupation and earnings are, if anything, more, not less, influential in this context than in the external market. Because of the rationalized personnel practices characteristic of bureaucracy, "it is more likely that purely functional points of consideration and qualities will determine . . . selection and career" (Weber 1946, p. 201). Hence universalistic criteria should be paramount in a BLM, especially education, while discrimination by ethnicity, sex, or class should occur mainly through differential access to the primary and secondary labor force sectors rather than within a specific BLM. Although this point of view has few adherents (e.g., Stinchcombe 1965), it is supported by some of the limited evidence available (Smith 1976; Stolzenberg 1978).

Cohorts and the Organizational Career

Still a third perspective on careers emphasizes the organizational processes that characterize the operation of a formal organization in a given historical context. In this view, success is a matter of being in the right place at the

right time. Hence the questions raised concern place in the organization; time in the life history of the individual, the organization, or the society; and the circumstances under which the two are right for each other.

Location on virtually any dimension along which an organization is differentiated may have consequences for career success. Such dimensions include, besides the vertical differentiation of authority, the geographic distribution of the organization's activities and differentiation into separate departments. Opportunities for advancement may be greater in the home office, though some experience in the field may also be advantageous. Similarly, the largest plant or department, or the one most closely identified with the organization's major product, is often the right place to be for career success (see Martin and Strauss 1959).

The right time to be there is during a period of organizational or departmental growth since rapid growth creates openings in the hierarchy, while decline, or even slower-than-average growth, constricts opportunities (Downs 1967). Either effect may be compounded if the organization's growth or decline is part of a boom or recession in the external labor market. A general economic slowdown should increase the number and qualifications of external applicants seeking work at all levels in the BLM. Prosperity, on the other hand, tightens the external labor supply and encourages the organization to offer inducements to retain its own workers (Doeringer and Piore 1971). Further, Rosenbaum's (1979) analysis of a single large American corporation shows that the organization's growth during the boom of the late 1960s did not benefit only those who were typically most likely to be promoted (the younger and more educated workers); it also had a spillover effect on less favored groups. A similar spillover might benefit employees whose disadvantages stem not from age or inadequate education but from race or sex (Doeringer and Piore 1971).

The timing of organizational growth in relation to the employee's own career is also significant. Growth may be most advantageous for those hired *before* a spurt, despite the commonsense belief that opportunities are greatest for those hired *during* a period of expansion. Workers hired during expansion become part of an "age lump" in the organization (Downs 1967, p. 20). Like those born in the post-World War II baby boom, such workers face stiff competition throughout their careers because of the sheer numbers of their peers, and their chances of career success may suffer as a consequence.

These considerations underscore the importance of cohorts in the study of organizational careers (Reed 1978). Comparisons across successive birth cohorts have permitted demographers to begin sorting out (1) effects associated with membership in a specific cohort and due to the unique experiences of that cohort; (2) effects of external historical events which cut across cohorts; and (3) effects associated with biological aging or its

sociological counterpart, progression through the stages of the life cycle (see Ryder 1965). For an organization, cohorts can be defined on the basis of hiring date instead of birth date. Analogous cohort, period, and "career cycle" effects can then be identified.

As Glenn (1977) points out, it is rarely possible to separate these intertwined effects by purely statistical means. When combined with information on relevant historical events, however, cohort analysis allows at least some plausible inferences to be drawn. By analyzing the careers of successive entering cohorts of U.S. civil servants, I attempt here to deal simultaneously with issues "of biography, of history, and of their intersections within social structures" (Mills 1959, p. 143).

THE U.S. CIVIL SERVICE

Labor Market Characteristics

Although large and highly diverse, the federal government may usefully be regarded as constituting a single bureaucratic labor market for its white-collar workers (Couturier et al. 1979; Taylor 1979). Agencies, bureaus, and occupations within the BLM form a network of overlapping submarkets, as is characteristic of white-collar markets within large enterprises (Doeringer and Piore 1971, p. 3). Explicit regulations, promulgated under congressional authority by the Office of Personnel Management (OPM; formerly the Civil Service Commission), apply uniformly to almost all white-collar employees. For example, white-collar salary schedules are uniform across the nation, while wage rates for federal blue-collar workers are pegged to those prevailing for similar work in the various local or regional markets. For the relatively few agencies with merit systems not directly administered by OPM, the major personnel regulations are nevertheless standardized by law and compatible with OPM procedures; even white-collar tenure rights are generally transferable throughout the federal government (see Couturier et al. 1979).

In pointing out that federal employment displays the features of a BLM, I do not claim that it is in any strict sense representative of all BLMs. In the federal government there is no direct equivalent to the profitability criterion of private enterprises. Perhaps more important, the administrative regulations of the federal BLM have the force of law, and some of these regulations (e.g., the veterans' preference) are peculiar to public employment. Indeed, its size alone makes the federal BLM atypical. Yet previous analyses of government bureaucracies have shed light on formal organizations in general (e.g., Meyer and Brown 1977), and an examination of socioeconomic achievement in federal employment is similarly instructive for a general theoretical understanding of organizational careers.

Recent History

The dominant event in American history over the past two decades was undoubtedly the war in Vietnam. Its effect on the size of the federal civilian labor force is clearly evident in table 1. In 1960, federal civilian employment stood at 2.4 million, about what it had been throughout the Eisenhower administration. As late as November 1965, federal planners were projecting total growth through 1969 at 4% (U.S. Civil Service Commission 1965). The actual increase during the ensuing four-year period was well over 20%.²

The largest component of this growth occurred in the federal civilian agency which was already by far the largest, the Department of Defense. The American military commitment in Vietnam went from 23,000 advisers in 1965, when Lyndon Johnson's State of the Union Address focused almost entirely on domestic issues, to a peak of over a half-million troops in 1968; by 1972, Richard Nixon's "Vietnamization" of the war had brought that figure back down to 39,000 (Morison, Commager, and Leuchtenburg 1977).

TABLE 1
TRENDS IN FEDERAL EMPLOYMENT, 1960-77*

YEAR	CIVILIAN EMPLOYEES							MILITARY PER- SONNEL
	Total	Defense	State	Postal	VA†	HEW	Other	
1960...	2,399	1,047	38	563	171	62	518	2,476
1961...	2,436	1,042	39	582	175	70	528	2,484
1962...	2,514	1,070	40	588	179	77	560	2,808
1963...	2,528	1,050	42	587	175	81	593	2,700
1964...	2,501	1,030	41	585	172	83	590	2,687
1965...	2,528	1,034	41	596	172	87	598	2,655
1966...	2,759	1,138	43	675	174	100	629	3,094
1967...	3,002	1,303	47	717	176	106	653	3,377
1968...	3,055	1,317	47	731	177	117	666	3,548
1969...	3,076	1,342	44	739	176	113	662	3,460
1970...	2,982	1,219	41	741	167	112	702	3,066
1971...	2,923	1,154	40	729	174	115	711	2,715
1972...	2,865	1,108	36	706	184	114	717	2,323
1973...	2,824	1,053	35	698	194	128	716	2,253
1974...	2,893	1,070	33	707	204	142	737	2,162
1975...	2,897	1,042	30	699	211	147	768	2,128
1976...	2,883	1,010	30	676	219	155	793	2,082
1977...	2,893	1,009	31	658	224	159	812	...‡

SOURCES.—Bureau of Manpower Information Systems (1976, tables A, C, E; 1977, tables 1, 2).

* As of June 30 of the indicated year; figures in thousands.

† All except 1977 approximated by applying proportion VA in average annual employment (from source table A) to June 30 employment (from source table E); 1977 taken directly from source table 2.

‡ Comparable data not available.

² The proportion of the federal work force which is blue collar, currently less than a quarter, has declined gradually since 1960, and the general trends reported in table 1 for all civilian employees also hold when only white-collar workers are considered (see Garganis 1974).

The associated expansion and contraction of civilian defense employment synchronized almost perfectly with the military trend, and, as table 1 shows, the same is true for the other federal agency concerned mainly with foreign affairs, the State Department.

A similar pattern is evident in the second-largest U.S. agency, the Postal Service, but for reasons only loosely connected with the war. As the national economy entered the longest cyclical upswing in history, from 1961 through 1969 (Gordon 1974), and as the leading edge of the baby boom reached adulthood, both business and personal mail volume increased substantially (see Myers 1975). Long a labor-intensive operation, the Post Office responded predictably by expanding its work force. However, the resultant budget deficit stimulated congressional reorganization of the Post Office Department, which became the quasi-independent U.S. Postal Service in 1971. A contract settlement with the four major postal unions then granted postal employees significant salary concessions; the new management's subsequent attempts to eliminate the deficit have focused on reducing the number of workers through mechanization (Adie 1977; Myers 1975). Otherwise, the reorganization has brought little change to the relationship of the Postal Service to the larger federal BLM. The pay scales remain national in scope, and the testing, scoring, and hiring procedures now administered by the Postal Service are essentially the same as those previously applied by the Civil Service Commission (Couturier et al. 1979).

The growth of the third-largest federal agency, the Veterans Administration (VA), has followed a different pattern, which nevertheless reflects primarily the impact of the war. Almost constant in size throughout the 1960s, it increased by nearly 30% after 1971. This delayed expansion was due in no small part to the need for veterans' services generated by the Vietnam conflict.

The Department of Health, Education, and Welfare, the fourth-largest agency, displays yet another pattern of growth. When President Johnson declared war on poverty in January 1964, "enlistments" in HEW and other social service agencies began a steady rise that continued until the beginning of the Nixon administration. Sustaining that growth was a record-setting volume of congressional activity during the period, including Medicare, federal aid to education, civil rights legislation, and other Great Society programs. After a period of stagnation beginning in 1969, HEW and related social service agencies showed substantial renewed growth from 1973 through 1977, notwithstanding Nixon's much publicized impoundment of appropriated funds for social services shortly after his reelection (see Morison et al. 1977). His increasing preoccupation with the Watergate scandal during this period probably contributed to his professed inability to control the bureaucracy (see Woll and Jones 1975), at least with respect to its size.

With few exceptions, the remaining federal agencies have displayed a pattern of consistent growth since 1960 or before (see Gartaganis 1974). It has been slow compared with that experienced intermittently by the four largest agencies, but it has been steady. Still, the growth of these smaller agencies hardly supports the common belief in an ever-swelling federal bureaucracy. Indeed, by 1974 the total federal labor force had leveled off near 2.9 million, after a peak of 3.1 million in 1969.

Prosperity in the national economy has fluctuated widely in the past two decades. Although economic expansion began before the war in Vietnam, it was then intensified by military spending. Despite a private labor force enlarged by the baby boom and by an increasing proportion of women working for pay, unemployment continued to drop from the 4% "full employment" criterion reached in 1965 to 3.5% in 1969 (Gordon 1974). The tight labor supply made recruiting federal personnel difficult (Couturier et al. 1979). However, with the war winding down, more restrictive economic policies during the early Nixon administration precipitated a mild recession from 1970 through 1972. This "Nixonomics" recession, as it was labeled in the press, saw unemployment rising above 6% (Gordon 1974); federal recruitment became correspondingly easier. There was a brief recovery in early 1973, but unemployment throughout the 1970s remained substantially above the low levels of the mid-1960s.

RESEARCH DESIGN³

The foregoing account suggests that *where* and *when* one entered the federal civil service during the past two decades might be matters of considerable importance for career success. In the analysis below, I examine their consequences by differentiating a sample of June 1977 federal employees into five groups on the basis of date of entry. Each of four cohorts is identified with a major historical period coinciding with its members' entrance into federal service: (1) the Great Society cohort, entering between January 1, 1963, and December 31, 1965, just before the rapid expansion of the federal work force; (2) the Vietnam cohort of 1966-68; (3) the Nixonomics cohort of 1969-71; and (4) the Watergate cohort, whose entry in 1972-73 marked the end of the federal labor force decline and the beginning of roughly steady-state employment. Those entering in 1974 or later are not included, to insure that all employees analyzed have been in the federal government the three years required in the competitive service to attain career tenure rights. This restriction also enhances the homogeneity of the Watergate cohort since one year amounts to a greater proportion of the elapsed federal career in 1977 for these employees than for earlier cohorts.

³ Data and measures are discussed here in highly abbreviated form; I can supply details on request.

Conversely, as Ryder (1965, p. 858) points out, precise dating of cohorts is of decreasing significance later in the life cycle because "adjacent cohorts tend to permeate one another as the pattern of life chances works itself out." Therefore, and because of constraints of data availability, the fifth cohort consists of (5) long-term employees, a quasi cohort of all pre-1963 entrants.

The bulk of the analysis uses a 1% random sample of federal civilian employees drawn from the computerized file of official personnel records maintained by OPM. To explore intergenerational effects and external labor market influences, I also present a secondary analysis of interview data (described later) on a smaller cross section of employees sampled from the same official file. That file covers virtually the entire executive branch (excluding White House staff, intelligence personnel, employees of the Tennessee Valley Authority, and the politically appointed heads of agencies), plus the General Accounting Office, the Government Printing Office, the U.S. Tax Court, and the administrative offices of the federal court system (see Schneider 1974).

The cohort analysis is confined to full-time, white-collar employees, as of June 30, 1977, who are included in one of the five entry cohorts and for whom data are available on all variables of interest. Only employees who were 50 years old or younger at entry⁴ are included in the analysis, to minimize differential cohort attrition due to death or retirement (see Glenn 1977). Employees must also have been in active federal employment at least 75% of the total elapsed time between entry and June 1977. In this way some discontinuity in employment is allowed, provided that the employee's main career is apparently federal.

The cohort samples are expressly not restricted to white-collar *entrants*. Much as Blau and Duncan (1967) report for the national labor force, cross-tabulations for the five cohorts show considerable one-way permeability of the federal white-collar BLM to workers entering government employment in blue-collar jobs. That is, almost all white-collar entrants (97%-99%) remain in white-collar occupations, but a sixth to a third of the blue-collar entrants had become white collar by 1977. Though the latter workers include only about 5% of the total 1977 white-collar labor force in each cohort, to delete them from the analysis would be to neglect Spilerman's (1977) emphasis on port of entry as a determinant of subsequent career trajectory in a BLM. Therefore I treat blue-collar versus white-collar entry as a possible source of career-line differentiation. Because my interest is in careers within the white-collar BLM, I examine only the first of Spilerman's two alternative perspectives on career lines, omitting consideration of the career outcomes of blue-collar entrants who remain blue collar.

⁴ Entry date is not included in the data for pre-1963 entrants; the age restriction for the long-term cohort refers to age in 1963.

COHORTS AND CAREERS IN THE U.S. CIVIL SERVICE, 1963-77

The model of socioeconomic achievement examined here (fig. 1) is based on that which is now standard in the status-attainment literature, with modifications appropriate to the federal BLM. Following the arguments advanced earlier, the model postulates occupational prestige as a determinant of salary, and occupation and salary at earlier times as influencing occupation and salary at later times. The BLM variables added to the model as determinants of rewards include seniority, agency, and employment in the Washington, D.C., metropolitan area (the federal "home office"), along with a variety of other employee characteristics, both biographical (education, age, sex, ethnicity, social origin) and bureaucratic (veterans' preference, entry port).⁵ Means and standard deviations of all variables for all five cohorts are provided in the Appendix.

Regression estimates of model parameters are provided separately for the long-term cohort in table 2, for the Great Society and Vietnam cohorts in table 3, and for the Nixonomics and Watergate cohorts in table 4. For

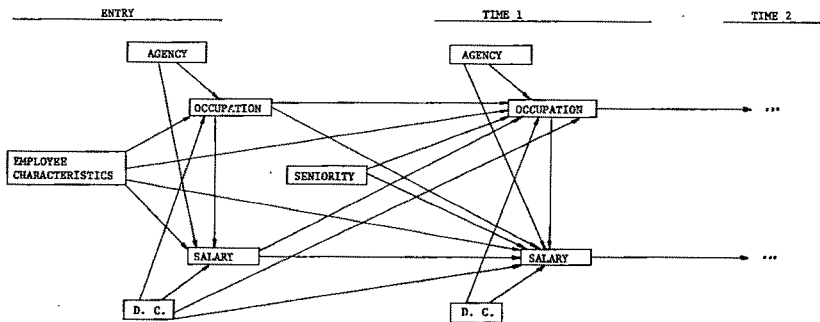


FIG. 1.—Schematic model of socioeconomic achievement in the federal civil service. For simplicity, the following are combined under the heading of employee characteristics: education, age, minority-group membership, sex, blue-collar entry port, veteran status, kinship with another federal employee, farm origin, father's occupation, and most recent nonfederal occupation.

⁵ Throughout the analysis, salary is inflated to 1977 dollars by compounding all statutory salary increases since 1963. Minority-group members include black, Spanish-surnamed, Puerto Rican, American Indian, Oriental, Aleut, and Eskimo employees (see Taylor 1979). Agencies are grouped into five categories based both on functions and on similarities in patterns of growth between 1960 and 1977. The four resultant dummy variables include (1) defense-related agencies (the Defense and State departments, plus the Selective Service, the National Security Council, and the U.S. Information Agency); (2) the Postal Service; (3) the Veterans Administration (plus the Soldiers' and Airmen's Home); and (4) social service agencies (HEW and HUD, plus the Railroad Retirement Board, the Commission on Civil Rights, the Equal Employment Opportunity Commission, the Office of Economic Opportunity, and the Community Services Administration). The comparison category consists of all other departments, agencies, and commissions for which OPM maintains personnel records. The remaining operational definitions should be apparent from the variable labels in the Appendix.

the two most recent cohorts, two time points in the career cycle are examined, entry and early postentry (i.e., 1977), while for the Great Society and Vietnam cohorts, the longer time span permits an examination of entry, early postentry (1970), and mid-career (1977). Because entry characteristics are unavailable for pre-1963 entrants, the data on the long-term cohort are limited to three time points in the middle to late career (1963, 1970, and 1977).

Explanatory Power of the Model

What draws immediate attention in these tables is the considerable power of the model to account for variation in occupational prestige and salary. Whether at entry or later in the career, however simple or elaborate the prediction equations, and whatever the cohort, the R^2 -values at the bottom of each table show that the variance explained always exceeds one-fourth, usually approaches half, and often exceeds 80%. Even the simplest human capital prediction equation (eq. [1]), in which the *only* independent variables are education, age, and the square of age, accounts for about 40%-50% of the variance in 1977 salary in every cohort. In this respect, the Weberian view of a BLM as a highly rationalized personnel system receives convincing support.

The other independent variables in the model cannot be readily identified as either human capital or BLM characteristics. For example, the influence of entry occupation or starting salary on the later career may be viewed either as reflecting indirect effects of human capital or as demonstrating the extent to which initial placement determines subsequent success in a BLM. Similarly, human capital theorists such as Becker (1964) would regard seniority as an indicator of enterprise-specific skills or on-the-job training, but its effects might also be attributed to BLM regulations designed more to promote employment stability than to stimulate "investments" in this type of human capital (see Doeringer and Piore 1971).

Wherever the line is drawn between human capital and BLM variables, the R^2 -values reported in the tables suggest that the explanatory importance of BLM characteristics cannot be dismissed, bureaucratic rationality notwithstanding. Under the most restrictive assumptions, with education, age, occupation, salary, seniority, and even minority-group status and sex all treated as non-BLM variables, and with the explained variance already above 80% the remaining variables still add several percentage points to the explanatory power of the model. Thus, while the Weberian view of the rationalized BLM is supported, there are apparently other influences on members' careers in a BLM which also warrant examination.

The explanatory power of the model is substantially uniform across cohorts, but the effect parameters show some important intercohort differ-

TABLE 2

SOCIOECONOMIC ACHIEVEMENT AMONG LONG-TERM EMPLOYEES: NET METRIC REGRESSION COEFFICIENTS
FOR PRE-1963 ENTRANTS TO THE U.S. CIVIL SERVICE^a

INDEPENDENT VARIABLE	DEPENDENT VARIABLE AND DATE OF DEPENDENT VARIABLE					
	Occupation					
	1963	1970	1977	1963	1970	1977
Education.....	2.28***	.66***	.21**	4.31***	3.50***	1.69***
Age, 1963.....	.71***	-.35*	-.14	3.45***	-1.70*	.10
Age squared, 1963 (100s).....	-.89**	.42†	.21	-4.00***	1.77†	-.96
Seniority ^b00	.00	.00	.20***	-.02†	.00
Minority status.....	-2.79***	-.58	.09	-9.60***	-4.04**	-3.68*
Female.....	-4.18***	-1.50***	.12	-23.33***	-16.16***	-5.99***
Blue collar, 1963.....	-12.33***	2.46***	2.21***	3.44	-12.03***	-.24
Disabled veteran.....	-3.75***	-.59	.43	-8.40**	2.02	.23
Other veterans' preference.....	-1.55***	-.43	.39	-.67	1.13	1.18
Defense/State ^b32	-.35	-.43	-.91	-.87	-9.17***
Postal ^b	-2.80***	-1.30**	-.83*	-15.72***	-25.08***	9.64***
V.A. ^b	1.68*	-.49	1.18**	-6.92*	-8.03***	-7.86***
HEW/HUD ^b	-3.59***	-.77	-.47	-2.60	10.15***	2.87
D.C., 1963.....	1.46**	1.61*	.26	9.33***	.79	-11.87***
D.C., 1970.....	...	-.56	.30	...	11.20***	9.79***
D.C., 1977.....10	9.56***
Occupation, 1963.....66***	.16***	1.41***	.13	.08
Occupation, 1970.....63**74***	-.01
Occupation, 1977.....40***
Salary, 1963 (\$100).....01†	.0088***	.10***
Salary, 1970 (\$100).....0099***
Constant.....	-1.20	12.49***	11.03***	-109.06***	-14.92	-17.68

^a Unstandardized coefficients from ordinary least-squares regression with all relevant independent variables controlled. Coefficients significant at the .05 level but only in a one-tailed test are indicated by †. See the Appendix for variable means and standard deviations.

^b As of the date of the dependent variable.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

TABLE 2 (Continued)

INDEPENDENT VARIABLE	DEPENDENT VARIABLE AND DATE OF DEPENDENT VARIABLE				
	Occupation				
	1963	1970	1977	1963	1977
	Salary (\$100)				
<i>R</i> ² -values: ^a					
Eq. (1).....	.371	.360	.314	.329	.405
Eq. (2).....659	.698	.418	.890
Eq. (3).....	.371	.659	.698	.514	.890
Eq. (4).....	.382	.661	.698	.549	.893
Full eq.....	.468	.666	.702	.569	.902

^a All *R*²-values have been adjusted for degrees of freedom. Eq. (1) includes as predictors for each dependent variable only education, age, and the square of age. Eq. (2) adds occupation and salary as predictors. Eq. (3) further adds seniority, and eq. (4) includes all of the foregoing plus minority status and sex. The full equation includes all relevant independent variables shown in the table and is the equation for which the regression coefficients are reported.

TABLE 3

SOCIOECONOMIC ACHIEVEMENT IN THE GREAT SOCIETY AND VIETNAM COHORTS: NET METRIC REGRESSION
COEFFICIENTS FOR 1963-65 AND 1966-68 ENTRANTS TO THE U.S. CIVIL SERVICE

INDEPENDENT VARIABLE	COHORT, DEPENDENT VARIABLE, AND DATE OF DEPENDENT VARIABLE									
	Great Society (1963-65)					Vietnam (1966-68)				
	Occupation		Salary (\$100)			Occupation		Salary (\$100)		
	Entry	1970	1977	Entry	1970	1977	Entry	1970	1977	1977
Education.....	1.92***	1.02***	.15	3.83***	3.98***	1.74***	2.16***	.86***	.64***	4.78***
Entry age.....	.39	.23	-.35†	2.96***	.29	-.92	.49*	.21	.02	2.09**
Entry age squared (100s).....	-.56	-.32	.41	-3.08*	-.71	.89	-.73*	-.24	-.11	-1.85†
Seniority.....	.03	.02	.0123**	.03	...	-.03*	-.04**	...
Minority status.....	-.21	-.21	-.15	.48	-.56	1.81	-1.25*	-1.07*	-.50	-.02
Female.....	-4.05***	-1.88**	-.38	-12.05***	-13.72***	-13.05***	-4.84***	-1.74***	4.16***	-10.71***
Blue-collar entry.....	-15.33***	2.86**	1.78*	6.33	-2.53	3.60	-15.22***	1.53*	.29	7.70*
Disabled veterans.....	-.78	-.74	-.25	-1.31	-.45	1.47	-2.52*	1.37	.27	6.20
Other veterans' preference.....	-1.24	-1.29*	.32	4.28†	-2.80	3.47	-2.15***	-1.12*	-.40	7.00***
Defense/State.....	-1.35†	-.57	-.62	9.10***	3.99†	-6.35**	1.45*	-.33	-.73	7.59***
Postal.....	-3.14***	-1.93**	-1.68*	7.63***	-22.57***	12.71***	-1.11	-.54	-1.27*	8.07***
VA.....	-1.12	-1.20	1.57	-2.95	-5.93	3.48	-1.24	-.44	-1.63*	-4.70
HEW/HUD.....	-1.93†	-1.20	.22	4.66	8.04*	3.91	-1.37	-.08	.62	13.31***
D.C. entry.....	-.97	-.14	.26	13.91***	1.31	-11.36***	.41	3.08***	-.63	10.12***
D.C., 1970.....75	-1.03	...	6.68†	14.81***	...	-2.26*	-.46	...
D.C., 1977.....63	...	9.97*	9.97*71	...
Occupation, entry.....61***	1.54***08	-.38**68***	.12***	1.58***
Occupation, 1970.....54***	...	1.14***	.1656***	.12
Occupation, 1977.....44***
Salary, entry (\$100).....01	-.0172***	.0100	.01	...
Salary, 1970 (\$100).....02*	1.01***00	...
Constant.....	10.72*	-1.17	16.48***	-97.96***	-74.71***	8.41	5.16	.67	11.17***	-97.74***
R ² -values:										
Eq. (1).....	.309	.397	.336	.325	.484	.457	.342	.415	.411	.392
Eq. (2).....605	.665	.465	.782	.856734	.674	.518
Eq. (3).....605	.665	.465	.785	.857735	.677	.518
Eq. (4).....	.329	.606	.664	.498	.786	.867	.363	.739	.676	.552
Full eq.465	.675	.669	.521	.827	.882	.492	.744	.686	.574

NOTE.—See footnotes to table 2.

TABLE 4

SOCIOECONOMIC ACHIEVEMENT IN THE NIXONOMICS AND WATERGATE COHORTS: NET METRIC REGRESSION COEFFICIENTS FOR 1969-71
AND 1972-73 ENTRANTS TO THE U.S. CIVIL SERVICE

INDEPENDENT VARIABLE	COHORT, DEPENDENT VARIABLE, AND DATE OF DEPENDENT VARIABLE									
	Nixonomics (1969-71)					Watergate (1972-73)				
	Occupation		Salary (\$100)			Occupation		Salary (\$100)		
	Entry	1977	Entry	1977		Entry	1977	Entry	1977	
Education.....	2.47***	.74***	5.22***	3.28***		1.79***	.72***	4.38***	2.86***	
Entry age.....	.51†	.09	1.70†	-.25		.69*	.22	1.17	2.38**	
Entry age squared (100s).....	-.67	-.18	-.78	.25		-1.00*	-.36	-.45	-3.67***	
Seniority.....		-.02		.49***			.04		.75***	
Minority status.....	-2.25**	-.69	-1.82	1.03		-1.41*	-.62	-5.51*	-1.59	
Female.....	-3.40***	-1.50†	-11.74***	-20.49***		-2.61***	-.92	-8.03**	-7.99***	
Blue-collar entry.....	-9.62***	6.04***	1.65	-.23		-15.71***	9.96***	27.52***	-1.26	
Disabled veterans.....	-4.03*	.32	-7.23	1.33		-2.12	-.40	-1.47	9.18†	
Other veterans' preference.....	-.71	-.60	5.34†	7.31*		-1.70*	-.25	3.19	.28	
Defense/State.....	2.76***	-.13	5.26†	-20.23***		.71	-1.50*	-1.91	-12.08***	
Postal.....	-.78	-3.06***	9.78**	-11.44***		-1.18	-2.53***	29.22***	-10.78***	
VA.....	.75	2.23*	-1.77	-16.97***		1.66	-.47	-4.23	-12.45***	
HEW/HUD.....	-1.89	-1.24	15.84***	.79		-1.40	-.79	1.42	-.13	
D.C., entry.....	1.33	-1.52	7.17*	-7.75		.56	.64	11.33***	7.95†	
D.C., 1977.....		1.50		23.96***			-.91		-2.98	
Occupation, entry.....		.51***	1.46***	.27			.67***	1.41***	.26†	
Occupation, 1977.....				.31†					.78***	
Salary, entry (\$100).....		.03***		.86***			.01		.82***	
Constant.....	-.56	10.43*	-95.71***	-48.05**		7.87†	1.63	-72.88***	-78.04***	
<i>R</i> ² -values:										
Eq. (1).....	.417	.401	.447	.446		.269	.330	.257	.390	
Eq. (2).....		.591	.546	.734			.612	.329	.776	
Eq. (3).....				.742			.613		.790	
Eq. (4).....	.439	.590	.571	.771		.274	.614	.391	.793	
Full eq.....	.484	.622	.588	.806		.352	.648	.494	.808	

NOTE.—See footnotes to table 2.

ences. In the following subsections, I examine each of these effects in approximately the order in which they appear in the tables. My discussion relies on apparent *patterns* of effects, without recourse to formal significance testing for intercohort differences.

"Human Capital" Effects: Education, Age, and Seniority

Overall, a year of education returns about two prestige points at entry and \$400 in starting salary. Education continues to bring direct returns in both prestige and salary throughout the career, though at diminishing rates. These results are consistent with the predictions of human capital theory, which sees education as a proxy for productive skills valued by employers (e.g., Becker 1964), but they do not rule out alternative interpretations. Returns to education could also reflect mainly "credentialism," or simply the beneficial effects of the "cultural capital" that formal schooling helps develop, including work attitudes and interpersonal styles acceptable to one's superiors (see Collins 1979). Except in a handful of occupations, federal law prohibits the use of an educational credential as an exclusive qualification for employment. However, education is explicitly permitted as a substitute for required experience, with a year of college generally taken as the equivalent of nine months of experience, and a year of graduate study equated with a year of experience (Couturier et al. 1979, p. 223). As Weber (1946, p. 241) observed, "Educational certificates in all fields make for the formation of a privileged stratum in bureaus and offices [and may] support their holders' claims . . . to monopolize socially and economically advantageous positions" without regard to the technical rationality of such claims.

The effects of age are also consistent with a human capital interpretation, though again other explanations are possible. Age at entry, a rough proxy for potential prior experience in the labor force, brings positive socioeconomic returns in all cohorts, especially with respect to salary. The quadratic term, square of age, is in general negatively associated with socioeconomic achievement, as predicted by a hypothesis of decay or obsolescence of human capital acquired through experience. Even more markedly than education, the direct effects of age appear to operate primarily at entry, a possible reflection of "orderly" careers in a BLM, in the sense defined by Spilerman (1977). That is, in comparison with external labor market findings (see Kelley 1973), later achievement is quite rigidly determined by prior occupation and salary in the BLM and correspondingly less subject to the direct influences of other employee characteristics. Still, the sample includes only employees with substantially continuous federal careers and hence may be biased toward orderliness.

On first inspection, the effect of seniority also seems to conform to a human capital interpretation. Within each cohort, variations in seniority measure differences in precise dates of entry and in time lost to discontinuities in service. In accord with the view that seniority represents organization-specific, on-the-job learning, each month of seniority brings a net salary increment in the first years after entry, amounting to about \$25 for the long-term, Great Society, and Vietnam cohorts, and about \$50 and \$75 for the Nixonomics and Watergate cohorts, respectively. However, seniority has little or no influence on occupational prestige, and in the three earliest cohorts, in which the model includes a third point in the career cycle, the effect of seniority on 1977 salary is entirely mediated by 1970 salary. Furthermore, preliminary analyses showed that the square of seniority is a consistently insignificant predictor of both occupation and salary in all cohorts (leading to its deletion from the model).

For these reasons, I interpret the influence of seniority on salary, not primarily as a return to human capital, but as a result of BLM administrative practices that institutionalize seniority per se as a basis of remuneration (see Weber 1946). The seniority steps built into white-collar federal pay schedules become more widely spaced with longer time in grade, and the prescribed minimum time between grade promotions is greater beyond the lowest grades. These two administrative features of the federal pay system probably account for the trivial direct effect of seniority on salary in the later career for the long-term, Great Society, and Vietnam cohorts. Also, the larger returns to seniority in the two most recent cohorts support the common opinion that promotions and step increases are now treated as more automatic than formerly; in other words, they represent compensation for time in grade more often than for exemplary performance.

The Effects of Minority-Group Membership and Sex

Following Taylor (1979), I refer to the additive effects of minority status and sex, net of the other variables in the model, as primarily indicative of discrimination, although alternative explanations (e.g., possible differences in previous employment history) are not altogether ruled out by the available data. Interaction effects involving race or sex (see Featherman and Hauser 1976*a*, 1976*b*) are not examined here, nor are the potentially unique experiences of specific ethnic groups (see Taylor and Kim 1980).

Much of the discrepancy in socioeconomic achievement between minority and nonminority federal employees can be traced to occupation at entry: minority entrants consistently score about two prestige points lower than others. This difference probably represents a combination of occupational stereotyping by race in the external labor market and discrimination in the

placement of entrants within the federal BLM itself. Since at least 1963, there appears to have been little racial discrimination in occupational achievement after entry into the federal service. Net of prior occupation, the estimated effect of minority status is generally negative but very weak; only one of the eight relevant coefficients differs significantly from zero.

Salary differences present another pattern. The Great Society and Vietnam cohorts show essentially *no* discrimination against minorities with respect to salary at entry; nor do they show any significant net salary differentials by 1970. The explanation might be sought in the vigor of the civil rights movement during the period, in selective recruitment and retention of exceptionally qualified or motivated minority employees, in President Kennedy's 1962 Executive Order establishing "affirmative action" in the federal service, or in a spillover effect from economic prosperity and growth in the federal labor force. Yet whatever the precise mix of these historical factors (see Burstein 1979), the apparent equality of opportunity by race for employees who entered the federal service during the 1960s is in sharp contrast to the experience of the long-term cohort and to the salary differentials between minority and nonminority civil servants which are evident in cross-sectional analyses (e.g., Smith 1976; Taylor 1979).

Unfortunately, this sanguine conclusion does not apply to the period since 1970. As the tables show, minority employees' entering salaries in the two most recent cohorts were \$180 and \$550 below those of nonminority employees, net of other salary determinants, and the 1977 salaries of minorities in the long-term, Great Society, and Vietnam cohorts fell behind those of their nonminority co-workers by similar amounts after 1970. The trend is not entirely uniform, and the salary differences mentioned are not all statistically significant, but the direction of the trend is clear enough. When "benign neglect" replaced affirmative action as the key to White House racial policy during the Nixon administration (see Morison et al. 1977), the impact was more than merely semantic.

The direct effect of sex on occupation declines over the career cycle, as does the effect of minority-group membership, but the sex coefficients are roughly double the corresponding minority coefficients. Women average three to five prestige points below men at entry, net of other variables, and another one to three points lower at the early postentry career stage. The lower values on these ranges are found in the two most recent cohorts, perhaps suggesting an intercohort decline in occupational stereotyping by sex. For the three earlier cohorts, there is essentially no difference between men and women in 1977 occupational prestige, once prior occupation is controlled; that is, the chances of mobility were equal for males and females in these cohorts after 1970, in part because neither men nor women display much mobility in the later stages of their federal careers.

On the other hand, the effects of sex on salary are pervasive and cumulative. With other determinants of salary (including occupational prestige) controlled, men command entering salaries \$800-\$1,300 higher than those of women in all cohorts. At each succeeding stage in the career, the gap widens by an *additional* \$600-\$2,000. An optimist might discount the especially large discrepancies in the Nixonomics cohort as a result of sampling variability or as a political or historical aberration; if that is done, the remaining analyses do suggest an intercohort decline in salary discrimination consistent with the preceding results on occupational attainment. The early 1970s saw an increased political awareness of sex-related social issues (see Freeman 1973) and in particular the establishment of the Federal Women's Program within the U.S. Civil Service Commission. Therefore it would not be unreasonable to infer that a period of feminist activism has had some positive effect on women's opportunities for socio-economic achievement in the federal government, just as the civil rights movement may have improved the career chances of minorities in the 1960s. However, the salary disparity that remains in 1977 still equals or exceeds the amount by which the gap has narrowed since 1963, and the recent trend for minorities suggests that it will not be narrowed further without continued strong political pressure. The disparity becomes especially striking when it is recalled that the analysis is restricted to men and women employed full-time in 1977, with substantially continuous service since entry, and that the accumulated seniority variable acts as a control for remaining male-female differences in career continuity.

On Port of Entry

Dichotomizing entrants into blue collar and white collar permits only a limited representation of Spilerman's (1977) argument regarding entry ports, mobility clusters, and career lines. Yet the degree of disaggregation described by Spilerman, who would identify as many as 10,000 separate career lines, presents severe practical problems even with very large samples (Spenner and Otto 1979). It also raises a question of theoretical parsimony. The collar-color dichotomy is certainly parsimonious and appears a priori to be a salient criterion for distinguishing entry ports. (Collins 1979). Although the dichotomy is crude, its influence on career success might be considerable.

As it turns out, tables 3 and 4 indicate that blue-collar entry is no great handicap to long-run success in the white-collar federal service for those who do cross the barrier. Not surprisingly, the occupational prestige of blue-collar entrants is, at entry, substantially below that of their white-collar counterparts. Given the inclusion in the analysis of only those blue-

collar entrants who later achieved white-collar jobs, it is equally unsurprising that they display above-average upward mobility at each subsequent stage in the career. Net of their lower entering prestige, the blue-collar entrants received above-average starting salaries, especially in the most recent cohort. However, in none of the cohorts entering the federal service since 1963 is there much difference between the eventual salaries of blue-collar and white-collar entrants, once entry salary is controlled. The net effect of blue-collar entry on later salary is negative but never approaches statistical significance. The information on the long-term cohort in table 2 suggests that those who make the switch to white-collar work well along in their careers do suffer a short-term salary disadvantage, but even for them the long-run direct effect of earlier blue-collar work on 1977 salary is essentially nil.

Veterans' Preference

The effects of another BLM variable, the veterans' preference, are also rather modest. An honorably discharged veteran of active duty who makes the minimum passing grade on an entrance examination has five or 10 points added to the test score, as does the spouse or mother of a service casualty. The extra points allow these applicants to rank higher on a competitive register and thus should increase their chances of appointment at a higher grade. Certain disabled veterans receive a further benefit in addition to the bonus points: "Their names float to the top of the register of eligibles in most examinations." Finally, employees hired under the veterans' preference enjoy some protection from layoff during reductions in force (see Bureau of Manpower Information Systems 1976, p. 46).

Notwithstanding these advantages, the tables show that employees claiming the preference, especially the disabled, enter the federal service a point or two below nonveterans in occupational prestige and may also have slightly poorer chances of upward mobility thereafter. These results are consistent with evidence that the armed services have tended to draw those with lower socioeconomic status and fewer marketable skills and that the loss of civilian labor force experience in military service impedes subsequent socioeconomic achievement (see Cutright 1974). The prestige disadvantage probably accounts in large measure for the lower average salaries of federal employees claiming veterans' preference (Taylor 1979). When prestige is controlled, veterans who are not disabled enjoy starting salaries \$300 or \$700 greater than nonveterans. Over and above any benefit of veterans' preference and the detriment of low prestige, physical disability appears to generate a disadvantage in entry salary of about the same amount. Neither category of veterans is consistently distinct from other federal employees with respect to salary increments later in the career.

Agency Effects⁶

The interplay of historical and organizational influences on success in a BLM is shown with particular clarity in the parameter estimates that summarize the relationship of agency to occupation and salary. The concentration of clerical positions in the Postal Service is evident in a comparatively low occupational prestige at entry and in little upward mobility later in the career. A preponderance of clerical work probably accounts also for the similar, though less pronounced, pattern in the various agencies grouped together here under the heading of social services, principally HEW and HUD. In contrast, the defense-related agencies (dominated numerically by the Defense Department itself) hire a larger proportion of professionals and administrators (see Couturier et al. 1979, table A-1), a fact reflected in above-average entry prestige in all cohorts for this agency category.

The coefficients of the four agency variables are almost uniformly negative for postentry occupation, suggesting that the likelihood of upward prestige mobility during the career is greatest in the residual category, made up of all other federal departments, agencies, and commissions. This is also the category for which growth during the period of study, though slow, was steadiest. With this exception, however, the pattern of occupation coefficients in these tables does not relate systematically to the agency growth and nongrowth shown in table 1. Apparently, opportunities for prestige and mobility have more to do with the occupational structure dictated by an agency's mission than with short-run fluctuations in its size.

The VA offers an extreme case of the importance of organizational mission for occupational structure and, in turn, for socioeconomic achievement. Almost half of the VA's white-collar workers are employed in a few medical occupations, including nurses, nursing assistants, and physicians. Over 75% of all the federal workers in each of these specialities are in the VA. The occupations are quite heterogeneous with respect to prestige, and thus the association of the VA dummy with prestige is generally small and somewhat unstable. But with respect to salary, all are, in one sense or another, dead-end occupations, a fact which is clearly reflected in the tables. Nurses and nursing assistants are concentrated in the lower pay grades at entry, and their career ladders top out early. Physicians, of course, command some of the highest entering salaries in the federal service, presumably as inducements to forgo private practice. Yet given the statutory ceiling on federal pay, their high starting salaries mean that physicians too have limited opportunities for subsequent increases. Hence these seemingly disparate occupational career lines combine to yield on the average moderately low entry salaries for the VA and, at each later point in the career,

⁶ Because of collinearity among lagged agency variables ($r > .8$), I have treated the effects of agency as entirely contemporaneous.

salary increments which fall \$350–\$1,700 below the average salary increases enjoyed by employees of agencies in the residual category. The rapid growth of the VA that began about 1972 seems to have had little if any effect on this pattern.

Nor has growth been the most noticeable historical influence on salaries in the Postal Service. The tables indicate that postal employees have enjoyed above-average starting salaries, net of other relevant variables, since at least 1963. After the 1971 reorganization, this advantage in entry salary more than tripled—to nearly \$3,000 above the comparison category. Concurrently, the financial prospects of postal workers in the three earliest cohorts (long-term, Great Society, and Vietnam) also improved substantially, as evidenced by an advantage of \$1,000–\$1,500 in 1977 salary once 1970 salary is controlled. Yet these marked gains coincided with a decline of over 10% in postal employment. The likely explanation is the contract settlement reached after reorganization, which provided increases in base pay and guaranteed semiannual cost-of-living raises (Adie 1977). This settlement apparently negated any effect of declining organizational size during the period.

Besides improving entry salaries, the contract settlement seems to have improved postal workers' prospects for salary raises during the early years of their careers. With prior salary controlled, postal employees in the long-term, Great Society, and Vietnam cohorts all fell roughly \$2,500 short of the residual agencies in increments to 1970 salary. In the Nixonomics and Watergate cohorts, postal workers were still unable to keep pace with employees in other agencies in the early years after entry, but the deficit was only \$1,100. These results are consistent with Spilerman's synthetic cohort analysis showing an unusually flat age-earnings profile for mail carriers. Spilerman (1979, p. 569) attributes his findings, based on 1970 census data, to a "salary schedule for post-office workers which makes little allowance for years in grade." In addition to outright pay raises, the contract settlement reached after reorganization reduced from 21 years to eight the time required to reach the top step in grade (Adie 1977). Thus, the early earnings profile should now be less flat than in 1970, and indeed the deficits are smaller in the two more recent cohorts, corroborating Spilerman's interpretation. The fact that some deficit remains despite the increase in base pay shows the resilience of the underlying flat salary trajectory.

Organizational growth has been of considerably greater consequence for salary attainment in the defense-related agencies than in either the VA or the Post Office. In the Great Society and Vietnam cohorts, defense employees enjoyed starting salaries \$800–\$900 above those of the residual category. However, with defense-related civilian employment winding down after

1969, the entry advantage of a defense agency declined to statistical insignificance in the Nixonomics cohort and even became a slight disadvantage for the Watergate cohort. For all cohorts, the effect of the defense agency dummy on 1977 salary, net of prior salary, is significantly negative and quite large, ranging from \$600 to \$2,000. The prospects for pay-grade promotions were bleak indeed for employees in those shrinking agencies during the 1970s.

Note also the benefit that accrued to being present on the ground floor of the expansion in the 1960s. The Vietnam cohort, which was unusually large precisely because of the expansion, felt slowed growth and incipient decline almost immediately. As early as 1970, the coefficient for the defense dummy indicates a salary \$850 below the comparison category. In contrast, for those in the long-term and Great Society cohorts, working in a defense agency did not present a measurable obstacle to salary attainment in 1970, though the negative effect was substantial by 1977. Since these workers were already within the federal BLM when the expansion began to open opportunities, the effect of constricting opportunities was not so rapidly apparent. Their career chances also benefited from the absence of an age lump such as that confronting the members of the large Vietnam cohort in the competition for promotions (see Downs 1967).

Growth has also affected the prospects for salary attainment in HEW and related social service agencies. In these agencies starting salaries were comparatively high for all cohorts, especially in the Vietnam and Nixonomics cohorts. The entry advantage in the latter cohort runs counter to the expected effect of growth since 1969 through 1971 was a no-growth period. Post hoc, one might impute the advantage to a willingness in the early Nixon administration to fill vacancies in high-grade leadership positions with new hires, despite the overall reduction in hiring into the social service agencies. In any case, the intercohort differences in the effect of the social service dummy on 1970 salary are consistent with the ground-floor phenomenon noted above. Vietnam-era entrants to the social service agencies suffered an immediate restriction of opportunity for pay-grade promotions when agency growth temporarily slowed, while the long-term and Great Society cohorts continued to enjoy above-average salary increments through 1970 and 1977.

After 1972, growth in the social service agencies occurred at about the same rate as in the residual agencies, and the coefficients relating social service employment to 1977 salary are correspondingly small in all cohorts. Overall, the preponderance of negative coefficients for the effects of the four agency variables on salary indicates that the steadily growing agencies in this residual category enjoyed a fairly consistent salary advantage between 1963 and 1977, just as they enjoyed a prestige advantage.

Headquarters versus Field

Employment in the Washington metropolitan area shows only a weak positive association with occupational prestige, but its effect on salary is substantial, conferring a benefit of about \$1,000 at each point in the career. Since federal white-collar pay scales are uniform nationwide, this result cannot be interpreted as a simple cost-of-living factor but must reflect a more favorable grade distribution at organizational headquarters, where the top administrative positions are disproportionately located. Short-run time-lagged effects are clouded somewhat by collinearity among the lagged District of Columbia dummy variables (with intercorrelations of $r = .7$ and above). The short-run lagged effects on salary are small and inconsistent, but the long-run effect of beginning one's federal employment in Washington is uniformly *negative*. For the two earliest cohorts, it is quite substantially so (over \$1,100). That is, at any given point in the career, salary opportunities are greater at the home office; net of this relationship, however, the employees commanding the higher salaries are those whose early experience was in the field, where the actual mission of the organization is pursued.

The Cumulative Effects of Occupation and Salary

The remaining variables in tables 2, 3, and 4 need only brief attention since they present few surprises. Occupational prestige at any point in the career is strongly dependent on occupation in the preceding stage, an effect which is quite similar in magnitude across all cohorts. A weaker but statistically significant lagged effect, also positive, links earliest occupation with 1977 occupation in the three cohorts in which the lag can be tested. Thus, the chance of upward mobility in mid-career is greater for those who entered in higher-prestige occupations, net of all other variables in the model. Although this result could be an artifact of measurement error (see Kelley 1973), it may also indicate a continuing effect of entry point on career lines in a BLM.

No consistent lagged effect is evident in the determination of salary by earlier salary. As with occupation, the direct link between salaries at successive points in the career is uniformly strong and positive—an apparent expression of orderliness in the federal career. Salary is also strongly affected by contemporaneous occupation, as hypothesized. Lagged effects of occupation on salary, and vice versa, are negligible.

Intergenerational and External Labor Market Influences

The preceding results omit family and external labor market characteristics because the records on which the cohort analysis relies do not include such information. However, these issues can be partially explored by means of a

secondary analysis of data from a national probability sample of federal employees interviewed in 1967 by the Survey Research Center (SRC) (see Interuniversity Consortium for Political and Social Research [ICPSR] 1978, p. 130). I have analyzed a subsample of the interviews, those with white-collar employees who entered the federal service before 1963 and who were less than 55 years old in 1967. To the extent possible, the subsample represents the same population as does the long-term sample, although the match is not perfect.⁷ The findings are reported in table 5.

The effects of the first 11 variables on federal occupation and salary are by now quite familiar and require little comment. I will point out only that the lower average occupational prestige of minorities and women in the federal service cannot be attributed entirely to occupational stereotyping in the external labor market since the estimated effects of race and sex on federal occupation are significantly negative even with most recent external occupation controlled. Indeed, if anything the federal service obtains female entrants from higher-status external jobs than males, perhaps because women in high-prestige occupations perceive greater salary discrimination by sex in private than in federal employment.

Other results of interest in table 5 concern points of contact between the federal BLM and stratification in the larger society. First, a rudimentary test of the influence of social contacts on career success in a BLM yields results counter to expectations. Having a relative who is also a federal employee should, at the very least, increase one's knowledge of opportunities for advancement in the BLM and might mean having an ally able to confer advantages directly. However, the survey respondents who report being related to other civil servants are not at all advantaged in either prestige or salary.

Second, family background has been proposed as an important determinant of career success in a BLM since access to the best career lines and decisions regarding advancement within a career line may be influenced by considerations of social class and its actual or presumed correlates. Yet table 5 shows no significant direct effect of family background on federal occupation or salary, with education, prior occupation, and other relevant variables controlled. Social origin, crudely measured by father's occupational prestige, seems to have a slight positive effect on salary, and a farm background may generate a salary disadvantage of \$200 or so. However, neither variable is associated as expected with federal occupation, and they

⁷ The SRC sampled only employees at least 21 years old and excluded a few agencies. No major modifications of the operational definitions used in the cohort analysis are required, except that salary must be approximated as the pay associated with the median step of the reported grade. Also, for the 10% of respondents who had no prior employment, the sample mean is used for external occupational prestige on the Siegel (1971) scale.

TABLE 5

INTERGENERATIONAL AND EXTERNAL LABOR MARKET EFFECTS ON SOCIOECONOMIC ACHIEVEMENT: NET METRIC AND
STANDARDIZED REGRESSION COEFFICIENTS, WITH VARIABLE MEANS AND SDs, FOR A 1967
CROSS SECTION OF PRE-1963 ENTRANTS TO THE U.S. CIVIL SERVICE^a

INDEPENDENT VARIABLE AND UNITS	COEFFICIENTS AND DEPENDENT VARIABLE						
	Metric			Standardized			
	External Occupation	Federal Occupation	Federal Salary (\$100)	External Occupation	Federal Occupation	Federal Salary	MEAN SD
Education (years).....	2.31***	1.76***	6.50***	.405***	.367***	.245***	13.43 2.18
Age (years) ^b	1.22*	-.56	.384†	.695*	-.426	.526†	42.75 7.96
Age squared (years squared, 100s)...	-1.61†	.51	-4.36	-.566†	.315	-.489	18.91 6.50
Seniority (months).....02***	.10***161***	.133***	170.31 80.97
Minority status (1,0).....	-2.91†	-5.13***	-10.12†	-.081†	-.170***	-.061†	.141 .348
Female (1,0).....	3.19**	-4.68***	-38.13***	.116**	-.202***	-.298***	.288 .453
Defense/State (1,0).....	...	-3.36***	-6.61	...	-.151***	-.054	.336 .473
Postal (1,0).....	...	-6.06***	-28.00***	...	-.273***	-.228***	.336 .473
VA (1,0).....	...	-5.39***	-30.62***	...	-.128***	-.131***	.066 .248
HEW/HUD (1,0).....	...	-4.66*	-7.30	...	-.109*	-.031	.064 .244
D.C. (1,0).....	...	-.09	32.57***015	.180***	.116 .320
Relative of a civil servant (1,0).....41	-4.74	...	-.004	-.040	.417 .494
Farm origin (1,0).....	-1.01	.41	-2.18	-.035	.017	-.016	.240 .428
Father's occupation (Siegel prestige)	.09†	-.04	.29†	.086†	-.044	.061†	39.17 12.24
External occupation (Siegel prestige)13***	.40*153***	.085*	40.24 12.44
Federal occupation (Siegel prestige)...	1.61***290***	46.93 10.48
Federal salary (1977\$, 100s).....	146.96 58.01
Constant.....	-14.67	26.38*	-164.03***

SOURCE.—ICPSR study no. 7277.

^a Unstandardized and standardized coefficients from ordinary least-squares regression with all relevant independent variables controlled. Significance levels reported as in table 2. Sample is not strictly comparable to that analyzed in table 2, and there are also minor differences in the measurement of variables; see text n. 7. $N = 441$.

^b As of 1967, except in equations predicting external occupation where age is taken as of the most recent entry into the federal service (mean age = 27.89, SD = 7.11; square of age in 100s, mean = 8.28, SD = 4.36).

TABLE 5 (Continued)

INDEPENDENT VARIABLE AND UNITS	COEFFICIENTS AND DEPENDENT VARIABLE				
	Metric		Standardized		
	External Occupation	Federal Occupation	Federal Salary (\$100)	External Occupation	Federal Occupation
<i>R</i> ² values: ^a					
Eq. (1).....	.203	.253	.326		
Eq. (2).....270	.474		
Eq. (3).....286	.487		
Eq. (4).....	.223	.342	.530		
Eq. (5).....	.226	.340	.534		
Full eq.....	.226 ^d	.369	.600		

^a All *R*² values have been adjusted for degrees of freedom. Eq. (1) includes as predictors for each dependent variable only education, age, and the square of age. Eq. (2) adds federal and nonfederal occupation as predictors. Eq. (3) further adds seniority, and eq. (4) includes all of the foregoing plus minority status and sex. Eq. (5) adds farm origin and father's occupation. The full equation includes all relevant independent variables shown in the table and is the equation for which the regression coefficients are reported.

^d The full equation is identical with eq. (5).

consistently contribute only a minuscule amount to the considerable explanatory power of the model.

Similar conclusions emerge when the most recent occupation held outside the federal government is the dependent variable, although here the direct effect of family background does appear somewhat greater than in predicting federal occupation or salary. Overall, the intergenerational status attainment of these federal employees looks much like that of employees in other samples not confined to a single BLM (see Alexander, Eckland, and Griffin 1975). As an independent variable, most recent external occupation has a small to moderate positive effect on both federal occupation and federal salary. The latter effect may reflect the higher external earnings of high-prestige occupations, along with a tendency to assign employees to pay grades roughly consistent with their external earning power.

DISCUSSION

Career Lines and Career Success

The results of this research clearly document the importance of entry ports and career lines as structural features of the federal labor market and at least suggest their importance for other BLMs as well. For example, the effects of employment in the Veterans Administration reflect the presence in that agency of a group of seemingly disparate career lines which have in common restricted opportunities for salary increases after entry. Similarly, the Postal Service displays a distinctive pattern of earnings over the career cycle. Or consider the earnings trajectory associated with a career that begins in the field but ends with an assignment in the national capital. Finally, although entering federal employment through a blue-collar port has little enduring effect on the careers of those who shift to white-collar work, both entry occupational prestige and starting salary are powerful predictors of subsequent achievement. In short, an emphasis on the career lines associated with different ports of entry, both occupational and organizational, is no small contribution of the first of the three theoretical viewpoints identified above, in the section headed "The Bureaucratic Career."

Still, the career-line perspective apparently errs in its assertion that "our implicit model of education and attainment . . . applies much less readily to the . . . civil service world . . . with [its] formal job structures and internal promotion systems" (Kaysen 1973, p. 149). Education, age, and occupation in the external labor market are by no means irrelevant to success within the BLM, and intergenerational influences on socioeconomic achievement in the federal service do not differ markedly from those observed in non-BLM samples. Nor do the effects of minority status and sex operate exclusively through differential access to advantageous entry ports and career lines. As just noted, starting salary and entry occupational prestige, the

blue-collar entry dichotomy, and even geographic location and agency are all associated in varying degrees with the characteristics making a given entry port more favorable or less so for subsequent career success. The race and sex differences in salary which remain after such variables are controlled almost certainly reflect more than mere differential access to career lines, and probably involve some direct discrimination in administrative decisions about employee remuneration (see Taylor 1979).

Bureaucratic Rationality and Socioeconomic Achievement

The observed effects of race and sex thus defy interpretation in terms of "purely functional points of consideration and qualities" (Weber 1946, p. 201), but the Weberian image of a BLM as a rational employment system otherwise fares rather well in this study. Personnel practices in the federal BLM seem especially consonant with the rationalistic assumptions fundamental to both the human capital and the status-attainment traditions (see Horan 1978). Status-attainment models estimated for non-BLM samples typically explain only a tenth to a third of the variance in individual income (see Jencks et al. 1972, 1979). The model used here is two to three times as powerful in explaining salary, even with education and age as the only independent variables.

Cohorts and the Organizational Career

What distinguishes the present model and helps account for its greater predictive power is its explicit treatment of variations in the structure of opportunity affecting individual careers (for a different treatment, see White 1970). *Organizational* sources of variation in opportunity are controlled in the model by limiting the analysis to substantially continuous careers within a single bureaucratic labor market and then by introducing variables which further specify an employee's location within that BLM. *Historical* differences in opportunity are controlled by analyzing separately cohorts of employees whose chances of success have been "variously affected by prosperity and depression, by peace and war" (Jaffe and Carleton 1954, p. 3). The considerable predictive power of the resulting model suggests that much of the variance usually attributed to "noise," to "error," or to "luck" in status-attainment models can be traced ultimately to organizational and historical sources of differential opportunity. Perhaps this should come as no surprise; "being in the right place at the right time" is not a bad definition of luck.

Viewed in this light, success in an organization is largely determined by the timing of career events. For example, the tremendous expansion and contraction of defense-related civilian employment associated with the

Vietnam War produced interpretable patterns of career outcomes. To a lesser extent, so did the growth and then the temporary stagnation of employment in the social service agencies. In both instances, employees hired by the affected agencies before the period of fastest growth enjoyed a ground-floor advantage over their counterparts hired during expansion. Similarly, the slow but steady growth which has characterized most of the smaller federal agencies during the past two decades has apparently benefited the career prospects of their employees. However, the hypothesis that organizational growth creates opportunities for career achievement, while useful, is incomplete. Opportunities in the VA seem not to have been greatly affected by that agency's recent substantial growth, and the dominant historical influence on the Postal Service appears to have been the postreorganization contract settlement, not growth or decline. Unsupplemented, the growth hypothesis oversimplifies the complexity of history.

CONCLUSION

Depending on how one counts, from a quarter (Stinchcombe 1965) to half (Caplow 1954) to three-quarters (Doeringer and Piore 1971) of employed Americans work within BLMs. Yet most research on socioeconomic achievement has ignored the organizational context of the career. The present study demonstrates that such research has thereby neglected the very social structural variables through which the impact of societal history is transmitted to the lives of individuals.

Attention to historical and organizational influences on career success can improve our models of status attainment. Conversely, the sociological understanding of historical events and of formal organizations can be improved by considering their effects on the careers of individuals (see Burstein 1979; Reed 1978). In Mills's (1959, p. 6) words, "no social study that does not come back to the problems of biography, of history, and of their intersections within a society has completed its intellectual journey."

APPENDIX

VARIABLE MEANS AND STANDARD DEVIATIONS FOR FIVE COHORTS OF CAREER FEDERAL CIVIL SERVANTS*

VARIABLE AND UNITS	COHORT AND DATE OF VARIABLE									
	Long-Term (pre-1963)		Great Society (1963-65)		Vietnam (1966-68)		Nixonomics (1969-71)		Watergate (1972-73)	
	1963	1970	1977	Entry	1970	1977	Entry	1977	Entry	1977
Education (years).....	14.01 [2.36]	13.83 [2.26]	14.14 [2.36]	...	13.80 [2.21]	...
Entry age (years).....	35.11† [7.33]	31.80 [9.15]	29.27 [9.27]	...	27.45 [8.37]	...
Entry age squared (years squared, 100s).....	12.86† [5.13]	10.95 [5.98]	9.42 [6.04]	...	8.23 [5.33]	...
Seniority (months).....	108.08 [73.81]	188.46 [73.29]	265.36 [73.58]	0 [0]	43.43 [10.98]	120.14 [12.18]	0 [0]	81.34 [10.26]	48.11 [7.11]	...
Minority status (1,0).....	.141 [.348]162 [.369]216 [.412]221 [.415]	...
Female (1,0).....	.271 [.444]333 [.472]430 [.495]460 [.499]	...
Blue-collar entry (1,0).....	.063† [.243]074 [.262]046 [.211]030 [.170]	...
Disabled veteran (1,0).....	.082 [.275]071 [.257]043 [.201]032 [.177]	...
Other veterans' preference (1,0).....	.527 [.499]403 [.491]338 [.474]318 [.466]	...
Defense and related agencies (1,0).....	.402 [.490]	.368 [.482]	.354 [.478]	.269 [.444]	.281 [.450]	.379 [.485]	.271 [.445]	.253 [.435]	.260 [.439]	.228 [.420]

* Data are for federal civil servants who were full-time, white collar on June 30, 1977, and entered during one of the periods shown. Source, variable definitions, and further sample restrictions are discussed in the text and in n. 5. Means are reported for each variable as of the date in the column heading, with standard deviations in brackets. For variables which are assumed constant over time, statistics are reported only once.

† As of 1963; entry data unavailable.

APPENDIX (Continued)

COHORTS AND DATE OF VARIABLE

VARIABLE AND UNITS	Long-Term (pre-1963)			Great Society (1963-65)			Vietnam (1966-68)			Nixonomics (1969-71)			Watergate (1972-73)		
	1963	1970	1977	Entry	1970	1977	Entry	1970	1977	Entry	1977	Entry	1977	Entry	1977
Postal Service (1,0).....	.159 [.366]	.160 [.367]	.160 [.367]	.360 [.480]	.348 [.477]	.349 [.477]	.286 [.452]	.286 [.452]	.290 [.454]	.211 [.408]	.210 [.407]	.237 [.426]	.232 [.422]		
VA (1,0).....	.074 [.262]	.077 [.266]	.077 [.267]	.054 [.226]	.050 [.219]	.053 [.224]	.053 [.224]	.055 [.228]	.062 [.241]	.094 [.293]	.102 [.303]	.102 [.303]	.107 [.309]		
HEW and related agencies (1,0).....	.054 [.226]	.072 [.258]	.071 [.258]	.071 [.257]	.082 [.275]	.089 [.284]	.080 [.272]	.082 [.275]	.085 [.279]	.096 [.295]	.109 [.312]	.121 [.327]	.135 [.342]		
D.C. (1,0).....	.109 [.312]	.098 [.297]	.129 [.335]	.114 [.318]	.090 [.286]	.127 [.333]	.123 [.329]	.097 [.296]	.119 [.324]	.120 [.325]	.142 [.350]	.169 [.375]	.158 [.365]		
Occupation (Siegel prestige)...	47.93 [10.78]	48.95 [10.52]	49.86 [10.15]	45.36 [10.54]	47.02 [10.41]	48.55 [9.97]	44.68 [10.35]	45.49 [10.14]	47.07 [9.85]	46.38 [11.06]	48.32 [10.67]	44.59 [9.43]	46.13 [9.55]		
Salary (1977\$, 100s).....	132.06 [46.27]	163.97 [62.65]	204.29 [76.57]	101.89 [36.38]	131.93 [53.22]	176.54 [63.78]	101.20 [37.97]	120.06 [49.64]	162.55 [60.65]	103.53 [42.17]	156.60 [60.59]	97.44 [36.76]	130.86 [48.38]		
N.....	2,214	814	1,023	...	668	...	742		

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Selection by Certification: A Neglected Variable in Stratification Research

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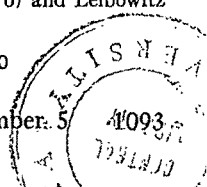
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Recent studies of status attainment have relied heavily on the general linear model. In the relationship between status and education, important departures from linearity may occur as a result of certification screening (or preference) by employers; yet many researchers have virtually ignored this process and the possibility that it may serve several functions. Data from the 1977 NORC General Social Survey show that academic certification has a substantial impact on occupational prestige and (more ambiguously) on respondent's income, independently of years of school completed and other predictors; this impact appears to vary according to age of respondent. In conclusion, the status-attainment process in the United States may depart substantially from the "rational choice" model favored by human capital theory.

Although the status-attainment literature has been sharply criticized for its limited conception of occupational structure (Bibb and Form 1977; Horan 1978; Beck, Horan, and Tolbert 1978) and for its neglect of social classes either too rich or too poor to be included in studies of the public schools or the civilian labor force (Beeghley 1978, pp. 308 ff.; cf. Lewis and Wanner 1979), it is nevertheless one of the more highly developed research traditions in the social sciences, involving large masses of data, sophisticated analysis, careful theorizing, and a serious concern with continuity through replication of earlier findings. Nearly all research in this area has used the "general linear model," specifically, multiple regression analysis. The resultant theoretical models explain variance in status attainment (e.g., years of school completed, occupational prestige, or income) with reference to multiple causes involving social background, ability, motivation, early school or occupational experiences, and so forth.¹

Considering that our ability to account for variance in occupational prestige has not been impressive, and that our ability to account for variance in income has been singularly unimpressive, it is strange indeed that educational certification as a discrete event, as a significant rite of passage,

¹ Detailed, albeit somewhat dated, summaries are found in Blaug (1976) and Leibowitz (1977).



has been largely ignored by scholars.² A strong emphasis on certification screening, creating a bias in the status system in favor of those with academic degrees, would introduce measurable departures from linearity in the relationship between years of school completed and measures of occupational status or income. Yet, paradoxically, few scholars have sought to identify such nonlinearities. In a study destined to become a classic, Blau and Duncan (1967) raised the issue of nonlinearity in the relationship between occupational prestige and educational attainment and concluded that there was "no occasion to use such models" in their own study (Blau and Duncan 1967, p. 140). However, when one examines the modified scattergram depicting the prestige-education relationship in their data (Blau and Duncan 1967, p. 144), one cannot help noticing that substantial positive residuals in prestige exist for the small proportion of the sample whose educational attainment includes "college graduation" or "five or more years of college." Similarly, Jencks et al. (1972, pp. 180 ff.) noted the importance of certification screening and yet made little effort to assess the possible impact of such a selection process in creating departures from linearity in the relationship between schooling and occupational prestige. Instead, they relied on Blau and Duncan's possibly premature skepticism and on a demonstration that equations containing nonlinearities typically add only about 5% to the variance in occupational prestige explained by schooling (Jencks et al. 1972, p. 160; Jencks 1973, p. 458). Since this additional 5%, however, "reflects the fact that a year of college has appreciably more effect on occupational status than a year of elementary or secondary school" (Jencks 1973, p. 458), and since the college educated comprise only about 14% of the adult population (NORC 1977, p. 32), it is arguable that this particular nonlinearity may be of substantial importance within that small segment of the population where it occurs. In a 1979 volume, Jencks and his colleagues try to correct these deficiencies by assuming bachelor's-level certification for those with four or more years of higher education (Jencks et al. 1979, p. 161); data on actual degrees are minimal in the surveys used by their investigation. The Occupational Changes in a Generation (OCG) surveys made no attempt to generate data on academic degrees.

A few studies have shown a tendentious disregard for the "credentialism" variable (e.g., Featherman and Hauser 1976, p. 481; 1978, pp. 264-65), arguing erroneously that reductions in the strength of the linear relationship between social origins and status discredit the "darkest suspicions" of those who accuse employers of an overemphasis on certification screening. (My own suspicions are no longer dark; over the years they have become

² For an exception see Featherman and Carter (1976, p. 155), who report that "for men of equal schooling and attendance duration, the one who holds a college degree . . . takes a higher-status first job." See also Taubman (1975), Goodman (1979), and Jencks et al. (1979); in the latter, four years of college is considered tantamount to college graduation.

hoary with age.) When investigators praise the virtues of an allegedly fluid status system with its weak linear relationships (Blau and Duncan 1967, pp. 432 ff.) while failing to assess nonlinearities, they may obscure a highly significant practice of credentialism. In instances where schooling has been assessed for its impact on status among females and racial minorities, as in a study by Treiman and Terrell (1975, pp. 182–84), it is most unfortunate that the linear model has been relied on throughout the analysis, and that the common lament of minority leaders about the “credentials game” has not been addressed at all. The pitfalls of such procedures have been well documented by Berg (1970, p. 26):

Education is often presumed . . . to be a continuous variable; approximately the same marginal differences in the economic values are assumed to exist between, say, any two successive years of high school. And researchers assume this despite the recurrent finding that diplomas and degrees command a price in the labor market that goes well beyond the marginal increment of learning that may be achieved between the third and fourth year of high school or college. An extreme example: A number of law schools have recently begun awarding a doctorate to their graduates in place of the traditional Bachelor of Laws degree. As a result, these graduates *automatically* start at higher civil-service classifications if they go to work for the federal government, even though their preparation has not changed.

The practice of certification selection is presumed to have various goals, functions, or consequences. One of the imperatives of any society is that of simplifying and legitimating social selection processes (Schelling 1963, pp. 54 ff., 76–77), and certification screening would appear to serve this goal effectively, “to regulate the allocation of young men and women to extremely desirable jobs carrying high salaries and status” (Wolff 1969, p. 108; Sharp 1970, table 4.3). Indeed, if it is true that “education is not a college’s primary function” and that “the crucial *raison d’être* of the American college, the *sine qua non* of its survival and current importance, may not be education but certification” (Jencks and Riesman 1968, p. 61; cf. Jencks et al. 1972, pp. 183–84; and Kamens 1977), it would not be at all surprising if there existed, as Berg (1970) has alleged, only minimal relationships (if any) between certification and occupational performance. If strong relationships do exist, however, certification selection may be an inexpensive means of maximizing worker productivity. It is possible, of course, that occupational performance in a narrow sense may not be uppermost in the minds of employers, who may seek out job applicants who have the “interpersonal skills” requisite for getting along with co-workers, who have stability, poise, self-assurance, who feel strong loyalty to the employer, whose “stick-to-it-iveness” overcomes the boredom of dull, routine work, and so forth (Berg 1970, p. 75). A number of economists, either

working within the framework of "human capital" theory or attempting to challenge it (or both), have developed mathematical approaches (Arrow 1973; Spence 1973) designed to separate the "social" from the "private" utilities of certification screening and in several instances have made empirical tests—with mixed results—of the screening hypothesis as it applies to individual income (Layard and Psacharopoulos 1974; Mincer 1974; Taubman 1975). Finally, another possible effect of certification screening is to maintain a closed door vis-à-vis racial and cultural minorities (Collins 1979) or, more generally, to contribute to the processes whereby an existing stratification system tends to "replicate" itself (Bowles and Gintis 1976).

If such functional necessities do exist in societies like the United States, one would expect that academic certification would have a significant impact on various indexes of prestige such as occupation or income. In the words of Jencks et al. (1979, p. 164), however, significant regression coefficients for academic certification would be "only a suggestive indicator" of the possible social impact of credentialism. Negative results, of course, would eliminate all variations of the credentialism hypothesis as outlined above; this critical first test is the major objective of the present study.

If the existence of selection by certification were demonstrated, the next task would be to derive necessary or probable consequences—conflicting consequences, one hopes—from each of the several alternative formulations of the certification hypothesis. If, for instance, the "social utility" hypothesis of efficient social selection were an accurate description of reality, we would expect that, first, employers would consistently have an eye toward minimizing costs of personnel selection; second, they would be far more wary of hiring poorly qualified individuals than of failing to hire well-qualified individuals (thereby emphasizing social over private utilities) and, in pursuit of this objective, they would continually upgrade credential requirements; third, they would maintain close surveillance over the ability of low-cost hiring programs to maintain high levels of worker productivity; and, finally, they would make a continuous effort to "legitimate" efficient, low-cost personnel selection procedures based largely on academic credentials (Kamens 1977). On the other hand, those who regard credentialism as irrational or "noneconomic" behavior on the part of employers (Albrecht 1978, p. 10) would be expected to demonstrate that employers pay little attention to the efficiency of their personnel selection or promotion (Rosenbaum 1979) procedures; that minimal relationships, if any, exist between certification and occupational performance; that vocational education is "virtually irrelevant to job fate" (Collins 1979, p. 16); that "schools are very inefficient places of learning" (Collins 1979, p. 17); that academic performance is a weak predictor of occupational productivity (Hoyt 1965); and that college dropouts are not necessarily of lesser ability than those

who complete their schooling (Watts and Whittaker 1968). Those who champion the "market signaling" variation of the credentialism hypothesis, claiming that "innately more productive workers are better able to attend and graduate from . . . college" and that therefore "college can serve as a means by which the inherently qualified identify themselves to prospective employers" (Albrecht 1978, p. 2), would have to demonstrate that, while school effects are small, employer rationality is high. Finally, the "closed-door" hypothesis would require a demonstration of minimal school effects and high employer irrationality tending to victimize racial minorities, cultural minorities, and females.

DATA AND METHODS

Data for this study were drawn from the 1977 General Social Survey of the National Opinion Research Center. The 1,530 respondents in this survey were a representative sample of English-speaking, noninstitutional adults living in the United States. A detailed account of the sample design, interviewer specifications, wording of questionnaire items, and coding instructions has been prepared by NORC (1977). The following questionnaire items (and codings) have been used at various stages of this analysis:

Question 4: Prestige of Father's Occupation

Question 14: Prestige of Respondent's Occupation

For each occupational prestige variable, scores were taken from the rating system developed at NORC in 1963-65; they range from 12 to 82 (NORC 1977, pp. 149-52). It should be noted that the General Social Survey does not attempt to ascertain occupational careers, that is, sequences of occupational positions through which individuals move. Data for this study pertain to current or past forms of work "normally" done by respondents and their fathers (NORC 1977, pp. 12, 22).

Question 25: Respondent's Education

Question 26: Respondent's Father's Education

Scores for education refer to years of school completed. Years of primary/secondary schooling were ascertained separately from years of college; thus use of a "spline" variable in which each respondent is assigned a score for primary/secondary schooling ranging from 0 to 12 and a score for years of college ranging from 0 to 8 or more was facilitated (NORC 1977, pp. 31-32, Appendix D).

Question 25: Respondent's Highest Earned Degree

Coded into three dummy variables representing high school graduation, four-year college graduation, or possession of a graduate degree. The "reference category" consists of respondents who claim no degree.

Question 26: Respondent's Father's Highest Earned Degree

Coded into two dummy variables representing high school graduation, or graduation at the bachelor's level or higher. The "reference category" consists of respondents whose fathers held no degree.

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Question 43: Respondent's Income

National Opinion Research Center income data are coded from 1 to 16 according to gross earnings for 1976. Income categories range from "under \$1,000" to "\$50,000 or over." For this study the midpoints of the earnings categories were used, with the extreme categories coded \$1,000 and \$50,000, respectively. In part of the analysis, natural logarithmic values of income were used.

Question 114: Respondent's Age

Coded 18 years old through 80 years old or over.

Data were analyzed in three phases: first, analysis of variance was employed to determine whether the relationship between schooling and occupational prestige or income involves significant departures from linearity and, if so, how much additional variance could potentially be explained by permitting nonlinearities; second, multiple regression analysis was undertaken to determine the net effects of degree certification on occupational prestige and income, after years of school completed and other background factors were controlled; third, the age variable was introduced into the analysis in order to obtain a rough indication of whether factors influencing status attainments interact with age and/or cohort membership.

FINDINGS

Among the 1,530 adults comprising the NORC file, a subsample of 950 was selected on the basis of their having been in the labor force in 1977 or earlier, having responded to the questionnaire item on their educational attainment, and having had an income in 1976 that they were willing to divulge to the interviewer. Among the original 1,530 respondents, 114 were eliminated because the occupational prestige item was either not applicable or (in one case) not answered. Among those to whom prestige scores were assigned, 10 failed to answer the question on education, and, among the 1,406 remaining cases, 456 reported no income for 1976. Slightly different patterns of subsample attrition occurred among the 837 females and 693 males who comprised the original NORC file. Of the 174 males eliminated, 158 (90.8%) failed to qualify solely by virtue of a lack of reported income for 1976; 519 males remain in the subsample. Of the 564 females eliminated, 101 (17.9%) reported neither an income for 1976 nor past participation in the labor force, while another 298 (52.8%) failed to qualify solely because of a lack of reported 1976 income; 431 females remain in the subsample. It is perhaps noteworthy that, in recent years, at least two major inquiries into the status-attainment process have ignored females altogether (Featherman and Hauser 1978, pp. 6 ff.; Jencks et al. 1979, p. 5).

Jencks et al. (1979, p. 28) use the correlation ratio (η^2) as a rough index of the extent to which a bivariate distribution—for example, that between

years of schooling and occupational prestige—departs from linearity. The correlation ratio, η^2 , is equal to the coefficient of determination we would obtain if the independent variable were broken down into a series of dummy variables; for 20 years-of-schooling categories, this procedure would entail 19 dummy variables. Since virtually no relationship is perfectly linear, η^2 is nearly always larger than the corresponding value of r^2 . In the preliminary phases of this study, an analysis of variance (identical with the dummy-variable procedure outlined above) was undertaken to ascertain whether schooling is associated with prestige or income in a way that involves significant departures from linearity. As a linear variable, the 20 years-of-schooling categories account for 32.5% of the variance in occupational prestige. The correlation ratio, allowing nonlinearities, adds about 6% ($\eta^2 = .389$) to explained variance, a result comparable to that reported by Jencks et al. (1972, p. 160); this departure from linearity is significant at the .01 level. Similar results were obtained for dollar income ($\eta^2 - r^2 = .102 - .062 = .04$); for log income, however, departures from linearity were nonsignificant.

Table 1 presents means, standard deviations, and a listing of missing cases for all variables used in this study. Comparing the NORC sample with the 1973 OCG sample, we note a significant disparity: although the Duncan SEI (socioeconomic index) is strongly correlated ($r = .86$) with the NORC occupational prestige scale, Featherman and Hauser (1978, p. 253), using the Duncan scale, report differences between fathers' and sons' occupational prestige scores ranging as high as 14.73 points, whereas the NORC data show no such differences. While the absence of females from the OCG sample has to be taken into account, this disparity could be due in part to the fact that while the NORC questionnaire asks about work "normally" performed by respondents' fathers while respondents "were growing up," the OCG survey asks about father's occupation when respondents were "about 16 years old" (Featherman and Hauser 1978, p. 502). This phraseology certainly raises doubts about the Featherman-Hauser measure of intergenerational occupational mobility. A comparison of these questionnaire items corroborates the thesis of Jencks et al. (1979, pp. 251-89) that large-scale surveys tend to produce disparate results because of differences in "research style," even though scholars may make a fairly concerted effort to use similar methods.

Additional comparisons are possible between the 1973 OCG survey and the 1977 NORC survey. While the 950 respondents included in the present study reported a mean 1976 income of \$10,704, the OCG mean income for 1972 was \$11,003 (Featherman and Hauser 1978, p. 291) for all ages 25-64; this seemingly conflicting result may be due largely to the inclusion of females in the present study. Both OCG and NORC data suggest that completion of 12 years of primary/secondary schooling is becoming uni-

TABLE 1
VARIABLES, DISTRIBUTIONS, AND MISSING CASES

VARIABLE	MEANS			STANDARD DEVIATIONS			MISSING CASES		
	All Ages	Age < 40	Age ≥ 40	All Ages	Age < 40	Age ≥ 40	All Ages (N = 950)	Age < 40 (N = 490)	Age ≥ 40 (N = 457)
Occupational prestige.....	39.47	39.20	39.80	14.40	14.87	13.86	0	0	0
Income.....	10,704.47	8,981.63	12,515.32	9,064.13	6,983.56	10,579.38	0	0	0
Log income.....	8.93	8.78	9.09	.90	.88	.90	0	0	0
Years of P/S schooling.....	11.12	11.60	10.61	1.77	1.10	2.17	0	0	0
Years of college.....	1.18	1.35	.99	1.95	1.98	1.90	0	0	0
High school graduate.....	.56	.63	.48	.50	.48	.50	27	20	7
College graduate.....	.10	.11	.10	.31	.32	.29	27	20	7
Graduate degree.....	.07	.07	.06	.25	.26	.24	27	20	7
Father's occupational prestige.....	39.47	40.33	38.54	12.56	13.35	11.63	122	63	58
Father's years of schooling.....	9.55	10.77	8.07	4.13	3.84	4.00	238	98	138
Father high school graduate.....	.27	.39	.14	.45	.49	.35	196	92	102
Father college graduate.....	.08	.11	.05	.28	.32	.22	196	92	102

versal in American society and that the variability of primary/secondary schooling has grown smaller within successive cohorts (Featherman and Häuser 1978, table 5.10); no such trends are discernible for college attendance. As shown in table 1, among respondents under 40 years old the standard deviation of primary/secondary schooling is 1.10 years, while it is 2.17 years among those aged 40 or above; standard deviations for years of college, in contrast, are nearly equal. The NORC data on academic degrees, however, raise doubts about Featherman and Hauser's inference, based on the above findings, that "acquisition of the high school diploma is an ever more prevalent norm" (Featherman and Hauser 1978, p. 244). Even on the dubious premise that social norms may be ascertained on the basis of statistical uniformities and without regard to socialization and social control, the data vitiate the claim that high school graduation is normative in contemporary American society: fully 26.3% of the 950 respondents included in this study held no diploma at all.

Substantial numbers of missing cases, listed in the last three columns of table 1, appear for those variables pertaining to fathers' work and schooling. Most instances of missing data arose either because respondents were not brought up by their fathers or because they did not have knowledge of their fathers' work or school achievements. Correlation matrices for the subsequent regression analyses were calculated through pairwise deletion of missing cases, with tests of significance allowing, conservatively, 400 degrees of freedom in denominators.

Table 2 presents occupational prestige, income, and log income as functions of schooling, certification, and several family-background factors. (An attempt to assess interactions between schooling and certification was abandoned because of multicollinearity.) The correlations between primary/secondary schooling and high school graduation (.49) and between college schooling and bachelor's- or graduate-level certification (.55 and .66, respectively) indicate that the schooling and certification variables are conceptually and statistically distinct, and that there is substantial danger in using four years of college as a proxy for graduation (as in Jencks et al. 1979). Respondents holding the bachelor's degree as the highest degree have had anywhere from 14 to 20+ years of schooling. In addition, academic certification in Western societies is typically celebrated by elaborate ceremonies amounting to a rite of passage that rivals baptisms, bar mitzvahs, initiations, engagements, weddings, retirements, and funerals in its social poignancy. Academic certification is what Durkheim would call a social fact, what Schelling (1963) would call a simple, binary, readily understood basis of social decision making. Ultimately, of course, the standing of academic certification as a social fact will depend on our assessments of its consequences for other social facts, such as job placement, income allocation, assortative mating, and so forth.

TABLE 2

REGRESSION ANALYSIS OF OCCUPATIONAL PRESTIGE, INCOME, AND LOG INCOME BY SCHOOLING, CERTIFICATION, AND BACKGROUND CHARACTERISTICS

DEPENDENT VARIABLE AND INDEPENDENT VARIABLE	b			β			F		
	All Ages	Age < 40	Age \geq 40	All Ages	Age < 40	Age \geq 40	All Ages	Age < 40	Age \geq 40
Occupational prestige:									
Years of P/S schooling.....	.53	.07	.97	.06	.01	.15	1.88	.01	4.46*
Years of college.....	2.16	1.77	2.98	.29	.24	.41	15.32*	6.15*	12.54*
High school graduate.....	4.97	5.34	4.95	.17	.17	.18	11.19*	5.97*	5.93*
College graduate.....	11.98	17.64	3.28	.25	.38	.07	16.81*	20.36*	.57
Graduate degree.....	17.72	23.11	8.20	.31	.40	.14	22.50*	21.55*	2.16
Father's occupational prestige.....	.07	.01	.09	.06	.01	.08	2.72	.07	1.99
Father's years of schooling.....	-.20	.09	-.27	-.06	.02	-.08	1.03	.09	1.03
Father high school graduate.....	.82	.54	2.35	.03	.02	.06	.33	.08	1.00
Father college graduate.....	-.80	-3.17	3.99	-.02	-.07	.06	.09	.83	.85
Constant.....	24.84	27.55	21.40
R ²40	.43	.42
Income:									
Occupational prestige.....	173.79	135.43	203.94	.28	.29	.27	36.60*	21.51*	15.80*
Years of P/S schooling.....	111.42	142.91	209.42	.02	.02	.04	.15	.11	.50
Years of college.....	360.39	178.90	669.44	.08	.05	.12	.75	.33	2.40
High school graduate.....	268.01	254.6001	.0206	.06	...
College graduate.....	2,250.27	...	4,630.10	.0813	1.03	...	4.08*
Graduate degree.....	1,829.71	2,916.3805	.1141	2.75	...
Father's occupational prestige.....	6.67	-11.91	-22.46	.01	-.02	-.02	.04	.14	.15
Father's years of schooling.....	-164.99	66.70	-48.36	-.08	.04	-.02	1.19	.14	.06
Father high school graduate.....	-1,326.88	-732.39	-1,215.73	-.07	-.05	-.04	1.54	.47	.46
Father college graduate.....	-640.83	-1,306.82	...	-.02	-.0610	.43	...
Constant.....	3,398.97	1,597.43	2,504.10
R ²15	.15	.18

TABLE 2 (Continued)

DEPENDENT VARIABLE AND INDEPENDENT VARIABLE	b			β			F		
	All Ages	Age < 40	Age \geq 40	All Ages	Age < 40	Age \geq 40	All Ages	Age < 40	Age \geq 40
Log income:									
Occupational prestige.....	.02	.02	.02	.34	.32	.32	55.54*	25.70*	22.56*
Years of P/S schooling.....	.02	-.02	.04	.04	-.02	.10	.47	.10	2.40
Years of college.....	.03	.01	.05	.06	.03	.11	.44	.05	.66
High school graduate.....	.04	.11	.15	.02	.06	.05	.12	.50 ^a	
College graduate.....	.11	.11	.11	.04	.04	.26	.12	.14	.27
Graduate degree.....	.07	.33	-.24	.02	.10	.07	.06	.80	.38
Father's occupational prestige.....	^a	-.002	-.002	.02	-.03	-.03	.30	.30	.25
Father's years of schooling.....	-.02	-.007	.01	-.07	-.03	.05	1.09	.10	.32
Father high school graduate.....	-.14	-.03	-.18	-.07	-.02	-.07	1.70	.05	1.02
Father college graduate.....	-.17	-.15	-.11	-.05	-.05	-.03	.76	.36	.12
Constant.....	8.01	8.30	7.83
R ²16	.14	.19

^a Tolerance level insufficient for computation (SPSS default).* $p < .05$, $df = 1, 400$.

An examination of metric regression coefficients in the upper segment of table 2 shows that, for the entire subsample, the net impact of college schooling on occupational prestige is substantially larger than that of primary/secondary schooling; the latter, in fact, is nonsignificant among respondents less than 40 years old, a result that parallels findings reported by Jencks et al. (1979, p. 189). While all three levels of certification have a significant impact on occupational prestige, bachelor's- and graduate-level degrees have an impact that is approximately equal to a full standard deviation on the prestige scale: bachelor's degrees bring about a 12-point advantage in occupational prestige, while graduate degrees bring about an 18-point advantage. As a rough benchmark, 15 points would represent the difference between an architect and a personnel worker, a photographer and a bill collector, a stenographer and a shipping clerk, a tailor and a stevedore, a butcher and a janitor. A person whose occupational prestige score moved from about seven points below the mean to about seven points above it would thereby surpass about 35% of the employed population, ending up close to the seventieth percentile in occupational prestige. Such an improvement is tantamount to moving from hairdresser to insurance broker, from bus driver to dental laboratory technician, from shoe repairman to flight engineer; note that the second of each pair would ordinarily require some sort of certification. It is noteworthy that the standardized regression coefficients for certification at the bachelor's level or higher are about the same size as the corresponding coefficient for years of college completed.

Finally, none of the several paternal status attainments—father's occupational prestige and school achievement—has a significant direct impact on respondents' prestige scores, a finding that is congruent with the bulk of the status-attainment literature (Leibowitz 1977). The 1973 OCG survey provides further evidence of the small and perhaps declining impact of paternal status attainments (Featherman and Hauser 1978, table 5.14). Jencks et al. (1979), however, are not fully convinced that the effects of family background are mediated entirely by education: "background characteristics seem to exert appreciable effects on both occupational status and earnings even among men with the same test scores and education. The background characteristics that exert these effects are not primarily the 'standard' demographic measures of parental advantages. . . . The unmeasured background characteristics that affect economic success appear to be different in kind from the background characteristics that influence test scores and educational attainment." Negative findings, then, may be due to our failure to discover and assess the appropriate "unmeasured" factors.

The substantial difference between the two age groupings in the impact of bachelor's- and graduate-level certification reflects either a tendency for more recent cohorts to receive a relatively large prestige payoff for achiev-

ing certification, or a tendency for certification to have a smaller impact as individuals grow older. Jencks et al. (1972, p. 182), interpreting the impact of years of schooling on status, lean toward the latter explanation (cf. Blau and Duncan 1967, pp. 177-78). On the other hand, although the 1962 and 1973 OCG surveys generated no information on academic degrees, data on college effects strongly suggest that the impact of a year of college is larger among more recent cohorts (Featherman and Hauser 1978, p. 262 and table 5.14). (The suggestion that "the additional premium for college is consistent with standard economic interpretations that the college-educated are more productive or trainable than other workers" [Featherman and Hauser 1978, p. 261] is totally gratuitous.) A combination of both aging and cohort effects, of course, is entirely possible.

Within the complete subsample, the correlation of respondent's income with occupational prestige is .35; with primary/secondary schooling, .17; with years of college, .28; and with the three levels of certification, -.09, .19, and .19, respectively. Examining the middle segment of table 2, we note that, as is typically the case, only a small proportion of the variance in income is explainable. When occupational prestige, schooling, certification, and family background are used as predictors, $R^2 = .15$ and all levels of certification turn out to be nonsignificant. Once again, however, the effects of academic certification differ between old and young respondents: among the former, bachelor's-level certification has a significant positive impact on dollar income, amounting to a metric value that approaches \$5,000. This finding is paradoxical when compared with that reported above for occupational prestige, suggesting either that older *cohorts* were better able to translate certification into relatively high incomes, or that older *individuals* have eventually succeeded in translating certification into relatively high incomes.

The latter interpretation is supported by Taubman's panel study of a sample of World War II veterans (1975, pp. 32-34, 98-100), among whom the impact of certification on earnings apparently became stronger as they grew older, and by Featherman and Hauser's finding (1978, pp. 306-7) that "the *premium* for college (i.e., the return compared to that for graded schooling) tends to get larger as men mature." Incidentally, Taubman suggests that "nonpecuniary" values are often accepted in lieu of income, and such a trade-off may have occurred among many of the younger, certified respondents of the NORC survey.

If we can believe both Jencks et al. (1972) and Taubman, the aging of individuals with certification may be accompanied by declining prestige payoffs and increasing income payoffs;³ but this pattern may vary over

³ The low correlation between income and occupational prestige ($r = .35$) suggests that substantial discrepancies exist between these two measures of socioeconomic status. Hypotheses that emphasize income-status trade-offs at the time of entry into the labor

time for successive cohorts, perhaps in response to the business cycle. In any case, Taubman's critics argue that one of the implications of the "screening hypothesis" is that returns to investments in education (including certification) should decline with work experience as employers "come to have better information about their employees' real productivity" (Layard and Psacharopoulos 1974, p. 992) and begin to close the income gap between academically certified and noncertified employees. That sounds plausible enough, but there are several good reasons for doubting the argument. First, Spence (1973, p. 366) has suggested that since "the employer's beliefs may drive certain groups from the market and into another labor market," there may be "no experience forthcoming to the employer to cause him to alter his beliefs." Second, labeling theory as developed by sociologists strongly implies that employers may adhere to erroneous beliefs about the greater productivity of highly educated employees, despite vast amounts of contrary evidence. The latter implication, in fact, is more clearly derived from the "screening hypothesis" than is the Layard-Psacharopoulos implication regarding the learning capacity of employers.

Such arguments, however, may be purely academic, for when logarithmic values of income are used as the dependent variable, as in the lower segment of table 2, only occupational prestige emerges as a significant predictor. Since this result diverges from that obtained for nontransformed values of income, we must decide which of the resultant causal models is preferable. The law of parsimony dictates a preference for models involving fewer predictors. Yet there is little theoretical rationale for expecting income to have a constant percentage rate of increase for each step on the prestige scale, as a semilogarithmic transformation implies; on the contrary, the notion of "diminishing returns" seems more appropriate. (It may be that the usual justification for log transformations of income is the need to normalize income distributions.) The certification hypothesis at least has the virtue of involving a theory.

If we do accept what appears to be the more parsimonious model, we shall have to adopt the conclusion of Jencks et al. (1972) that education (including certification in our analysis) influences income primarily through its influence on occupational prestige (cf. Featherman and Hauser 1978, p. 298). Following causal paths from years of college and certification (bachelor's level) to log income, with occupational prestige as the intervening variable, an increase of a standard deviation in both independent variables would produce an increase of $(.29) (.34) + (.25) (.34) = .184$

force imply a low correlation between income and occupational prestige, and one that would remain stable as cohorts age. On the other hand, hypotheses suggesting an enhanced income payoff (e.g., for schooling) with increasing age, combined with declining occupational prestige payoffs, imply a pattern of diminishing income-prestige correlations as a cohort ages.

standard deviation units in log income. Starting with an income of \$10,000, such an increase would have a value of about \$1,800, or 18%, and this percentage would remain constant for all initial values of income. It should be noted that the standard deviation for bachelor's-level certification (as is true of all levels of certification) is fractional (.31), and is therefore not defined for individuals, who are either certified or noncertified at a given level. If an aggregate of noncertified individuals under 40 years old, starting with a mean income of \$15,000, were to increase their mean years of college by about two years, and if the proportion certified at the bachelor's level were to increase to .32, the mean income of the aggregate would be expected to increase by about 20%, from \$15,000 to \$18,000. Such "predictions," of course, are very imprecise.

CONCLUSIONS

Credentialism, which in its crassest form is tantamount to certification screening, appears to be a phenomenon of substantial impact on American status hierarchies. An essential task for future studies is to assess the generalizability of this finding over time, through space (e.g., cross-nationally), and across various segments of the population. In particular, far more detailed analyses of cohort membership versus age variations would be necessary to resolve the issue of a time trend, or lack thereof, in the severity of credentialism. A series of surveys at various times such as the OCG series, longitudinal surveys such as Project TALENT and the National Longitudinal Survey of the Department of Labor, and cross-sectional surveys in which respondents are asked to reconstruct their life-cycle patterns from memory—all such approaches are indispensable in our efforts to resolve the issue of a trend.

It is most unfortunate that, while investigators of status allocation in modern societies have attempted to describe such processes among females and among racial and ethnic minorities, they have rarely evaluated the persistent claim of minority spokespersons that certification selection is just another way in which culturally dominant strata maintain their position in society (Collins 1979), that it is essentially a form of discrimination. The highly consistent finding that females and blacks tend to receive less status payoff for any given level of early achievement (e.g., Blau and Duncan 1967, pp. 208–13; Featherman and Hauser 1976; Suter and Miller 1973; Treiman and Terrell 1975) suggests that certification among such minorities would also have a restricted payoff. On the other hand, it could turn out that those females and blacks who achieve certification are thereupon compensated for the limited payoff gleaned from years of schooling per se, so that we end up with a classic case of the "superwoman" or "superblack" syndrome. Jencks et al. (1979), for instance, provide some evidence that

blacks who achieve bachelor's-level certification are well compensated for their efforts.

A large part of the recent literature on the status-attainment process has been informed—or perhaps misinformed—by what has come to be known as the “human capital” perspective (Becker 1964). To many sociologists and most economists the human capital perspective has a strong allure: it is derived from a straightforward utilitarianism, from the “rational choice” or “economic man” model of human behavior, from the conviction that most social behavior, most of the time, has the properties of an elaborate game in which each player has a clear and complete understanding of his own options, the options available to others, and the “utilities” or consequences of each combination of “move” and “countermove.” The certainty of outcomes makes the game a little dull, but this game is not played for excitement. Human capital theorists think of each individual as investing in himself with a view toward maximizing access to valued social rewards such as safety, income, and deference. Conversely, society, itself presumed to behave rationally, seeks to establish a “meritocracy” in which each individual, assessing the relationship between his self-investments and the advantages thereby achieved, will have a strong sense of virtue rewarded and justice fulfilled.

Human capital theorists are gratified to discover instances in which society seems to be reducing utilities associated with ascriptive traits such as sex, race, ethnicity, or family background and to be increasing utilities associated with achievements, say, in the academic realm (Featherman and Hauser 1978). The human capital tradition assumes, along with Milton Friedman, that it is an easy task to distinguish between “equality of opportunity” and “equality of results”; never mind that what was an instance of opportunity and achievement for one's parents or grandparents is slowly transformed into an ascriptive status for oneself. In part, human capital theorists protect themselves against such realities by failing to study the rich and the super rich as a self-perpetuating class, by failing to inquire into such matters as net worth or political power as opposed to annual income or occupational prestige, and by failing to develop an adequate conception of family background. The critique of human capital theory, then, begins by accusing it of using the wrong variables.

The salient issues, however, extend well beyond matters of conceptualization. First, it has been argued that ascriptive traits such as family background are subject to serious measurement error of a random or nonrandom form, and that such error leads to erroneously low estimates of the effects of ascription on status attainments. Recent responses to this argument (e.g., Featherman and Hauser 1978, pp. 258–59; Jencks et al. 1979, pp. 34–37) are not at all reassuring, primarily because investigators have not yet made the transition from recognition of the problem to plausible solutions; an

ingenious attempt, however, is found in Hauser and Featherman (1977, pp. 271 ff.). Second, the strong prospect of "noneconomic" behavior on the part of employers (Albrecht 1978, p. 10) has received short shrift in the literature, and it is entirely possible that many aspects of employer behavior, such as the ways in which employers use certification selection, cannot be interpreted within the rational choice model of the behavior of the firm. Third, the human capital approach tends to ignore ways in which structural features such as the "dual economy" (Beck et al. 1978) serve to constrain individuals regardless of their personal efforts, that is, to change utilities in ways not clearly perceived by individuals. Finally, the human capital thesis tends to exaggerate the degree to which public opinion has arrived at a consensus about the "fairness" of a society's status-allocation processes (Faia 1980).

Human capital theory derives from the basic postulates of the rational choice model, and disputes about the theory invariably create a larger dispute about the model. Not surprisingly, other subdivisions of the social sciences encounter a similar range of issues. In penology, for instance, the recent resurgence of deterrence theory has been met head-on—once again—by the claim that potential felons do not invariably play rational games with society. Nor, I hasten to add, does society always play rational games with potential felons: witness our feverish efforts to combat "crime in the streets" while ignoring far costlier instances of "crime in the suites." When an economist such as Isaac Ehrlich (1975) speaks of the "murder supply function" and the prospects of suppressing it by threatening potential suppliers with increased execution rates, he is soon accused, correctly, of pushing the rational choice model beyond its reasonable limits (Baldus and Cole 1975; Bowers and Pierce 1975). Ehrlich has collaborated closely with Gary Becker (Becker and Landes 1974), staunch advocate of the human capital thesis and, earlier still, of the thesis that human fertility is readily explained as a rational choice process (Weeks 1978, pp. 91–92). Such a profession of faith can hardly be edifying to a social scientist trying to understand the behavior of unmarried teenagers who, although wishing to remain without children, believe that the withdrawal method of birth control cannot fail.

It has been said that, while economics is the study of choices people make, sociology is the study of why people do not really have any choices. If that is true, then human capital theorists need to learn more sociology.

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Power and Privilege in the Large Corporation: Corporate Control and Managerial Compensation¹

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The research presented here investigates the relative utility of a power theory versus a functional theory of organizational stratification as they pertain to managerial compensation in the large corporation. Concretely, it examines the effects of different types and levels of corporate control, adjusted for the effects of corporate size and performance, on three dimensions of compensation among 218 industrial corporations during 1975 and 1976. In order to assess the power of the chief executive officer in relation to other directors, the analysis employs a hierarchy of control configurations based on the distribution of stock ownership among the members of the board of directors. In general, the results confirm the hypothesis that the remuneration received by a chief executive officer is directly related to his power within the corporation. A major exception to this pattern involves chief executive officers who are also principal stockholders in their corporations and receive dividend income from their stock.

In recent years, sociologists have come to recognize the importance of power, as distinct from the related but narrower concept of formal authority, as a key concept in the study of organizations. This distinction has led to the realization that organizational power is often employed to augment and protect the privileges enjoyed by those who wield such power. Indeed, the reciprocal relationship between the distribution of power and the distribution of resources within organizations has led some theorists to view the organization as a "political economy" (Zald 1970). Other theorists have suggested explicitly that organizational elites sometimes use their power to reward themselves disproportionately for their contributions to an organization. Perrow, for one, observes that "organizations generate surpluses and leverage in our world, and those who have any power in them can use these for their own ends" (1972, p. 21). These observations are

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important because they serve to integrate recent theories of organization with parallel theories of social stratification.

One of the most promising substantive arenas for research on organizational stratification is the large corporation. Stratification theorists have speculated that corporate managers may "overcompensate" themselves for their activities because of their often unchallenged power within large firms. As Lenski notes, "The fantastically high salaries of managers in American industry can be explained only by their power position within the organization" (1966, p. 355). Although there have been many studies of managerial compensation, most of them have been conducted by economists concerned solely with the incentive structures and operative goals of large corporations. There have been only a few limited attempts to examine the relationship between managerial power and compensation. Moreover, none of these studies has attempted to examine explicitly the relative utility of power versus functional theories of stratification as they pertain to the large corporation. It is the intent of this study to present precisely such an analysis. Concretely, it examines the effects of different types of corporate control on managerial compensation, while controlling for the effects of corporate size and performance, among 218 large industrial corporations during the two-year period 1975-76.

THEORIES OF MANAGERIAL COMPENSATION

The issue of managerial compensation in the large corporation has attracted the attention of many social scientists. Most of the empirical research on this problem has been conducted by economists seeking to identify the incentive structures and, thereby, the operative goals of the business corporation. According to the managerial theory of the firm, which takes into account the putative separation of ownership and control in the large firm, the operative goals of the corporation involve both growth and profitability (Baumol 1959; Marris 1964). Consequently, managerial compensation is presumed to be closely linked both to the size and to the performance of a corporation. In point of fact, the managerial theory of the firm was developed as a result of the observation that "executive salaries appear to be far more closely correlated with the scale of operations of the firm than with its profitability" (Baumol 1959, p. 46). This theoretical formulation has led several researchers to attempt to determine the relative importance of size versus performance as predictors of the compensation of chief executive officers. Recent attempts to disentangle the relative effects of these variables on compensation have been complicated by the fact that sales and net income are so highly correlated that it is impossible to obtain exact estimates of their independent effects on compensation. Despite these methodological problems, the general conclusions of these studies are unequivocal.

cal: the compensation of a chief executive officer is directly related both to the size and to the performance of his corporation (McGuire, Chiu, and Elbing 1962; Lewellen and Huntsman 1970; Smyth, Boyes, and Peseau 1975; McEachern 1977).

Although the relationship between corporate size and performance and managerial compensation can be interpreted in terms of a functional theory of organizational stratification, such interpretations are problematic for two reasons. First, there is no clear theoretical rationale for the relationship between corporate size and compensation. Perhaps the most common explanation of this relationship is simply that managers of large corporations receive more compensation owing to the greater "responsibility" inherent in their positions (Broom and Cushing 1977). While it is true, of course, that large firms have more customers, more employees, and more stockholders than small firms, the use of the notion of responsibility as a basis for monetary compensation in organizations can be questioned on the grounds that "it entails an unexplained and unjustified acceptance of the status quo" (Offe 1976, p. 114). The problem with such an explanation is that it is virtually impossible to measure responsibility independently from hierarchical position within the organization. Second, there is no apparent rationale for the relationship between corporate profitability and compensation. The most obvious implication of such a relationship, of course, is that managers are being compensated for their contribution to performance. This explanation is suspect because the available research suggests that differences in profitability among firms are due primarily to differences in market environments rather than differences in management (Lieberson and O'Connor 1972). Even if the profitability of a firm is attributable, at least in part, to the efforts of its managers, it is extremely difficult to determine with any precision their incremental contribution (Offe 1976).

Despite these ambiguities, the explanations of managerial compensation in terms of the size and performance of the firm are still essentially functional explanations. Managers are rewarded presumably in relation to their contribution to the continued survival of the firm (Broom and Cushing 1977). Certainly the major weakness of these explanations, as with most strict functional explanations, is that they ignore the importance of power (Kemper 1976). In particular, functional explanations of managerial compensation ignore the fact that chief executive officers often possess the power to influence their own levels of compensation (Simpson 1956). Indeed, the observed relationships between corporate size and profitability and managerial compensation are entirely consistent with a power theory of organizational stratification. Specifically, power theory holds that larger firm size and greater profits merely provide larger surpluses within these organizations which managers can allocate, at least in part, to their own

compensation (Lenski 1966). Within the context of a power theory of organizational stratification, the notions of responsibility and performance merely provide plausible justifications, not valid explanations, of managerial compensation.

The importance of power within the corporation as an explanation of managerial compensation has not gone entirely unnoticed by social scientists. One of the first major studies of the relationship between corporate control and managerial compensation failed to find any consistent evidence that management-controlled firms have different patterns of compensation from family-controlled firms (Larner 1970). However, this study neglected to differentiate between family-controlled firms in which the chief executive officer was a principal stockholder and those in which he was not. A recent study which employed such a distinction found that managerial compensation was generally lower among management-controlled firms than among family-controlled firms, especially when the chief executive officer was not a member of the controlling family (McEachern 1977). However, the conclusions of even this most recent study are not definitive because the analysis involved a limited sample of firms and because it failed to differentiate between family-controlled firms in which the chief executive officer is the sole principal stockholder and those in which there are other principal stockholders among the members of the board of directors.

Aside from studies of corporate control focusing primarily on the presence or absence of principal stockholders, there have been only a few analyses of the distribution of power within the large corporation. These studies suggest that, although the chief executive officer is almost invariably the most powerful individual within any corporation, other directors can exercise considerable power, particularly if they represent or control a significant block of stock (Gordon 1945). In general, the power of any director, including the chief executive officer, depends largely on the extent to which that individual possesses valuable expertise or controls critical resources (Zald 1969). In the business corporation, internal directors usually possess valuable expertise about the operation of the organization, whereas external directors often control resources critical to the organization. Certainly one of the most important sources of power for any director is the control of a significant block of stock in the corporation. Other things being equal, a chief executive officer is more powerful whenever he is a principal stockholder and less powerful whenever there are principal stockholders among the other directors.

These theoretical observations suggest a hierarchy of control configurations based on the distribution of stock ownership among the members of the board of directors, which either reinforce or restrict the inherent power of the chief executive officer. First, a chief executive officer is clearly most powerful if he is the only director who controls a significant block of stock.

Second, a chief executive officer is probably only slightly less powerful if none of the directors controls such a block of stock. In both of these situations, there are no principal stockholders among the other directors capable of restricting the power of the chief executive officer. Third, a chief executive officer is markedly less powerful, even if he controls a significant block of stock, if there are other directors who control comparable blocks of stock. Fourth, a chief executive officer is least powerful if he does not control a significant block of stock but there are other directors who do control such blocks. In these two situations, there are principal stockholders among the other directors capable of restricting the power of the chief executive officer.

The central hypothesis of this research, simply stated, is that the compensation received by a chief executive officer is directly related to his power within the corporation in relation to the power of other directors. Moreover, if this explanation of compensation in terms of power theory is to surpass the explanation in terms of functional theory, the relationship must obtain even after the effects of corporate size and performance on compensation are controlled. In other words, it is anticipated that chief executive officers who are also principal stockholders tend to be overcompensated, other things being equal, whereas chief executive officers accountable to directors who are principal stockholders tend to be undercompensated. The magnitude of such a relationship is contingent, of course, on the specific type of managerial compensation under consideration and the proportion of stock ownership necessary to identify any director as a principal stockholder.

SAMPLE AND MEASURES

In order to examine the postulated relationship between corporate control and managerial compensation in the large corporation, an initial sample was constructed which contained the 250 largest industrial corporations as ranked by either assets or sales in 1975. It must be noted, at the outset, that this sample does not include public utilities, transportation companies, retailing chains, commercial banks, insurance companies, or financial services firms. These types of corporations were excluded from the analysis either because they are not strictly comparable with industrial corporations, owing to their distinct financial structures, or because they operate in regulated industries with different patterns of managerial compensation (Smyth et al. 1975). After some additions, the preliminary sample contained 284 industrial corporations. However, it was necessary to delete 66 corporations for a variety of reasons: 35 corporations because of succession of the chief executive officer, 22 because of incomplete or missing proxy statements, and nine because of minority control by another corporation

in the sample. An examination of the corporations deleted indicates that they do not differ significantly, in terms of size, profitability, or control configurations, from the corporations retained for the analysis. As a result of these deletions, the final sample contained 218 industrial corporations.

The dependent variable of interest in this analysis is managerial compensation. As in all prior studies of this issue, managerial compensation is defined operationally as the compensation received by the chief executive officer of a corporation rather than the aggregate compensation received by senior management officials as a group. This variable is especially relevant for this analysis, since it focuses on the compensation received by the chief executive officer in relation to the control configuration of the corporation. Specifically, this analysis uses three separate but empirically related measures of the compensation of the chief executive officer. The first of these is direct remuneration, defined as the sum of the salary, bonus, and any deferred compensation received by the chief executive officer for his services during a given year. The second is total remuneration, defined as the sum of direct remuneration plus the current value of the capital gains realized from any stock options exercised by the chief executive officer during the year. Finally, the third measure is aggregate income, defined as the sum of total remuneration plus the income from dividends paid on any stock in the corporation held by the chief executive officer.

These measures of managerial compensation require further elaboration on two points. First, in order to correct for minor annual fluctuations, each measure of compensation is based on the average annual compensation received by a chief executive officer during 1975 and 1976. Similarly, corporate size is measured by the average annual sales for each corporation during this period, while corporate performance is measured by the average annual return on investment for each corporation, in terms of net income as a ratio of assets, during this same period. Second, the measure of aggregate income combines income from two conceptually distinct sources. One source is remuneration for services as a management official, while the other, involving dividend income from corporate stock, is a return on capital. These two components of aggregate income are important because they represent all of the sources of income from a single organization. It must be noted, however, that there is a necessary relationship between this aggregate income measure and the different control configurations, unless a corporation does not pay dividends on its stock.

In order to examine the relationship of managerial power within the corporation to managerial compensation, it is necessary to classify each corporation as having one of four distinct control configurations based on the distribution of stock ownership among the directors, including the chief executive officer. The first configuration is that of management control, in which no member of the board of directors, including the chief execu-

tive officer, controls a significant block of stock. Next there is the configuration of direct family control, in which the chief executive officer is a member of the only family represented on the board of directors which controls a significant block of stock. A third configuration is that of joint family control, in which the chief executive officer is a member of a family which controls a significant block of stock but at least one other family represented on the board of directors also controls a significant block of stock. Finally, there is the configuration of indirect family control, in which the chief executive officer does not control a significant block of stock but at least one of the other members of the board of directors controls such a block of stock. In each case, it is the family, defined as the members of a particular descent group and their affines, which constitutes the basic unit of corporate control (Zeitlin 1974). For example, a corporation in which several directors, including the chief executive officer, control significant blocks of stock is considered a case of direct family control, rather than joint family control, whenever all of these individuals are members of the same family.

For the purposes of this analysis, a significant block of stock is defined initially as 5% of the voting stock in a corporation, even though this stock may be distributed among the members of a family. Recent studies of corporate control have adopted 5% stock ownership by a single family, in conjunction with representation on the board of directors, as a minimal condition for family control (Villarejo 1962; Chevalier 1970; Burch 1972; McEachern 1977). Indeed, a family with representation on the board of directors may be able to exert some degree of control with much less than 5% of the voting stock (Zeitlin 1974, 1976; Allen 1976). Consequently, this analysis differentiates between family control, requiring at least 5% of the voting stock and representation on the board of directors, and family influence, requiring as little as 1% of the voting stock and representation on the board of directors. As with prior studies of corporate control, information on the principal stockholders of each corporation is compiled from reports filed with the Securities and Exchange Commission, articles published in major business and financial publications, and previous studies of corporate ownership and control (Villarejo 1962; Lundberg 1968; Larner 1970; Chevalier 1970; Burch 1972; Pedersen and Tabb 1976).

RESULTS

The primary theoretical question posed by this research is the extent to which the compensation received by a chief executive officer is directly related to his power within the corporation as revealed by the control configuration of that corporation. A direct test of this hypothesis is simply to compare the average compensation received by chief executive officers of

corporations with each type of control configuration using analysis of variance. However, such an analysis ignores the fact that the control configuration of a corporation may be related to other important determinants of managerial compensation, such as corporate size and performance. The analytical technique most appropriate for such a problem is analysis of covariance. This technique yields estimates of the effects of each type of control configuration on managerial compensation, while controlling for the effects of corporate size and performance on this compensation. These adjusted effects, as they are termed, represent the expected differences in managerial compensation for corporations with each type of control configuration if all corporations were equal in size and performance.

The first set of comparisons involving the average level of managerial compensation for corporations with each type of control configuration, controlling for size and performance, is presented in table 1. These adjusted effects are expressed as deviations from the grand mean, given in thousands of dollars, on each of the three measures of compensation. For example, the average direct remuneration for chief executive officers of management-controlled firms is \$16,600 above the overall average of \$357,500 for all chief executive officers, while the average direct remuneration for chief executive officers of indirectly family-controlled firms is \$18,000 below this overall average. This table also indicates the correlation between corporate size and performance and each of the measures of compensation as well as the contribution of each set of independent variables to an explanation of the variance in each dependent variable. For example, the measures of corporate size and performance together explain 30.3% of the variance in direct remuneration, while these two variables in conjunction with the control configuration variables explain 34.9% of the variance in this same dependent variable. Therefore, the control configuration variables explain

TABLE 1
ADJUSTED EFFECTS OF CONTROL CONFIGURATIONS ON MANAGERIAL
COMPENSATION IN CASE OF CONTROL BY FAMILY

Type of Control Configuration	Direct Remuneration	Total Remuneration	Aggregate Income
Management control ($N=142$)	16.6	21.1	-98.5
Direct family control ($N=23$)	-67.7	-110.1	692.5
Joint family control ($N=10$)	-3.0	7.9	560.7
Indirect family control ($N=43$)	-18.0	-12.6	-175.5
Mean	357.5	415.6	602.0
r_{zs}	.518	.324	.100
r_{zp}	.209	.228	.149
$R_{z.sp}^2$.303	.151	.031
$R_{z.sp^2}$.349	.184	.339
$R_{z(c.sp)^2}$.046	.033	.308
Probability	.002	.037	.000

4.6% of the variance in direct remuneration that cannot be explained by both corporate size and performance.

An examination of the results presented in table 1 reveals that most, but not all, of the differences in managerial compensation among corporations with each type of control configuration are consistent with the hypothesis that the compensation of a chief executive officer is directly related to his power within the corporation. For instance, chief executive officers of management-controlled firms receive more direct and total remuneration than other chief executive officers in general, although they receive less aggregate income than chief executive officers of directly or jointly family-controlled firms. Moreover, chief executive officers of indirectly family-controlled firms receive less direct and total remuneration, as well as less aggregate income, than those of management-controlled firms. As anticipated, chief executive officers of jointly family-controlled firms receive more direct and total remuneration than their counterparts in indirectly family-controlled firms but less than chief executive officers of management-controlled firms. There are, however, some entirely unanticipated results as well. Chief executive officers of directly family-controlled firms, for example, receive less direct and total remuneration than other chief executive officers in general, even though these same persons receive more aggregate income than other chief executive officers. In short, the main exceptions to the central hypothesis of this research involve chief executive officers who are also principal stockholders in their corporations. These chief executive officers receive less direct and total remuneration than others, perhaps because they receive considerably more aggregate income than others as the result of dividends paid on their stock in the corporation.

Although the control configuration variables explain a significant proportion of the variance in each of the measures of compensation, it is evident that corporate size and performance explain a larger proportion of the variance in both direct and total remuneration than the control configuration variables. All of these variables explain less of the variance in total remuneration than in direct remuneration. This pattern may be attributable to the fact that total remuneration includes capital gains from stock options. Since the magnitude of these capital gains depends upon the appreciation in the market value of the stock prior to the exercise of an option by a chief executive officer, this measure of compensation is probably affected by stock market conditions in general as much as it is by the characteristics of particular firms. Finally, it must be reiterated that the relationship between the control configuration variables and aggregate income is inherently spurious, inasmuch as chief executive officers who are also principal stockholders necessarily receive a major portion of their aggregate income from dividends paid on their stock in the corporation. For example, chief executive officers of directly and jointly family-controlled

firms receive 76.4% and 63.6% of their respective aggregate incomes from dividends on their stock. Conversely, chief executive officers of management-controlled and indirectly family-controlled firms receive only 13.3% and 5.5% of their respective aggregate incomes from such dividends. These variations in aggregate income are still theoretically significant because they contribute to an interpretation of the variations in direct and total remuneration among corporations with different control configurations.

The final theoretical issue raised by this study is the effect of family influence, as opposed to family control, on managerial compensation. The adjusted effects of each control configuration on the three dimensions of compensation, using only 1% stock ownership by a family represented on the board of directors as the basis for classification, are presented in table 2. A comparison of these results with those presented earlier indicates that the general relationship between the control configuration variables and the measures of managerial compensation obtains, albeit in greatly attenuated form, even under conditions of family influence rather than family control. In general, there is a direct relationship between managerial power and compensation. Once again, the major exceptions involve the direct and total remuneration received by chief executive officers who are also principal stockholders in their corporations. The only differences between the results based on family influence and those based on family control concern the magnitude of the effects of each control configuration on each dimension of compensation. Indeed, the variations in direct and total remuneration among corporations with different control configurations are not statistically significant under conditions of family influence, even though the variations in aggregate income are statistically significant.

TABLE 2
ADJUSTED EFFECTS OF CONTROL CONFIGURATIONS ON MANAGERIAL
COMPENSATION IN CASE OF INFLUENCE BY FAMILY

Type of Control Configuration	Direct Remuneration	Total Remuneration	Aggregate Income
Management control ($N=83$).....	17.1	26.8	-120.2
Direct family influence ($N=40$).....	-18.9	-53.3	445.9
Joint family influence ($N=13$).....	3.7	3.5	417.9
Indirect family influence ($N=82$).....	-8.7	-1.7	-162.2
Mean.....	357.5	415.6	602.0
$\beta_{xs \cdot p}$510	.315	.094
$\beta_{xp \cdot s}$187	.214	.145
$R_{x \cdot sp}^2$303	.151	.031
$R_{x \cdot spc}^2$315	.167	.278
$R_{x(c \cdot sp)}^2$012	.016	.247
Probability.....	.278	.250	.000

CONCLUSIONS

The results of this analysis provide considerable, albeit somewhat qualified, confirmation for the hypothesis that the compensation of a chief executive officer is directly related to his power within the corporation. Certainly, chief executive officers of corporations without any principal stockholders as directors are generally overcompensated in terms of their direct and total remuneration. Conversely, chief executive officers of corporations with one or more principal stockholders as directors are typically undercompensated in terms of their direct and total remuneration, particularly if these chief executive officers are not themselves principal stockholders. These same results also suggest major modifications of the original hypothesis. It is apparent that chief executive officers who are the only principal stockholders among the directors of their corporations are undercompensated in terms of their direct and total remuneration, perhaps because they are simultaneously overcompensated in terms of their aggregate income. Indeed, this pattern is not inconsistent with the basic premises of power theory, since it may represent a deliberate strategy on the part of chief executive officers who are also principal stockholders to obviate any potential proxy contests or litigation by dissident stockholders over the issue of managerial compensation.

Despite the fact that these results generally serve to confirm the power theory of organizational stratification, it cannot be concluded that they necessarily disconfirm the functional theory of organizational stratification as it pertains to the large corporation. In fact, the substantial effects of both corporate size and performance on managerial compensation are equally compatible with either a power or functional theory of organizational stratification. Nevertheless, power theory goes beyond functional theory inasmuch as it posits a relationship between the control configuration of a corporation and the compensation received by its chief executive officer which is independent of the size and performance of the corporation. In short, power theory provides a more adequate explanation of managerial compensation in the large corporation than one derived solely from functional theory.

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

ECONOMIC EFFECTS OF REVOLUTION: A REEVALUATION OF CUBAN EVIDENCE

In an interesting recent article in this *Journal* ("Some Economic Effects of Revolution: Models, Measurement, and the Cuban Evidence," *AJS* 84 [March 1979]: 1127-49), Michael S. Lewis-Beck uses an interaction regression model to test for short-run shifts in the level of output and long-run shifts in the rate of growth of output which may have occurred as a result of the Cuban Revolution. Sugar production, GNP, and energy consumption are considered as measures of output, with the best results obtained using energy consumption. He concludes that the revolution produced a short-run increase in the level of output but a long-run decrease in its rate of growth.

Lewis-Beck stresses the usefulness of energy consumption as a measure of Cuban economic activity: "Energy consumption figures seem much less likely to be 'fudged' than GNP figures, which easily lend themselves to propaganda purposes" (p. 1140). In this comment we present evidence that the energy consumption figures may contain certain politically motivated distortions which make statistical tests based on an interaction regression model highly unreliable. We also comment on Lewis-Beck's contention that U.S. sanctions have been a primary external economic influence on post-revolutionary Cuban economic growth (p. 1146).

He begins by presenting three stylized theories of revolutionary economic effects: the Marxist, the Thermidorian, and the Conservative (p. 1128).

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Each theory predicts that a revolution will create temporary economic disruption and therefore be associated with a short-run decrease in energy consumption. Lewis-Beck's statistical results are inconsistent with this prediction: they suggest that a short-run *increase* in energy consumption occurred as a result of the Cuban Revolution. An ad hoc rationalization might be that the Batista regime was so oppressive, and its overthrow so swift and costless, that suppressed economic vigor was immediately released. Careful examination of the energy consumption data, however, casts serious doubt on such an explanation.

Since the author did not publish his raw data, we reconstructed them from sources cited. This proved to be more difficult than expected because many of the figures were revised in later issues. In each case, we took the latest available figure. For example, 1973 energy consumption was 10.49 million metric tons of coal equivalent according to the 1977 United Nations *Statistical Yearbook*, but 10.65 according to the 1975 *Yearbook* (a 1.5% difference); consequently, we used 10.49 as the figure for 1973. These revisions tended to be minor with a notable exception discussed below. Our raw data series are reproduced in the Appendix.

His model is: $\log_e Y_t = b_0 + b_1 X_{1t} + b_2 X_{2t} + b_3 X_{3t} + e_t$, where $\log_e Y_t$ is the natural logarithm of energy consumption (total or per capita); X_{1t} is a counter for years from 1 to N , the number of observations; X_{2t} is a dichotomous (dummy) variable scored 0 for observations before the revolution and 1 for observations after the revolution; X_{3t} is a counter of years scored 0 for observations before the revolution and 1, 2, 3, . . . for observations after the revolution; b_0 is the intercept of the time series before the revolution; b_1 is the slope before the revolution; b_2 is the change in the intercept after the revolution; b_3 is the change in slope after the revolution; and e_t is an error term with the usual least-squares properties. When this model is applied to the data in the Appendix, the following results are obtained:

$$\log_e Y_t = .839 + .076X_{1t} + .099X_{2t} - .033X_{3t} + e_t, \\ (35.12) \quad (19.74) \quad (3.69) \quad (-7.85) \quad (1)$$

$$R^2 = .99, \quad d = 1.55;$$

$$\log_e Y_t = 6.081 + .055X_{1t} + .093X_{2t} - .032X_{3t} + e_t, \\ (264.26) \quad (14.70) \quad (3.62) \quad (-8.05) \quad (2)$$

$$R^2 = .99, \quad d = 1.75.$$

The parentheses contain t -ratios, R^2 is the coefficient of determination corrected for degrees of freedom, and d is the Durbin-Watson statistic. Total energy consumption is used in equation (1); per capita energy consumption is used in equation (2). Our results are almost identical to those

obtained by Lewis-Beck, the minor differences probably being due to the fact that our data extend to 1976 while his stop at 1974.

Energy consumption figures for 1957 and 1958 (the years before the revolution) differ substantially in different issues of the *Yearbook*. The Appendix contains the latest figures provided by the Castro regime: 4.71 for 1957 and 4.70 for 1958. The 1959 *Yearbook* is the latest one containing data supplied by the Batista regime: it cites 5.59 and 6.06 as the energy consumption values for these years. These revisions by the Castro regime are of a major nature; the Batista figures are revised *down* by 16% and by 22%, respectively. If the Castro estimates are accepted, consumption *increased* by 14% immediately after the revolution (in 1959). If the Batista estimates are accepted, however, there was a postrevolutionary *decrease* of 12% in energy consumption. This is confirmed when equations (1) and (2) are rerun substituting the Batista figures for the Castro figures for 1957 and 1958. The results are:

$$\log_e Y_t = .765 + .097X_{1t} - .039X_{2t} - .054X_{3t} + e_t, \\ (24.16) \quad (19.03) \quad (-1.09) \quad (-9.75) \quad (3)$$

$$R^2 = .99, \quad d = 1.81;$$

$$\log_e Y_t = 6.007 + .076X_{1t} - .044X_{2t} - .053X_{3t} + e_t, \\ (192.62) \quad (15.05) \quad (-1.26) \quad (-9.83) \quad (4)$$

$$R^2 = .99, \quad d = 1.89.$$

Comparing equation (3) with (1), and (4) with (2), the most striking difference is that b_2 , the coefficient for X_{2t} which measures the postrevolution short-term change in energy consumption level, shifts from strongly positive to weakly negative. The pattern of slower postrevolutionary growth is present in both sets of equations; however, the difference between pre- and postrevolutionary growth rates ($b_1 - b_3$) is larger when the Batista data are used. Thus the Castro data are ambiguous with respect to the three theoretical models, whereas replacement of two questionable observations with their unrevised Batista equivalents leads to results which support the Conservative model (initial postrevolutionary output drop, followed by slower long-term growth). Equations (1) and (3) are shown graphically in figure 1.

The substantial differences in the 1957 and 1958 figures make it difficult for us to accept Lewis-Beck's contention that energy consumption data are not likely to be altered for political purposes. We do not, however, wish to debate the question of which set of estimates is more accurate. The main point is that the interaction regression model yields questionable estimates of the postrevolutionary short-run change in output level because (a) small changes in the data in the vicinity of the revolution can produce large

changes in the postrevolution intercept, (b) the incoming regime has a strong political incentive to alter precisely these data points to generate the appearance of a smooth transition of power, and (c) even if the data are not intentionally altered, immediate postrevolutionary observations are likely to contain substantial measurement error.

Next, we consider Lewis-Beck's analysis of the reasons for the lower postrevolutionary growth rate in Cuba. He states that "while the Cuban government obviously directs the Cuban economy, there are external influences over which it has little control. Of overwhelming importance here is . . . the U.S. embargo. . . . The imposition of the embargo is almost coincident with the revolution and so may share responsibility for the drop in economic output" (p. 1146). He goes on to argue that since the revolution occurred in late 1958 and the embargo did not commence until 1960, the

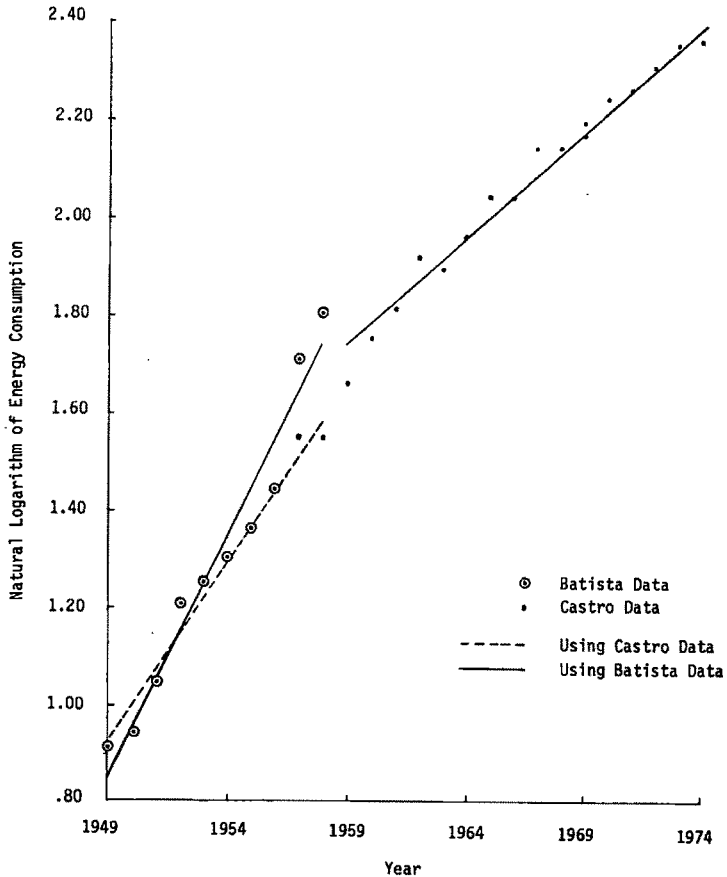


FIG. 1.—Cuban energy consumption: Batista data versus Castro data

value of b_2 , the postrevolutionary change in intercept, constitutes a partial test of short-run revolutionary effects.

Two comments are appropriate here. First, a balanced assessment of Cuba's postrevolutionary growth experience should include more than one "external influence." A better statement might read (to paraphrase Lewis-Beck): "Of overwhelming importance here is U.S. (on the negative side) and USSR (on the positive side) policy." It is not clear that the United States must shoulder all of the blame for Cuba's lower postrevolutionary growth rate: internal problems no doubt have played a role. Nor is it clear that the USSR would have refrained from aiding Cuba had the United States not imposed the embargo, since both countries had much to gain from political and economic ties.

Second, a complete model should include the revolution and the embargo as separate variables so that their effects are not confounded. Following the helpful suggestion of an anonymous referee, we experimented with several models of this type and obtained interesting results by adding an embargo dummy, X_{4t} , equal to 0 before 1961 and 1 thereafter, to equations (3) and (4). The new regression coefficient, b_4 , measures the change in the level of the series after the embargo. Applying this model to the Batista data in the Appendix yields the following results:

$$\log_e Y_t = .765 + .097X_{1t} - .084X_{2t} - .056X_{3t} + .079X_{4t} + e_t, \\ (25.69) \quad (20.23) \quad (-2.09) \quad (-10.55) \quad (2.03) \quad (5)$$

$$R^2 = .99, \quad d = 1.95;$$

$$\log_e Y_t = 6.007 + .076X_{1t} - .084X_{2t} - .056X_{3t} + .069X_{4t} + e_t, \\ (201.05) \quad (15.71) \quad (-2.08) \quad (-10.38) \quad (1.77) \quad (6)$$

$$R^2 = .98, \quad d = 1.96.$$

Bearing in mind that, as demonstrated above, the intercept shift coefficients b_2 and b_4 in this type of model are sensitive to small changes in the data, it is nevertheless clear that equations (5) and (6) are an improvement over (3) and (4) in terms of t -ratios and the Durbin-Watson statistic. Comparing b_2 and b_4 , it appears that the revolution triggered a significant drop in the level of energy consumption which was partially counterbalanced by an increase after the embargo. The postembargo increase may have been due to Soviet aid, to increased domestic effort in the face of an external threat from the United States, to normal processes of recovery from the trauma of revolution, or, what is most likely, to some combination of these three factors.

In conclusion, Lewis-Beck has extended sociometrics to a new and interesting field of investigation with his analysis of the Cuban Revolution. It is clear that many interesting issues about Cuba remain to be addressed; for

example, disentangling the causes of the short-term reaction to the embargo. What is less clear is whether this approach is of general usefulness in the analysis of revolution, given the problems inherent in applying regression methods to small, nonexperimental data sets.

APPENDIX

CUBAN ENERGY CONSUMPTION AND POPULATION (1949-76)

Year	Population (Million)	Source: <i>Demo- graphic Yearbook</i>	Energy Consumption (Million Metric Tons)	Source: UN <i>Statistical Yearbook</i>
1949.....	5.40	1966	2.485	1952
1950.....	5.52	1970	2.561	1953
1951.....	5.64	1970	2.840	1954
1952.....	5.76	1970	3.346	1956
1953.....	5.89	1970	3.504	1957
1954.....	6.02	1970	3.692	1957
1955.....	6.15	1970	3.95	1959
1956.....	6.28	1970	4.21	1959
1957.....	6.41	1970	4.71*	1961
1958.....	6.55	1970	4.70*	1962
1959.....	6.69	1970	5.34	1963
1960.....	6.83	1970	5.76	1964
1961.....	6.94	1970	6.17	1965
1962.....	7.07	1971	6.97	1966
1963.....	7.31	1972	6.64	1967
1964.....	7.51	1973	7.10	1968
1965.....	7.72	1974	7.67	1969
1966.....	7.89	1975	7.72	1970
1967.....	8.05	1976	8.45	1971
1968.....	8.20	1977	8.43	1972
1969.....	8.42	1977	8.90	1973
1970.....	8.55	1977	9.39	1974
1971.....	8.69	1977	9.60	1975
1972.....	8.86	1977	10.05	1976
1973.....	9.04	1977	10.49	1977
1974.....	9.19	1977	10.62	1977
1975.....	9.33	1977	11.15	1977
1976.....	9.46	1977	11.60	1977

* According to the UN *Statistical Yearbook* (1959), these observations were 5.59 for 1957 and 6.06 for 1958.

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CAN WE ASSESS THE EFFECTS OF REVOLUTION?
A THIRD LOOK AT THE CUBAN EVIDENCE

One is always pleased to have his or her work read carefully. My pleasure is doubled when the readers are economists and statisticians. But the evaluation by Pilarski and Snyder has a number of uses beyond providing these personal satisfactions. For one, it updates the Cuban findings to 1976. More broadly, however, it draws attention to two critical questions about doing research on revolution. First, Is it possible to get good data in a revolutionary situation? Second, granting good data, To what extent are the observed changes caused by the revolution? I will address each of these questions in turn.

It goes without saying that reliable indicators of social and economic phenomena are harder to come by in a revolutionary situation. Certain common data-gathering procedures are difficult for researchers to apply. Further, data gathered routinely by government statistical bureaus may be spotty, inconsistent, or distorted for political purposes. A prime example here is annual Cuban GNP figures across the Batista and Castro eras. There are gaps in the series, the method of computation changes from the capitalist to the socialist government, and the figures beg to be altered for political ends. Hence, if one is interested in tracking an aggregate indicator of Cuban economic growth, a substitute should be found for GNP. As discussed in detail in my article (pp. 1139-41), strong reasons exist for using energy consumption as a proxy for GNP.

When a simple interrupted time-series (ITS) analysis is applied to energy consumption observations, the results show that the Cuban Revolution was followed by significant, positive economic growth in the short run but significant negative economic growth in the long run. Pilarski and Snyder question the finding of a significant short-run effect, arguing that it is probably caused by Castro's distorting key observations for political ends. They show that, when the Batista figures are substituted for these key years (1957 and 1958), this significant, positive short-run effect disappears. Thus, they conclude, the energy consumption measure is vulnerable to politically motivated manipulation and cannot serve as a reliable national economic indicator in a revolutionary situation. I disagree.

The Castro and Batista figures on energy consumption for 1957 and 1958 do in fact differ widely. The question is, Why this difference? A definitive answer is of course impossible, but I will put forward a few guesses. The explanations can be grouped into two broad categories: "fudging" and measurement error. I would first like to consider fudging, defined as manipulation of a statistic for political purposes. With regard to the Cuban case, there are two basic possibilities: Castro fudged the 1957 and 1958

figures, or Batista did. Pilarski and Snyder entertain only the first possibility. However, inspection of the data suggests that the Batista team may actually be the culprits. Look at the scatterplot provided by Pilarski and Snyder. The Batista figures for 1957 and 1958 deviate seriously from the straight line formed by the observations on previous years (fit a ruler to the circled dots, 1949–56). In fact, if we accept these Batista figures, we are forced to conclude that, during his last two years in power, the Cuban economy was doing better than ever! Given that this outcome is so unlikely, the implication is strong that Batista altered the data to achieve it.

These energy consumption data, then, can be made to appear compatible with the fudging hypothesis, although the fudging may have been committed by Batista rather than Castro. However, I reject this interpretation in favor of a measurement error hypothesis. There might be at least two kinds of measurement error operating: random measurement error and “revolutionary” measurement error. The deviations (from the 1949–56 line) represented by the 1957 and 1958 observations can be explained by the notion of revolutionary measurement error. That is, the revolutionary situation prevailing in 1957 and 1958 made it unusually difficult to get complete, accurate data. Therefore, the Batista energy consumption estimates of those years were very preliminary. Before Batista’s statisticians could revise these estimates, Castro came to power. Castro’s statisticians, benefiting from the more complete data finally in on 1957 and 1958, were able to make more precise estimates. Empirical evidence that the Castro estimates are more accurate comes from the fact that they are much closer to the projection of a straight line from the 1949–56 points. In other words, the Castro revisions merely reflect the more complete data which eventually became available. Milder instances of this revision process occur throughout the entire series; regardless of whether the year is during the Batista or Castro eras, the figures tend to be revised in later volumes of the statistical yearbooks, in keeping with the more complete information that becomes available.

Thus, with regard to energy consumption, Castro’s statisticians did not fudge; they merely had better data. Such a conclusion is not as controversial as it may sound. We are not talking about measuring politically sensitive variables like GNP or sugar production. Neither officials, academics, nor the public regarded energy consumption as a central economic indicator. Undoubtedly, unheralded government bureaucrats went along quietly gathering data on energy consumption, much as they had always done. After all, if sugar production data were left unaltered by Castro’s statisticians, it seems unlikely that they would alter the obscure energy consumption figures (even critic Mesa-Lago concedes there was no sugar data manipulation until 1965; my analysis suggests Castro never manipulated the sugar data; see p. 1133).

Because of the foregoing reasons, I must accept the energy consumption series as I originally compiled it (including the "Castro" data on 1957 and 1958). An ITS analysis of this series indicates that the revolution had a positive, significant, short-run economic effect. I might add that this conclusion squares with the general impression of many observers of the Cuban economic scene immediately after the revolution.

Granting the reliability of these energy data brings us to the second question, To what extent are the observed changes caused by the revolution? In seeking an answer, I estimated the ITS model discussed above. From a methodological point of view, a revolution seems an ideal "interruption" to assess, because it so resists the confounding influence of third variables. That is, revolution represents such a massive manipulation that any significant changes afterward would almost undoubtedly be attributable to it. However, as with any quasi-experimental design, the confounding potential of third variables must be considered. A prime candidate here is the U.S. embargo, which began at almost the same time. One way to handle the embargo is simply to regard it as part of the revolutionary interruption, arguing that a revolution involves internal and external changes, for example, a shift in trade patterns. Then, any observed effect is assignable to the revolution, but with the understanding that the revolution encompasses external as well as internal events.

Another way to handle the issue is to separate the internal aspects of revolutionary change from the external ones. A precise partitioning into internal and external revolutionary effects is problematic in the Cuban case, because the U.S. embargo began in 1960. Still, this means there is one year, 1959, when the Cuban revolution was operating free of an embargo. For that year, the first year of the revolution, energy consumption increased significantly from 1958, as my ITS analysis shows. This suggests that, at least in the short run, a domestic revolution produces a positive economic effect.

It is more difficult to separate unambiguously the long-run effect of the domestic revolution from that of the U.S. embargo, as I am at pains to make clear in my discussion (p. 1146). I nowhere imply that "the United States must shoulder all the blame for Cuba's lower postrevolutionary growth rate," as Pilarski and Snyder charge. Instead, I indicate that the U.S. embargo is *one* of the "external influences" operating and "*may share responsibility for the drop in economic output*" (p. 1146, italics added). I go on to state, "But, at about the same time the embargo was taking hold, important internal changes in Cuba were occurring, for example, the transition to a socialist state was being completed, and the Soviet Union was assuming a major role. Therefore, by the 1960-61 period, major variables potentially influencing growth are once again confounded" (p. 1146).

This confounding of internal and external variables is not overcome by

the procedure Pilarski and Snyder propose. They insert into the original ITS model a dichotomous variable, X_4 , scored 0 before 1961 and 1 thereafter, in an attempt to separate the effects of the domestic revolution and the embargo (see their eqq. [5] and [6]). This attempt fails on several counts: (1) Given that the principal interest is in the effect of revolution free of the contaminating influence of the embargo, the more proper test is the change from 1958 to 1959, for there was no embargo at all during this period (this effect, of course, is what is estimated by b_2 in my original equations; see also, Pilarski and Snyder's eqq. [1] and [2]). (2) This dichotomous variable assesses only a short-run effect, from 1960 to 1961, whereas the research question concerns the long-run effect of the embargo. (3) Inclusion of the X_4 variable poses formidable multicollinearity problems (recall that X_2 and X_4 are scored exactly alike except for two years, 1959 and 1960). Although Pilarski and Snyder do not present a correlation of these variables, I suspect it is high enough to make their estimates quite unreliable. Certainly, the appearance of large coefficients for b_2 and b_4 and the opposite direction of their signs hint at a multicollinearity problem.

In the end, Pilarski and Snyder are terribly pessimistic, concluding that my analysis of revolution has limited utility because of "the problems inherent in applying regression methods to small, nonexperimental data sets." How, then, are we to study revolution? Survey research? Field experiments? Participation-observation? While these methods have their strengths, they also have obvious weaknesses in this context. Admittedly, the regression analysis of aggregate time-series data on revolution is not problem free, as this exchange of comments demonstrates. Nevertheless, the problems are those we routinely grapple with in nonexperimental social science research, for example, measurement error, spuriousness, multicollinearity. The occasional presence of such difficulties does not cause me to give up on regression analysis of nonexperimental data. I am sure that Pilarski and Snyder, as economists and statisticians, have not really given up on it either.

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ON CAMIC'S ANTIPRESENTIST METHODOLOGY

Quite beyond its unusually scholarly reading of the works of Hume, Smith, Bentham, and Mill, Charles Camic's "The Utilitarians Revisited" (*AJS* 85 [November 1979]: 516-50) provides some exceptionally self-conscious re-

flections upon the means by which sociologists reconstruct their past. Critically, endorsing the arguments of Stocking (1968), Peel (1971), and myself (Jones 1977), Camic maligns the "presentist" or "Whiggish" approach which "distorts the very process which the study of the past seeks to elucidate" (p. 517); more constructively, he at least partially endorses my proposed alternative, which suggests that to overcome presentist orientations and consequent distortions, we must try to understand classic sociological works "in their own terms" (p. 517).

Almost immediately, however, Camic adds that his approach differs from mine "in one major respect and may thus appear methodologically unsound. If the objective of historians of sociology is to understand sociological classics," he asks, "why discuss the works of Hume, Smith, Bentham, and J. S. Mill, which are in no sense sociological classics?" (p. 518). His answer is that this objective itself is misconceived, that is, historians of sociology "cannot limit their inquiries to classic sociological works. To take sociological classics as the starting point for the investigation of sociology's past is to accept present definitions of what past works and authors it is legitimate to examine" (p. 518). While Camic grants that my own approach may relieve us of presentist interpretations of Durkheim, he argues that it still leads us to Durkheim rather than, say, Plato or Hume, whose works (and here Camic quotes the estimable work of John Peel) were "attempts to grapple with a different reality, the answers to different problems, the upshot of different purposes" (p. 518, quoted from Peel [1971], p. 264). But it is precisely such variability of realities, problems, and purposes, Camic urges, which historians of sociology need to explore. Therefore he concludes that we must "move beyond the analysis of classic works by sociologists to investigations of the social theories of those who occupied other social roles. Unless such a step is taken, sociologists sacrifice crucial material for understanding the process whereby ideas of the social grow, emerge, and change as a consequence of problems quite different from those addressed by those who were sociologists" (p. 545).

Conjoined as it is to a substantive rereading of the utilitarians, Camic's critique raises some interesting methodological questions; and as he himself suggests (pp. 517, 545), our agreements may be more important than our differences. Nonetheless, it seems to me that he has neither fully understood my argument nor thought through the implications of his. In the first case, for example, I have never suggested that we should limit our investigations to "sociological classics," least of all in the narrow sense assumed by Camic. Instead, the consistent force of my argument has been to attack (1) the traditional justification for such a limitation—that such classics contain certain "timeless truths" of continuing relevance to contemporary sociological theory and thus constitute both the necessary and the sufficient focus for our historical investigations; and (2) the traditional

method which this justification legitimates—the reading of a few texts “over and over again,” in relative ignorance of the historical context in which the works were produced (see Jones 1977, 1978, 1981). In making such an argument, of course, my focus has necessarily been on the conditions requisite to an understanding of such texts; and, in a sense quite different from that attacked by Camic, I have indeed argued that such texts occupy a special place in our investigations. But, as I will try to make clear, this is not the same as arguing that historians of sociology should “limit their inquiries to sociological classics.” On the contrary, it leads us into a variety of contextualism beyond anything Camic has yet imagined.

Second, Camic seems encumbered with an unusually restrictive notion of what the term “sociological classic” implies, so that even if I *had* suggested that our inquiries be so limited, we would still be talking at cross-purposes. In fact, his own definition must be determined from the way he uses the term; but since it quite peremptorily excludes the works of Plato, Hume, and Smith, and since Camic’s corrective to my “limitation” is the study of those who occupied other social roles, it seems reasonable to assume that his notion of a sociological classic is simply that of a highly reputable work written by someone occupying the institutionalized role of “sociologist.” And had I both implied the limitation denied above and applied it in this restrictive manner, Camic would indeed be quite right to reprimand my focus on such works and recommend instead that we “move beyond the classics.” Indeed, in our search for works thus defined, we could scarcely move back into the 19th century before finding ourselves in the embarrassing situation of writing the history of an entity (“sociology”) which did not, in fact, exist.

If Camic’s definition is thus unduly restrictive, I must admit, nevertheless, that my own has remained unspecified; his argument thus performs the salutary function of forcing us to clarify our terms. What, then, is a sociological classic? To borrow (as I consistently do) from the more self-consciously developed field of the history of political theory, it is difficult to improve upon the notion that to speak of a particular work as a classic is simply to suggest that we have special reasons for wishing to understand it; and when these special reasons include (but are not necessarily limited to) the fact that we find the work sociologically rational, significant, and interesting, we might reasonably speak of a sociological classic. Within such a definition, of course, what is classic, what is sociological, and the special reasons why a given work is defined (or not defined) as either will vary historically, but to say this is merely to confirm historical precedent: the fortunes of “classic works” have been as capricious and unpredictable as the careers and reputations of their authors. Such a definition, incidentally, would at present include at least some of the works of Plato, Hume, and Smith in any but the most parochial of minds; thus I recom-

mend it to Camic if only to assuage at least a few of his otherwise quite reasonable fears.

So far, however, I have not faced Camic's central argument; indeed, I seem almost to have supported it. For I have suggested that the collective judgment of what is or is not a sociological classic (like that of a "sacred object") is less a matter of identifying certain intrinsic qualities than one of providing a socially contingent definition. Therefore, even with our enlarged conception of the meaning of these terms, if our decisions as to whether a work is classic or sociological and the criteria leading to those decisions are historically variable, it is difficult to see how they can be, at any given time, anything other than presentist. How, then, are we to avoid the "presentist dilemma" of which Camic warns us?

The answer, of course, is that we cannot. For at the beginning of any historical inquiry, we must make some rather important decisions about what is worthy of study, and it is inconceivable to me that these decisions can be made reasonably by standards other than those operant in the present. In other words, we must apply our own (indeed, who else's?) present criteria of what is sociologically rational, significant, and interesting. For this reason, I have suggested (*pace* Kuhn) that it is not our "presentist preconceptions" which necessarily subvert our efforts to produce a reasonable account of past ideas, for these are, in the sense outlined above, unavoidable. The difficulty in writing good history arises when our familiarity with and commitment to such preconceptions leads us to ignore the intended meaning of a document for the agent who produced it; and to overcome this difficulty, of course, I have advocated an "intentionalist" approach to the history of sociological thought, though in a much larger sense than Camic seems to understand.

This is not to say that more ambitious solutions to this dilemma have not been proposed. One suggestion, for example, is that the object of analysis should be the whole gamut of "languages" in which a culture articulates a particular type of experience over time (Pocock 1971, pp. 3-41); or again, that we should avoid the old "internalist" histories of rational discoveries by attempting to gain "a full picture of the new science" (Rattansi 1968, p. 131; see Hesse 1973). The difficulty with these enthusiastically sociological approaches has been pointed out by Quentin Skinner. If we try to produce the "full picture," Skinner observes, "we are bound sooner rather than later to be reduced merely to coloring in the most boring and trivial details. We are in any case bound to fail. For the facts are infinite in number, and unless we have some ideas about where to begin and why to begin there, we may literally condemn ourselves to going on forever" (Skinner 1974, p. 281). A second alternative, that of applying only those criteria of significance which were accepted at the relevant historical time (see Butterfield 1931, pp. 9-31), I have discussed earlier (Jones

1977, p. 284). First, it is highly unlikely that there will be substantial agreement as to "what matters" among the writers of any historical period; and second, even if there were, it would certainly be a tedious and unsatisfying "history" which merely endorsed such judgments (see Dunn 1968, pp. 97-98; Skinner 1974, p. 281; Jones 1977, p. 284).

Camic presents us with a third "alternative"—that of moving "beyond the analysis of classic works by sociologists to investigations of the social theories of those who occupied other social roles" (p. 545). This proposal, however, is either overambitious or not ambitious enough. If Camic wishes to carry it out in a determinedly "antipresentist" manner, as he occasionally implies, it is hard to see how he can avoid the difficulties of the first alternative discussed in the preceding paragraph, for indeed, the body of writings on "the social" produced by those occupying roles other than that of "sociologist" is surely limitless. Limits might be introduced, of course; but if they were, it is hard to see how Camic could avoid the difficulties of the second alternative and, endorsing the judgment of Hume's contemporaries, treat him more as a historian than a moral philosopher, and ignore altogether the *Treatise on Human Nature*.

In lieu of these egregious alternatives, Camic's only recourse is to make his "crucial decisions" by applying his own present criteria of what is worth studying; and of course this is precisely what he has done. Indeed, one of the most noteworthy features of his antipresentism is that, in going beyond the classics, he quickly comes to rest upon four figures of considerable present stature. Moreover, his effort to understand the utilitarians is typically limited to their most retrospectively prominent works and largely avoids the larger context of ideological discourse from which they emerged. Ultimately, it is only by imposing the utterly restrictive definition of a "sociological classic" assailed above that he is able to present himself as a contextualist at all and, thus, as an opponent of that largely discredited form of "Whiggish" or "presentist" history which he ostensibly rejects. In urging us to go "beyond the classics," therefore, Camic is right for the wrong reasons; and since the reasons are wrong, he does not know how very right he is—in other words, how very seriously the injunction to study the context of past ideas must be taken, and how utterly difficult this enterprise will be.

Briefly, we must concentrate upon the classic texts because, unlike the first two alternatives discussed above, they provide us with one potential answer to the unavoidable question of where our historical inquiries ought to begin; and ultimately, unlike the traditional appeal to the recovery of "timeless truths," this concentration provides a potential means of investing the study of classical social theory with its present value. Once we take seriously the injunction to understand any classic work (in the sense of recovering the author's intention in writing it), we are necessarily asking

questions about the ideological context or "normative vocabulary" which rendered it understandable in the first place. In other words, we must study the social and political conditions of the society in which the classic writer lived and particularly the inherited assumptions about the social order embodied in the more ephemeral writings of his antecedents and contemporaries. That this will involve us in the study of those who occupied "other social roles" is blatantly obvious, although it seems to me to be of questionable explanatory or interpretive significance; the important thing is that these writings set the limits of the normative, ideological context which, in turn, governed the discussion of those topics with which the classic text deals.

While our initial focus may appear traditional, its necessary consequence is consistent with one of the most recurrent themes in modern historiography and one which I suspect Camic would embrace: if we wish to understand earlier societies, we must recover their different *mentalités* in as deeply sympathetic a fashion as possible and thus produce a history of sociological thought which is genuinely historical. It is only by asking very specific questions, however, that we can secure any answers at all, and thus the classics will continue to occupy a special role in some stage of our investigations.

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ON THE METHODOLOGY OF THE HISTORY OF SOCIOLOGY:
A REPLY TO JONES¹

In his comment on my article, Jones provides an illuminating amplification of features of his methodology that were only implicit in his remarkable, earlier *AJS* article, "On Understanding a Sociological Classic" (1977). Now that this methodology is fully exposed, however, it seems to me nearly as insufficient in terms of the purposes for which sociologists study past social theories as I found it to be in "The Utilitarians Revisited."

My argument here can be introduced by clarifying a technical issue raised by Jones's remarks. My article suggested—in part to qualify Jones's original article—that those seeking a sociological understanding of sociology's past "cannot limit their inquiries to classic sociological works," for "to take sociological classics as the starting point for the investigation of sociology's past is to accept present definitions of what past works and authors it is legitimate to examine" (p. 518). Jones now maintains that, in developing this contention, I incorrectly articulated his position on the nature of a sociological classic. Since now he pauses to define that term formally, I can see that I did misconstrue his meaning. That, however, was something his article insufficiently discouraged. There, as Jones quite fairly acknowledges at this point, the term sociological classic "remained unspecified." In the absence of specification, I interpreted Jones in the context of current sociological work on sociology's past. And here, as I am sure Jones would affirm, something closely akin to what he now calls the "unusually restrictive notion of what the term 'sociological classic' implies"² is all too prevalent—witness those works on sociology's past that, while devoting scarcely a line to the Platos and Humes, concentrate on Durkheim, Weber, and a few other 19th- and early 20th-century luminaries who were engaged in producing works of sociology, either as sociologists or as those trying to institutionalize such a social role. Especially since Jones's article focused upon a work that even the most restrictive notion of a sociological classic would recognize—Durkheim's *Elementary Forms*—it was natural to assimilate him into this context. He now makes explicit that he is not to be read in this way and that, like me, he expressly opposes the conventional, myopic view of which works in sociology's past merit investigation. I am glad to learn that we concur on this fundamental point and that he finds obvious my reminder that the study of sociology's past must

¹ I am grateful to Ira Cohen for his remarks on this commentary.

² Jones incorrectly attributes to me the "restrictive notion" of a classic considered in my article. When speaking of the notion, I was describing not my own position, but that traditionally found in sociology.

encompass those not associated with the role of the sociologist.³ One hopes that the readers of Jones's original article realize this and that, insofar as sociological classics are a primary concern in the history of sociological thought, the broadened definition Jones now provides does not remain a minority position.

Since my initial protest against focusing upon sociological classics was developed because it seemed to render illegitimate an examination of Hume, Smith, Bentham, and Mill and since now Jones's liberal conception of a classic grants classical sociological status to the works of these four figures, it would seem that the matter at issue has been resolved. It has not, and this brings me to my central argument. Despite this liberal conception of a classic and although it legitimizes the specific empirical study of the utilitarians reported in my article, I still insist, for precisely the same general reasons I advanced initially, that historians of sociology cannot limit their inquiries to classic sociological works and must instead move beyond the classics. In spirit (though, as we shall see, not quite in letter), Jones's comment challenges this proposal. He argues that insofar as my proposal does not—and it *does* not—wish, in moving beyond the classics, to select which past writings are appropriate to investigate by “applying only those criteria of significance which were accepted at the relevant historical time,” it is too ambitious, for the number of writings susceptible of analysis would then be “limitless.” Now, if this means (and this is all I can imagine it can mean) that my recommendation that studies in the history of sociology move beyond the classics does not state where concretely they should move and thus allows them potentially to move anywhere, it is correct. But this does not appear to be an objection, let alone a rationale for Jones's seemingly contrary suggestion that places boundaries on the vastness of the past by directing historians of sociology in the first instance to the classics.

The reason for claiming this is that, following the methodological proposal implied in my article, one would in practice never actually face the past in all its infinity. Rather, one would—that is, in my judgment one should—approach the past with certain sociological research questions. The most general of these that I mentioned was one that, curiously, Jones himself had formulated earlier (though apparently he took himself less seriously here than I did). I suggested that the object of a sociological history of sociology is “to illuminate the process whereby theories of the social emerge, grow, and change” (p. 518; based on Jones 1977, p. 311). It will be no-

³ At the same time, however, Jones seems not to have grasped why I emphasized social roles in this connection. He remarks that my introduction of the concept of social roles “seems . . . to be of questionable explanatory or interpretive significance,” without once questioning (or even acknowledging) the results of the empirical portions of the article, in which certain notions about social roles are employed in accounting for how the ideas of utilitarians developed and changed over a period of two centuries.

ticed that this concern makes this subarea of sociology analogous at this level to various other subfields in sociology, such as those exploring the process whereby communities, or systems of stratification, or the like, emerge, grow, and change. Of course, the number of communities and stratificational systems is nearly as limitless as the number of past social theories. But this does not generally lead students of communities and stratification to insist that classics—classic communities (New Haven, Chicago), classic stratificational systems (India, early modern England)—be accorded a distinct primacy in their researches.⁴ Such a stipulation would foster the neglect of all relevant materials afforded by nonclassic research sites, when such materials are essential for confirming, extending, or rejecting any conclusions the classics suggest about the process in question. But taking this position does not, practically speaking, doom every investigator to grapple blindly with the limitless possibilities. Although there is no hard and fast logic for discovering where any particular investigator should begin, there is the current stock of knowledge in the subfield. What it knows, half-knows, and must still learn about the empirical process it explores suggests specific empirical issues to the investigator wishing to extend, dispute, or transcend the received wisdom. And it is in the light of these issues—and such variables as imagination, talent, luck, personal preference, time, resources, and good advice—that some classic or non-classic research site comes to appear an appropriate starting point.

The possibility selected—whether it is a community, a stratificational system, or a social theory—can, of course, turn out to be a poor choice, revealing only the “boring and trivial details” Jones fears. But against this outcome there can be no guarantee in any area of sociology. Surely, in the field of the history of sociology, focusing on the classics and employing the (somewhat question-begging) criteria—“sociologically rational, significant, and interesting” (etc.)—by which Jones identifies them offer no such guarantee. Investigations of sociologically interesting and significant work can frequently produce results that are the reverse, that reveal nothing about the process by which social theories develop. This is something Jones’s original article convincingly demonstrated about most previous studies of Durkheim’s classic, *The Elementary Forms*. On the other hand, research on writings that themselves are dubious specimens of work that is sociologically rational and/or significant (and so on) can reveal much about the empirical process in question: witness studies of a group Jones himself has insightfully considered in a very different context, the Saint-Simonians (see Jones and Anservitz 1975).

In so emphasizing the importance of current research questions when

⁴ The relevance of the analogy implied here is not affected by the obvious fact that what makes a research site a classic in these subfields differs somewhat from what makes a social theory a classic.

approaching the past, I may seem to be shifting from the antipresentism of my original article and, in the process, corroborating Jones's claim that studies of sociology's past—including the one presented in "The Utilitarians Revisited"—can proceed only "by applying . . . present criteria of what is worth studying." Two comments are appropriate concerning this contention. First, I am not modifying my earlier argument: at several points, my article insisted that the history of sociology must approach the past with a very contemporary concern, understanding the process whereby theories of the social emerge, grow, and change.

It must be noted, second, however, that this position in no way contradicts the antipresentism I am advocating, I reject two kinds of presentism: (1) the practice, so well exposed in Jones's article, of reading the work of past social theorists, not in terms of "the purposes for which it was constructed, and the questions it was meant to answer" (Jones, cited by Jones [1977], p. 283), but "in an effort to find [in it] . . . certain theories, thematic ideas, or problems regarded as currently constitutive of our discipline" (Jones 1977, pp. 284–85); and (2) the practice, that Jones tends to endorse, of concentrating upon one, often distinctive, subset of past social theories, those currently regarded as sociological classics. These presentist methodological tactics are objectionable precisely because—by leading one either to misinterpret the meaning of past social theories or to misrepresent the range of such theories—they can subvert our present substantive goal, understanding the complex empirical process whereby social theories emerge, grow, and change. When Jones argues that the history of sociology can proceed only "by applying . . . present criteria of what is worth studying," he confounds the field's legitimate, necessary, and unavoidable present substantive concerns with the second variety of methodological presentism, which can and (depending on the specific empirical problem at issue) should be avoided. In other words, Jones conflates the idea that "X-theory is worth studying because it can help answer present questions about how social theories develop" with the notion that "X-theory is worth studying because it is at present regarded as a sociological classic." My study of the utilitarians coincidentally fosters Jones's confusion here because he is coming from the field of political theory where the utilitarians are "figures of considerable present stature," and thus he assumes that they are my subjects due to their worthiness in the latter sense. This, however, is not the case: within contemporary sociology, Hume, Smith, Bentham, and Mill are hardly figures of much stature and in any event, my interest in their theory was dictated by the former criterion, the priority of which is in no way affected by the fact that in some cases it may lead not beyond, but to sociological classics.⁵

⁵ An outcome that would be especially likely were one to employ Jones's broad definition of a classic.

As I noted above, the whole tenor of Jones's comments is to criticize my claim that "historians of sociology . . . cannot limit their inquiries to classic sociological works" (p. 518) and to offer his emphasis on the classics as an alternative and as a means to surmount the problem—the limitless number of past writings—he detects in that claim. Hence my remarks so far. Most strangely, however, when Jones formulates his alternative explicitly, it sounds scarcely different from my own position. All he says is that "classic texts . . . provide . . . one potential answer to the unavoidable question of where our historical inquiries ought to begin." This comment is so carefully worded that it invites the dual inference that in Jones's view: (a) classics are not the only place for historical inquiries to begin; and (b) once begun, such inquiries accord no particular primacy to the classics. But does my contention that historians of sociology should not limit themselves to the classics at all imply the absurd argument that classics cannot be one potential starting point for their inquiries? Except for a minor matter of emphasis, the only difference here—and this is not so minor—between Jones and myself appears to be the following. The argument I have advanced, as should now be clear, does not seek to deny that, in moving beyond the classics, one potentially confronts limitless possibilities for research. Recognizing this, the argument proceeds to indicate that this theoretical problem is mitigated in practice because any particular historical investigator operates with specific empirical questions that (in conjunction with certain other variables) aid in delimiting concrete arenas for research. Jones, however, by placing the classics in the foreground, evades the fact that as soon as one pursues the dual implication of his precise formulation—seeking to identify either the other answers to the question of where historical inquiries ought to begin or the primary focus of such inquiries once they have begun—one immediately confronts the limitless possibilities that trouble him, but, paradoxically, without the pragmatic means of surmounting these that I have proposed.⁶

For the record, one last comment is necessary. Jones observes that I am not a "contextualist" and have not understood that his methodology demands contextualism, or the analysis of "the larger context of ideological discourse" from which classic texts emerge. On the former count, Jones is right, but not for the latter reason.⁷ In my view (formally developed elsewhere [see Camic 1979, pp. 165–68]), the contextualism of Jones, his men-

⁶ Unless his vague reference in his final sentence to "asking very specific questions" is construed as suggesting, as I have, the importance in this connection of sociological research questions. If this is the case, his position is even closer to mine.

⁷ The fact that my article not only cites but draws approvingly from contextual approaches (esp. in the work of Forbes) to certain of the utilitarians should have intimated to Jones that I am hardly innocent of what his brand of contextualism is and does. In fact, I have taken work on past social theory to task for not adequately employing contextual approaches of this kind (see Camic 1980).

Review Essay: States and Revolutions: The Implications and Limits of Skocpol's Structural Model

States and Social Revolutions: A Comparative Analysis of France, Russia, and China. By Theda Skocpol. New York: Cambridge University Press, 1979. Pp. xvii+407. \$29.50 (cloth); \$7.95 (paper).

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Theda Skocpol is perhaps the most ambitious and exciting of a new generation of historical-comparative sociologists who have focused their attention squarely on the big issues of social change that once preoccupied the classic sociologists. Her recent work, *States and Social Revolutions*, is an impressive examination of the causes and consequences of social revolution in France, Russia, and China. On the basis of these three cases, she develops a general structural model of social revolution and issues a strong challenge to existing perspectives on revolution, especially the Marxian one.

In this essay, we shall examine Skocpol's structural model of social revolution. After sketching the essential features of her argument, we shall suggest some of its wider implications, especially how completely it requires us to rethink the nature of revolution. Finally, we shall examine some of the theoretical and methodological problems evident in *States and Social Revolutions*—problems that are less those of the author than those of the structural approach itself.

SKOCPOL'S ARGUMENT

Social revolutions, Skocpol writes, are "rapid, basic transformations of a society's state and class structures; and they are accompanied and in part carried through by class-based revolts from below" (p. 4). There are two critical elements in this definition. First, the transformation must be both social and political; social revolutions are distinct from rebellions, revolts, and political revolutions in that social revolutions require a successful transformation not only of the polity but also of the social bases of political power. Second, this transformation must include a popular uprising; social revolution involves more than change engineered from above by an elite.

Skocpol argues that social revolutions are best understood from what she calls a "structural perspective," one which stresses "objective relationships and conflicts among variously situated groups and nations, rather than the interests, outlooks, or ideologies of particular actors in revolutions" (p. 291). The essential elements in her analysis thus involve relationships (among classes, among states, and between classes and states) and exist independent of the consciousness of the individuals who enter into these relationships.

Skocpol identifies three social revolutions, those occurring in late 18th-century France, early 20th-century Russia, and China in the first half of the 20th century. She examines each of these three cases within a comparative framework, noting, on the one hand, their similarities with each other and, on the other hand, their differences from selected cases of non-revolutionary change or failed revolutionary attempts—England in the 17th century (a political revolution), Prussia in the early 1800s and Japan in the late 1860s (transformations from above), and Germany in 1848 and Russia in 1905 (failed revolutions).

In so doing, Skocpol identifies three stages of revolution: the collapse of the old-regime state, mass mobilization of the peasantry into class-based uprisings, and the reconsolidation of state power by a new elite. At each stage, specific structural factors directly determine social outcomes.

The Collapse of the Old-Regime State

Revolutions begin with the weakening or outright collapse of the existing state structure. The state collapses not because of revolutionary action from within (this follows rather than precedes state collapse), but because of several structural pressures. Revolutionary political crises have occurred, according to Skocpol, in relatively weak states faced with military or economic competition with stronger states in the context of unevenly developed international economic and political systems. Confronted with military collapse and/or fiscal crisis, the state seeks to strengthen itself through relevant reforms, such as ending the tax privileges of the upper classes or acquiring direct control over the agricultural surplus. How successful these reform efforts are depends in turn on two other structural relationships. The first is the role played by the landed upper class in national government. If the upper class maintains a powerful collective, autonomous presence (e.g., through bodies like the Parlement of Paris), it can block reforms successfully, and the resulting conflict between the upper class and the state can immobilize the repressive and administrative apparatus. If the upper class is either uninvolved in national politics or integrated into it in a way that makes it wholly dependent on the state, it cannot block reforms. The second structural relationship has to do with whether or not agricultural class relations allow for high productivity. The higher the potential productivity, the better the state's chances of weathering its crisis.

If international tensions are modest, if the landed upper class is politically weak, and if agricultural relations are suitable for high productivity, the state can enact reforms from above and overcome its crisis (e.g., Prussia in the early 1800s and Japan in the 1860s). If the crisis is severe (Russia in 1917), if the landed upper class is strong on the national level (France and China), and/or if agricultural relations are resistant to increases in productivity (France, Russia, and China), the state cannot resolve its crisis. A combination of state crisis and upper-class insurrection immobilizes the state and opens the way for mass uprisings.

Mass Peasant Uprisings

Political crisis is necessary for revolution, but it is not sufficient. Social revolutions arise from a conjuncture of political crisis and widespread peasant revolt aimed at the landed upper class. The potential for peasant revolt in turn is determined by another set of structural factors—the degree of peasant autonomy and solidarity and the local strength of the upper class. Peasant uprisings are likely (a) when the peasant community is strong and peasants have some economic and political autonomy and (b) when landlords lack direct economic and political control at the local level.

In both France and Russia, peasants owned a substantial proportion of the land and lived in cohesive, self-governing communities, while the landed upper class did not directly control either production or the local administrative and coercive apparatus. The collapse of the state thus left a peasantry capable of organized insurrection against a landed upper class incapable of stopping them. In China, where peasant community was weak and local landlord power strong, the collapse of the Manchu state did not lead directly to a successful peasant uprising, which occurred only decades later when the peasants had been organized and local landlord power broken by the Chinese Communist Party (CCP).

Reconsolidation of State Power

Once the state has been weakened and peasant uprisings have occurred, the main activity of revolution is the reconsolidation of state power by a previously marginal political elite. The general outcome of revolution is the creation of a stronger, more bureaucratic, more highly centralized state. The way state power is reconsolidated, as well as the fate of would-be political leaders, depends on the structural opportunities available in each case.

The relatively developed industry of Russia provided an urban base for the reconsolidation of power not available in either France or China. The lack of indigenous peasant organization in China provided a rural base not available in either France or Russia. Thus the Bolsheviks rooted themselves successfully in the cities, the CCP in the countryside, and the Jacobins in neither.

This quick summary hardly does justice to the beauty and complexity of Skocpol's argument, but we hope we have demonstrated its resolute structuralism and its systematic effort to avoid recourse to voluntarist factors. It is an impressive argument, and, as we shall see, it presents a stiff challenge to the conventional Marxian theory of revolution.

A NEW PERSPECTIVE ON REVOLUTION

The main fruit of Skocpol's work is a new way of looking at revolution. This new perspective in effect has three dimensions: (1) a distinctive set of general principles for studying revolution, which logically imply (2) a critique of Marx's theory of revolution, which in turn provides the basis for (3) a drastic rethinking of the nature of revolution in the contemporary advanced capitalist world. Let us examine each of these.

General Principles

As Skocpol notes, "three major principles of analysis" underlie *States and Social Revolutions*—first, a "nonvoluntarist, structural perspective" on the causes and processes of revolution; second, a "systematic reference to international structures and world-historical developments"; third, a conception of states "as administrative and coercive organizations . . . that are potentially autonomous from (though conditioned by) socioeconomic interests and structures" (p. 14).

These principles are formulated in self-conscious opposition to four prevailing models of revolution: the Marxian theory of self-conscious movements rooted in class conflict, the "political conflict" theory of Charles Tilly, the "aggregate psychological" approach of Ted Gurr, and the Parsonian "system-value consensus" model of Chalmers Johnson. All of these models, according to Skocpol, overemphasize voluntarist elements in revolution, ignore the international context, and undervalue the autonomy of the state. She aims these criticisms at the Marxian model in particular; and by examining her three general principles, we can see how her critique develops.

The structural perspective.—As we have already shown, Skocpol works within a structural perspective. In emphasizing this approach, she rejects what she calls "voluntarism"—the idea that the causes, process, and outcomes of revolutions can be understood wholly or primarily in terms of the values, purposes, and deliberate efforts of self-consciously revolutionary classes and organizations. She has three telling arguments against such an analysis. First, revolutions are not made; they happen. That is, revolutions are not *begun* by revolutionary vanguards, with or without an organized, mobilized, and ideologically imbued mass following. They arise from certain structural conditions—a political-military state crisis in conjunction with certain relationships between peasantry and landed upper class

and between landed upper class and the state. Second, actors in a revolutionary situation are not always clear about what they want; their motives are often complex and ill-formulated. Third, even when some revolutionary actors have clear aims, the outcomes of revolution are often at variance with those aims: although revolutionary groups in all three revolutions studied by Skocpol sought to reduce or limit state power, each revolution in fact created a new, more centralized, more powerful state.

All three of these arguments make a great deal of sense, and they clearly point to the conclusion that an analysis of ideology and organized, self-conscious social action is not sufficient for understanding revolution. Skocpol, however, goes further in her actual analysis: in effect, she argues that these "voluntarist" elements are not even necessary for understanding revolution. Consciousness and conscious social action thus play an ambiguous role in her analysis: they are there, but she does her best to ignore them.

Transnational relations/world-historical developments.—Revolutions, Skocpol argues, do not arise simply from contradictions and conflicts internal to a society. What she calls "world-historical developments" and "transnational relations" are also crucial. The path of revolution is shaped partly by the opportunities and requirements of the historical moment. Thus the Chinese Revolution had the example of the Bolshevik Revolution upon which to draw, while the Bolsheviks obviously did not. Revolutions are also shaped by relations between states. Skocpol argues that underlying all social revolutions is the "internationally uneven spread of capitalist economic development and nation-state formation" (p. 19). Revolutions occur "only in countries situated in disadvantaged positions within international arenas" (p. 23).

We should make clear immediately that this is not a world-system argument of the kind advanced by Immanuel Wallerstein. Skocpol is not saying simply that revolutions occur in countries on the periphery or semiperiphery of a world-capitalist system. Her position is more complex. Following Hintze, she holds that there are two international systems, a capitalist economic system and a nation-state system, neither of which is wholly reducible to the other. Indeed, she argues, "Although uneven economic development always lies in the background, developments within the international states system as such . . . have directly contributed to virtually all outbreaks of revolutionary crises" (p. 23). So the French revolutionary crisis was precipitated by decades of political-military conflict with England, the Chinese by Western imperialist intrusion and defeat by Japan, and the Russian by World War I.

The autonomy of the political.—Skocpol's emphasis on the independent role of the international states system leads naturally to her third general principle. She objects to the view that political structures and struggles can be reduced to socioeconomic forces and conflicts or that the state is simply an arena in which social conflicts are fought. Instead, she argues, the state must be seen as an *autonomous* structure with its own logic and interests and the power of the state treated as a crucial independent factor in revolution.

This notion of the autonomous state should be distinguished from the Marxian notion of the *relative* autonomy of the state. From the latter perspective, first articulated by Marx in *The Eighteenth Brumaire* and taken up by more recent Marxist theorists, the state is autonomous of direct dominant class control, but for that very reason is all the more able to preserve the existing mode of production and thus protect the interests of the dominant class. The state is thus still primarily understood in terms of the class interests it serves. Skocpol, in contrast, sees the state primarily as a separate institution, though with important relations to the socioeconomic structure.

The state appears as the main figure in Skocpol's account of revolution; and the origins, process, and results of revolution are portrayed largely in political terms. Revolutions begin with a breakdown of the state apparatus, which is shaped not by class struggle, but by relations between the state and both the dominant class and other states; the revolutionary process involves the building of new state forms, an effort in which class struggles are important but not central; revolutions end with the consolidation of new state power. This last point is particularly important. For Skocpol, the central result of revolution is not a more advanced mode of production, but a more centralized, mass-incorporating, bureaucratic state.

As Skocpol notes, her emphasis on the autonomy of the state goes well beyond virtually all Marxist theory (pp. 27–28). Her focus on the relations between the state and the various social classes, moreover, separates her from Max Weber, whose main concern is relations within the state between rulers and their staffs (p. 304). Finally, her stress on the uniformity of both the causes and the outcomes of all three revolutions goes against the neo-Marxist tendency of Barrington Moore, dependency theorists, and others to differentiate several paths to modern industrial society. The categories of “early,” “late,” and “dependent” development thus have no place in Skocpol's analysis—as France (“early”), Russia (“late”), and China (“dependent” in a sense) manifest to her eyes similarities rather than differences.

Her approach is not without roots, however, though they are scarcely acknowledged. Hovering just out of sight at every point in her argument is the spirit of Alexis de Tocqueville. Indeed, one might say that Skocpol is a closet Tocquevillian and that she achieves the long overdue introduction of Tocquevillian insights into a general Marxian framework. Like Tocqueville, Skocpol is concerned with the relations between the state as an autonomous institution and society. Like Tocqueville, she sees growing political centralization and autonomy as *the* consequence of revolution. (Indeed, her general assessment of the French Revolution follows that of *The Old Regime and the French Revolution*.) Like Tocqueville too, though in a less obvious way, she presents the irony of revolutions made in the name of freedom but culminating in a much strengthened state.

In bringing Tocqueville in, Skocpol of course has not ushered Marx out. The two are hardly incompatible, and Skocpol does a good job of com-

binning a Tocquevillian approach to the state with a Marxian conception of "class relations as rooted in the control of productive property and the appropriation of economic surpluses from direct producers by nonproducers" (p. 13).

Challenging Marx

At the same time, however, Skocpol does challenge the specific Marxian theory of revolution, and her own account of revolution is at odds with it at every major point. There is, she argues, a "lack of fit between Marx's theory of revolutions and the actual historical pattern of social revolutions" (p. 292).

Skocpol's very selection of cases crosscuts Marxian categories: the French case, a bourgeois revolution in Marxian terms, is lumped together with socialist revolutions in Russia and China. Her intellectual journey, as she notes (p. xiii), goes in a direction opposite to that of conventional Marxism—in effect interpreting the Russian Revolution in terms of the Chinese and the French in terms of the Russian, rather than the other way around. She thus begins—and stays—with a very different image of revolution: not "western bourgeois revolution," but "agrarian bureaucratic revolution."

Thus it is not surprising that most of the categories central to the conventional Marxian analysis of revolution are absent from Skocpol's book. Revolutions, according to her analysis, do not arise from a contradiction between the forces and relations of production within a society. They arise from a state crisis conditioned by the state's position in international political and economic systems and by its relation to the dominant social class.

From the Marxian standpoint, revolutions are made by more or less self-conscious rising social classes—the bourgeoisie in the case of capitalist revolutions and the proletariat in the case of socialist revolutions—against the dominant classes and the preexisting mode of production. In Skocpol's analysis, nothing of the sort happens. Not only are rising classes not the major agent of revolution, but also they sometimes do not exist as distinct social classes at all (e.g., the bourgeoisie in France in 1789). Instead of rising classes, we find peasant uprisings, conflict between the landed upper class and the state, and marginal elite movements.

The Marxian perspective on revolution generally gives some role to a "vanguard party" in making socialist revolutions—whether this role be the relatively ambiguous one of the *Communist Manifesto* or the quite clear one of Lenin's work. In contrast, "vanguard parties" do not appear in Skocpol's analysis at all; instead we find "marginal elites" struggling for state power. The difference is important: To call a political group a "vanguard party" implies that it has a natural or unproblematic relation to a revolutionary social class. To call it a "marginal elite" suggests that this relation is less immediate, more problematic. Marginal elites may mobilize particular social classes in their efforts to consolidate state power,

but they are not inherently the representatives or partisans of those classes. Vanguard parties are understood in terms of the class interests and the wider social goals they pursue; marginal elites in terms of their immediate activity, consolidating state power. In short, although "vanguard party" and "marginal elite" may be used to refer to the same political group, the latter term implies a more critical approach to that group's goals and social base.

Finally, the "withering away of the state" is central to the Marxian concept of socialist revolution. Since the state is the "organized power of one class for oppressing another" (Marx's phrase), the abolition of classes necessarily means the gradual disappearance of the state. In Skocpol's analysis, far from withering away, the state flourishes after a revolution—whether capitalist or socialist.

Rethinking Revolution

If we take Skocpol's challenge to Marx seriously, we must wholly rethink our approach to revolution. Marx's model allowed us to draw analogies between the bourgeois revolutions of the past and the socialist revolution of the future. Just as capitalism emerged from a contradiction between productive forces and feudal social relations, so socialism would emerge from a contradiction between productive forces and capitalist social relations. Just as capitalism was built by a rising bourgeoisie, so socialism would be built by a rising proletariat. As Marx remarked in the *Communist Manifesto*, the two processes involved a "similar movement." Skocpol's model of revolution, in contrast, destroys any possibility of such analogies: the revolutions of the past simply do not provide relevant models for revolution in advanced capitalist societies.

Unlike the old-regime states studied by Skocpol, the advanced capitalist state is hardly in a disadvantaged position in international economic and political arenas. Previous revolutions, therefore, do not provide any direct model for the way in which the initial state crisis could develop. At the same time, if the advanced capitalist state is relatively autonomous, as current theories of the state claim, the upper class should be unable to block the state from enacting the reforms necessary for weathering its crisis. Hence previous revolutions do not provide a model for exacerbation of the initial state crisis by successful internal resistance. Advanced capitalist societies also lack any producing class with the economic and political autonomy of the French and Russian peasantries. Hence previous revolutions do not provide a model for the structural conditions that might generate a mass uprising and turn an exacerbated state crisis into a social revolution.

Most important, the revolutions of the past resulted in the rise of strong, bureaucratic, centralized states, their outcomes in this respect directly contradicting the ideals of the revolutionaries. Thus they do not provide a model for transformation of the strong, bureaucratic, centralized states

already existing in advanced capitalist societies. Instead, they provide a cautionary tale, an admonition.

None of this implies that major progressive social change is impossible in the advanced capitalist societies. It merely suggests that such change must be of a quite different nature from the revolutions of the past and, hence, that the past cannot provide much of a positive model. Skocpol in effect urges us to stop aping the past and get on with transforming the present. Marx once said that "the social revolution of the nineteenth century cannot draw its poetry from the past but only from the future." Skocpol's work argues strongly that the same is true of the social revolutions of the 20th century.

THE LIMITS OF A STRUCTURAL PERSPECTIVE

Skocpol has much to tell us about revolution, and the implications of her work are both stimulating and unsettling. There is, however, one striking gap in her approach, which is rooted in her general analytic principles and which in turn underlies several of the substantive problems in her work: although she has a great deal to say about the causes and consequences of revolution (and the correlations between them), she devotes scant space to the process of revolution, to how human beings actually make a revolution.

This omission arises directly from her insistence on a structural perspective with no admixture of voluntarism. Because of her uncompromising stand against voluntarism, Skocpol forgets that human beings thinking and acting (however haphazardly) are the mediating link between structural conditions and social outcomes. Structural conditions, moreover, do not dictate absolutely what humans do; they merely place certain limits on human action or define a certain range of possibilities. Structural conditions may define the possibilities for mass uprisings or the options available for consolidating state power in a revolutionary situation, but they do not fully explain how particular groups actually act, what options they pursue, or what possibilities they realize.

In short, even if Skocpol is correct about the relationships between structural conditions and revolutionary outcomes, her analysis is incomplete. She ignores the mediating factors—human consciousness and action—that are always part of the story and sometimes crucial to it. Or more precisely, she simply assumes that the appropriate actors are always there, waiting to perform the role required by structural conditions—peasants ever ready to make massive uprisings and marginal elites ever able to reconsolidate power. She rarely regards their response to structural conditions as problematic, and thus she systematically undervalues the role of ideology, political organization, and self-conscious social action.

This gap in her analysis manifests itself at various points in her argument, but it is especially crucial in two instances—the peasant uprising in China and the role of the Bolsheviks in Russia. Skocpol argues that the

degree of peasant autonomy/community and the extent of local landlord power determine the potential for peasant uprisings, but she leaves little room in her analysis for the ideological and organizational factors that actualize this potential. The omission undoubtedly mars her analysis of all three revolutions, but it is particularly glaring in the case of China. Structural conditions there were *not* wholly ripe for a peasant uprising, which did not occur until peasants had been organized by the Chinese Communist Party. In other words, the self-conscious action of an organized revolutionary group was crucial—a fact Skocpol discusses but whose implications for her structural perspective she ignores. If the CCP played a central role in the peasant uprisings, however, we must ask how they turned the historical trick. Through what combination of ideology and organization did they create a mass rising in unpromising structural conditions? The very “voluntarist” factors that Skocpol seeks to play down become quite important.

Skocpol also argues that structural opportunities determine how state power is reconsolidated after the revolution. She seems to assume, however, that given a set of structural conditions, the reconsolidation is more or less automatic. The case of Russia in particular is problematic in this regard and raises questions that force us beyond a structural perspective. Granted that the developed urban industrial world of tsarist Russia created a potential base for the reconsolidation of power that was available to neither the Jacobins in France nor the CCP in China, how did the Bolsheviks manage to take advantage of the opportunity? We cannot simply assume that a group capable of doing so just happened to exist. What ideological and organizational resources allowed the Bolsheviks to survive through all those lean years prior to 1917 and to capitalize on the possibilities created by the conditions in Russia? Successful realization of these possibilities required a distinctive ideology—one that justified the importance of immediate revolutionary action from an urban base. A group whose ideology stressed waiting for the fuller development of capitalism or placed an emphasis on the peasantry as the revolutionary class simply would not have had the proper orientation for reconsolidating power successfully. In other words, in the Russian Revolution, the specific ideology of the Bolsheviks was an important factor in its own right in determining the outcome.

This critique of Skocpol's structural perspective does not reject her essentially valid claim that a voluntarist analysis by itself is insufficient to explain revolution (or any other social phenomenon). *States and Social Revolutions* goes too far, however, in assuming as well that voluntarist factors are unnecessary to a complete analysis of revolution. Skocpol treats structural analysis and voluntarist analysis as mutually exclusive opposites, rather than as two necessary elements of a complete sociological explanation. She thus loses the delicate synthesis that Marx struggled to achieve—“the coincidence of the changing of circumstances and of human activity.” To reverse the emphasis of the Marxian dictum: human beings do not make history “under circumstances chosen by themselves,” but they *do* “make their own history.”

Book Reviews

Work, Mobility, and Participation: A Comparative Study of American and Japanese Industry. By Robert E. Cole. Berkeley and Los Angeles: University of California Press, 1979. Pp. ix+293. \$17.75.

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When W. S. Gilbert asserts that it is possible to give "the very model of a modern major general," he poses a central problem for the social sciences. Clearly the reason an Englishman recognizes the portrait the major general gives of himself is that a model major general has some "traditional" public school virtues: he knows the kings of England and he quotes the fights historical from Marathon to Waterloo, in order categorical. But it would not be satire unless everyone knew there was a realistic wartime environment in which a major general who was a battle leader, a brave man, and a soldier would be effective. Going across cultures we are not surprised when Tolstoy comes up with a different model major general, who certainly sounds more Russian, and we have a vague notion from Japanese movies that a traditional model major general in Japan grits his teeth to look fierce. I take these examples from mythology and fiction for much the same reason Lévi-Strauss does: fiction and myths are sufficiently detached from the mundane realities of actually being a major general that we can see the cultural models working alone, by their own aesthetic laws.

But when we turn with Bendix or Dore or Cole to the problem of the very model of a modern industrial worker, we cannot avoid the mundane and contingent realities. The most successful enterprises of cultural interpretation, such as Clifford Geertz's brilliant interpretation of the betting odds in Balinese cockfights (in "Deep Play," in *The Interpretation of Cultures: Selected Essays* [New York: Basic Books, 1973]), tend to be of settings detached from the pressures of daily life. When Geertz turns to more mundane questions, such as how much rice Javanese eat (in *Agricultural Involution* [Berkeley and Los Angeles: University of California Press, 1963]), the power of the colonial Dutch and the fact the Dutch could sell sugar but not rice in Europe become more important than patterns of nested communal opposition. Likewise, Japan's model of a modern industrial worker has to produce automobiles, or the cultural model will change. Like the neat kinship systems that need adjustment to reality whenever the cross-cousin a male ego was supposed to marry is already past menopause, so cultural models of industrial workers have to meet marketplace tests, or they need adjustment.

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But the industrial relations systems of different countries or regions within countries are in fact very different. One is convinced immediately that the difference between industrial relations in England versus South Africa, or the American North versus the South, has something to do with contrasting cultural models which judge political action of industrial workers as "freeborn Englishmen" in one case, "uppity niggers" in the other.

The first problem in interpreting the cultural bases of Japanese labor relations is therefore to get the question right. If one conceives the question as, "Was there a straightforward transfer of feudal labor relations traditions to the permanent employment tradition of the modern factory?" then Koji Taira has provided the evidence for the answer ("Characteristics of Japanese Labor Markets," *Economic Development and Cultural Change* 10 [January 1962]: 150-68). Labor relations in Japanese cities around the turn of the century were more Ricardian than in English cities in Ricardo's day, and permanent employment or paternalistic care for a member of the corporate family was nowhere to be seen. But then are the peculiarities of permanent employment for males in large firms simply a historical sport, a feature of different equilibria in the labor market in Japan after World War II? Is there nothing of old Japan, of Japanese cultural models of the ideal worker and the ideal boss, in the modern system? Clearly the conclusion that "tradition" has disappeared is much more sweeping than what has been demonstrated by Taira's high turnover rates and complete lack of family-oriented fringe benefits in turn-of-the-century Japanese industry.

If one asks the question, "Is today just like the old days?" the answer is always no. Tradition works through people, and people combine tradition with the latest fashions when they make up their minds and adjust both to what they believe reality to be. It seems that each generation of social scientists has to be taught anew that traditions are things *made use of* by people to figure out what to do about the main problems of their lives. The symbolic interactionists and John Dewey taught it to one generation. Parsons repeated it as the major discovery of classical sociology in the thirties, the ordinary language school in philosophy rediscovered it in the forties, and according to our tastes we can now relearn it from Goffman, Geertz, Lévi-Strauss, or Braudel. Apparently the "natural" human view of causation is not one that fits very well the way culture really works—as an influence operating as a means, through a "people-to-make-use-of" mechanism.

This is a long roundabout way to justify the assertion that Robert Cole's first chapter disposes of a lot more rubbish of useless ideas inherited from Abegglen than is apparent on the surface. *Work, Mobility, and Participation* is on the right question—how did Japanese people, particularly managers, make use of their cultural tradition (and borrowings from Western social science) to construct their model of a modern industrial worker, and how was the operation of that model checked and tempered by the economic and political realities of Japan? Getting the question right in the first chapter is, after all, not that common in sociological monographs.

The behavioral fact to be explained is that, although the structural pat-

tern of intragenerational mobility is the same in Japan as in the United States, the volume of job changing is about half as much in Japan. Cole's sample includes only males. Since women are uniformly excluded from the benefits of the permanent employment system in Japan, this omission of women should exaggerate the differences. I thought that the fashion of excluding women from mobility studies was out, but perhaps for data collected in 1970 we can excuse it. In particular, not only is there only half as much job changing *between* firms; there is also only about half as much job changing *within* firms. Internal labor markets in Japan have only half as much promotion and job transfer. An excellently conceived study comparing nearly all job changes in a sample of male workers in Detroit and Yokohama gives many details of this similar structure but different volume. Unfortunately the Michigan Survey Research Center's tradition of omitting data on short-term jobs makes it impossible to compare volume of job changing exactly and makes it very hard to fit models of job mobility properly. This is a clear case of convenience in interviewing causing great difficulty in analysis, and Michigan is choosing the wrong trade-off. Given that limitation, the data are excellently analyzed in two chapters coauthored by Paul Siegel.

The explanatory problem this poses is to specify the causes that lead Japanese employers to try to retain (male) workers by extensive educational, career development, and company housing and recreation programs, which lead workers to stay on the same job for twice as long (on the average) as American workers (the "permanent" in "permanent employment" is not, after all, forever), and which lead to low transfer and promotion rates within firms. Much of Cole's evidence for the explanation comes from his field studies in Toyota Auto Body and from the labor relations literature in Japan. The personnel practices again apply only to men and are more common in the large-firm sector.

Cole sets out to explain the low rate of promotion and transfer within firms by a different Japanese definition of what a job is. The career of an auto worker in Japan is much like that of a professor in America: "job tasks" (as Cole insists on calling "tasks") are added to and taken away from a person's role as suits the organizational convenience, while promotions are by ability and seniority and are not really connected to tasks—a professor does the same tasks as an assistant professor. We do not appoint a professor of second-year Latin (some South American universities used to do that); so also Japanese automobile plants do not appoint people to a particular machine. People move among machines without "changing jobs," just as professors move among classes without changing job titles each term. A professor's mobility interview would show the same job throughout his life, perhaps, while its content completely changed and his salary doubled; Cole argues that essentially the same thing happens with promotions and transfers in Japanese factories. Cole's argument on this is very convincing, and he ends up making the American system of super-specific jobs sound odd.

Cole explains the low mobility between firms as the result of three main

factors. First, the overwhelming power of Japanese management, challenged only by weak unions and supported by a conservative government, means that most of the increased productivity obtained by investments in the labor force can be appropriated by the capitalists. Second, Japanese managers believe that the workers have substantial creative contributions to make to productivity and invest substantial sums of money to treat workers as humans capable of contribution. Third, wage rates and security in the lower part of the dual labor market in Japan are much worse, compared with the advantaged large-firm sector, than in the United States, and chances for good jobs in the large-firm sector are available only to young workers, so a worker with a good job cannot afford to quit.

Making this system run, Cole argues, requires a high rate of economic growth, particularly in the large-firm sector. For example, the Toyota company produced 16 cars per person year of employee time in 1961, 30 cars per employee year in 1968 (computed from p. 158). Such a near doubling of productivity provides a cushion of employee rewards—to distribute as wage increases or to pay for retention of employees during a downturn or to pay personnel department employees (roughly twice as many per thousand workers as in the United States) to help workers plan their careers as auto workers—and still leaves a lot to pay stockholders and bankers who provide capital. Ronald Dore also has to attribute the much higher gains of the Hitachi unions (10%–18% per year) as compared with English Electric unions (2%–5% per year) to greater productivity increases (*British Factory, Japanese Factory* [Berkeley and Los Angeles: University of California Press, 1973], p. 267).

As is typical of explanations of cross-cultural differences, there are more relevant causes than there are differences between cases to be explained. I am left with the uncomfortable feeling that I still do not really understand why Japanese factory workers stay on the job. I do not think I could stand working in Toyota Auto Body.

Assessing Organizational Change: The Rushton Quality of Work Experiment. By Paul S. Goodman. With major contributions by Edward Conlon, Dennis Eppler, and Eduard Fidler. New York: John Wiley & Sons, 1979. Pp. xii+391. \$22.95.

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As the recent debate over the level of funding for social science research from the National Science Foundation reaffirms, there is considerable doubt that social science can help improve the quality of life. But if the social sciences have had a disappointing record in designing innovation, they have certainly excelled in measuring its impact. A complex technology stands

ready to monitor the effects, intended or otherwise, of any attempt to improve upon the good life.

Assessing Organizational Change falls squarely into this positivist tradition of the measurement of innovation. The organizational change in question refers to what took place between 1973 and 1977 200 feet underground in a small "room and pillar" coal mine in north-central Pennsylvania. Largely through the efforts of Eric Trist, a founding member of the Tavistock Institute, the presidents of the Rushton Mining Company and the United Mine Workers Union signed an agreement to conduct a "quality of work" experiment. Funds were made available to hire Trist to head a team of consultants, and, independently, to support an evaluation of the experiment by Paul Goodman of Carnegie-Mellon.

Goodman's book begins by describing the events that led to the experiment, the changes made in one section during the first year, the clumsy attempts to extend these changes to the entire mine, the union's rejection of the proposed extension, and management's taking over the experiment as an administrative prerogative. The major part of the book reports the results of four years of assiduous evaluation research and includes: repeated attitude surveys, multivariate analysis of productivity and safety improvements, an assessment of the effects of the experiment on job skills and union-management relations, and a tactfully presented cost-benefit analysis. Goodman's method of measuring productivity improvements by fitting production-function models makes a definite contribution to the evaluation field. In the final section, Goodman interprets the significance of his findings for designing, implementing, and sustaining quality of work experiments.

Taken together, the results indicate that the experiment had a modestly positive effect on the dependent measures, but much less so than reported earlier by Trist and his colleagues. But having digested, with Goodman's competent help, an enormous quantity of data the reader is left uneasy: from the very beginning, Goodman warns us that asking whether the Rushton experiment was successful or not is the wrong question. He tells us to look instead for "why" things happened as they did. But for some reason he has trouble following his own advice; the focus of this book is incontestably on the measurement of effects.

Goodman's resolve to provide an objective accounting of what happened at Rushton appears to have originated from a fairly subjective position. On the first page he casually describes the Rushton experiment as part of a "revolution in new forms of work organization." This is presumably because the quality of work project was supposed to promote a "problem-solving" as opposed to an "adversary" relationship between labor and management. But such language may serve only to confound a particular style of conflict with its basic formal properties. Contracts are peacefully negotiated and grievances quietly settled every day in situations where there are genuinely polarized positions held by two parties. Adversaries can and do solve problems.

Had Goodman made his initial assumption the central problem of his study, that is, had he asked, "What, if anything, is innovative about Rushton?" the entire evaluation (if not the experiment itself) might have turned out differently. To ask what was innovative about Rushton is to attempt to penetrate the mind of Eric Trist.

Trist approached Rushton as he had British longwall coal mines 20 years earlier. There he attempted to show that if mining crews could work without interference from management—the so-called autonomous work group—they would be saved from one source of indignity and be more productive as well. His rationale for this has come to be known as the theory of sociotechnical systems. The theory maintains that all work organizations contain two separate but interdependent systems: one technical, the other social. But behind Trist's two-systems notion lies an astute attempt to establish a scientific basis for a certain type of industrial morality. By conceptualizing separate systems, Trist is able to stake out a claim for democracy in otherwise profit-minded organizations. Is this innovative? Western social science has long attempted to forge a link between the ideal of democracy and the ideal of efficiency. At root Trist's vision is no different from Elton Mayo's.

The experiment at Rushton is an example of the use of social science to legitimate a democratic morality in the workplace. Ironically, Trist himself likely contributed heavily to its failure. In his determination to eliminate the presence of coercive power at the workplace, he failed to exploit the potential of the quality of work experiment to bring together workers and managers in a way that would facilitate the expression of polarized values and, perhaps, the identification of new, less polarized positions.

It is interesting to note that nowhere in the 400 pages of *Assessing Organizational Change* does the word "democracy" appear. Of course, Goodman's book is addressed to an audience with very practical concerns, and his own criticism of Trist's model, neatly tucked away on a page and a half at the end of the book, is that both the technical and the social systems are subordinate to the economic system. But my purpose in calling this book to the attention of sociologists is not to pander to social evaluation technology but to suggest that, in the story of Rushton, as in the story of Hawthorne before it, there is ample evidence of the power of social science to change the way we live. Whether these innovations, if they are that, will actually improve the quality of life will depend more on the quality of the theory that underlies them than on the quality of the techniques used to measure the outcomes.

Good Jobs, Bad Jobs, No Jobs. By Eli Ginzberg. Cambridge, Mass.: Harvard University Press, 1979. Pp. x+219. \$15.00.

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Eli Ginzberg's latest book about the state of manpower problems and policies in the United States is a multifaceted work that tries to be at times history, at times policy analysis, and at times a study in politics. But through all these alternations, the reader is impressed with the constancy of Ginzberg's central message that, despite the technical and political obstacles, the goal of a decent job for every American who wants one should be part of our national culture. In articulating this concern, Ginzberg does not see himself as a prophet, bringing to the people secular wisdom arising from the musings of social theorists and philosophers. On the contrary, in his view the people have already grasped and accepted the message. His mission is to take it to the leaders of the nation's business, labor, educational, and governmental institutions. Through their efforts, he argues, progress can be made in fulfilling society's responsibility to provide more opportunities for the disadvantaged and a better fit between ambition and attainment for workers at all levels. Yet, while wide ranging, the book cannot be described as ambitious in matters other than scope. In attempting too much, in the end it accomplishes too little to be described as much more than an overview of the present state of employment policy and prospects.

Good Jobs, Bad Jobs, No Jobs is a collection of essays that have almost all appeared previously, either as articles in professional or general interest journals, or as addresses in one or another forum. All describe some aspect of manpower policy, but this theme is not capable of effecting more than a loose integration. The range of topics is enormous. Ginzberg discusses the problems of women, minorities, the elderly, and youth; he writes about unemployment in Britain and the United States, apprenticeships in Canada and the United States, the prospects for skill shortages in the foreseeable future, overeducation of the nation's youth, the problems young managers and professionals face as they begin careers, and the problems they create for the large organizations that hire them. He further includes essays on the history of manpower policy in the United States and its future prospects, the difficulties in carrying out these policies equitably, and more. In each case, he tries to propose solutions which are consistent with both his overarching goals and his sense of the difficulties involved in carrying out successful manpower policies in present-day America.

Like his perspectives on the direction manpower policies should take, his suggestions for how to move toward these goals are unified under certain themes. He advocates a cautious liberalism, a willingness to experiment with a variety of limited programs sponsored by both the public and private sectors. He is consistently skeptical of academic approaches to prob-

lems relevant to employment policy. His strongest criticisms are in fact reserved for economists ("I am not persuaded that, if these economists were given their way, conditions would improve. They might well deteriorate" [p. 199]), but the attitude seems to extend to social scientists in general. For guidance, he prefers a Lindblomian "intelligence of democracy": "The United States will debate employment policy in the political arena for a long time to come, and as a result of such debates, actions and feedback, we will slowly improve our understanding of both how to achieve the objectives of full employment and how to design politics that will advance us on the road to that goal without taking unnecessary risks" (p. 186).

While Ginzberg's orientation toward manpower policy is presented forcefully, and some of his policy suggestions are valuable, the book's limitations are apparent in much of the discussion within which his suggestions and exhortations are embedded. The treatments of his subject matter are too often superficial and seemingly ignorant of the work of other scholars (references in the book are sparse). Too often his commonsense approach to policy questions is simply inadequate. For example, he argues that management should try to keep channels of communication open between superiors and subordinates. While expressing his understanding that such a goal cannot be readily achieved ("This is no easy matter" [p. 183]), he goes on to suggest that the solution is a change in attitude on the part of management ("If those at the top are willing to open their ears, they will hear. If they are defensive, the information they need will not reach them" [p. 183]) and cites the consequences of personality differences of recent U.S. presidents as evidence for his assertion. Never does he raise the question of why management might be "defensive" or pay any attention to the work of sociologists who try to shed light on the difficulties in increasing communication within large organizations, especially when the communication concerns the sensitive matter of dissatisfaction. As a consequence, Ginzberg's suggestions appear naive, more like wishes than serious policy suggestions. Even in instances where his conclusions are more reasonable, as in his discussion of strategies to increase employment in cities, he makes only a token attempt to present possible political and technical pitfalls and to deal with them.

Clearly, Ginzberg's intended audience is not specialists but the educated public. If such readers were to raise questions about his conclusions, they would probably emerge from two basic concerns. The first might be described as the traditional American unease about government interventions into the labor market of the scope discussed by Ginzberg. Regarding this objection, he argues repeatedly that the intertwining of the public and private sectors has made these fears outmoded. The more timely objection would no doubt be that in a time of accelerating inflation, priority must be given to reducing expenditures, not increasing them. About this second objection, Ginzberg has disturbingly little to say. While this might be understandable in a book of more limited focus, it is difficult to accept in a book that is suggesting policies for the entire range of manpower issues. His failure to address the macropolitical and macroeconomic issues in a force-

ful manner is as disappointing as the cursory treatment he gives to many of the specific topics of the book. Nonetheless, the book does serve a useful purpose. It provides a good introduction to an important set of issues in contemporary American political and social life and an important point of view on the direction in which manpower policy in the country should be headed.

Measuring Underemployment: Demographic Indicators for the United States. By Clifford C. Clogg. New York: Academic Press, 1979. Pp. xiii+279. \$21.50.

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When a concept comes to be measured by official statistics it begins a life of its own and soon becomes inviolate. It took a long time for the GNP or the rate of unemployment to become established, but once in place they are noted by millions of people each year and each month. A government of the United States could cut the dollar loose from gold and so make possible our current inflation; it could not with so little public attention cease to publish the monthly unemployment rate based on the Current Population Survey. The defects of such measurements are acknowledged, even by those who use them most, and it is a mark of sophistication to tip one's hat to the critics. Nonetheless the measurement becomes an entity as clear-cut and durable as a highway or a tall building, a permanent part of the social landscape.

It is the merit of Clifford Clogg's *Measuring Underemployment*, and the work by Hauser and Sullivan of which it is a continuation, that it not only seriously discusses the defects of the unemployment figures but also proposes and uses a clear improvement. It recognizes the gradient in employment, from the person who works 40 hours a week in a regular job to one who is wholly unemployed, and notes that to make the cut at the single point where it is made officially—the person stating that he or she is without a job and looking for work—is not as informative as we require. Aside from the people who meet the formal definition of unemployment, there are those who work part time but need or want full-time work, as well as those who work but have incomes below the poverty line, and those who cannot find suitable work and have taken jobs well below their level of skill. Particularly in the less developed countries, but even in the United States, the neglect of these categories leaves us with only the crudest of indications of time trends and differences among subgroups of workers and would-be workers.

Using the March Current Population Survey for the years 1969–73, Clogg finds that the variability of the three underemployment measures is less than that of the usual unemployment percentage. And the measure

for one group—those who are overqualified or mismatched—does not change with fluctuations of the economy but steadily increases for all four sex-race groups; this could be because it is defined in terms of years of schooling, and that is subject to secular increase. Unfortunately the five-year span (1969 through 1973) to which the book is confined is too short to establish definitively the relations among these measures. But what even so short a period does show is that the underemployment measures in the labor utilization framework are not mere replicates of the usual unemployment measure. They provide different information, with regard both to the time trend of labor utilization and to differentials by age, sex, family type, and region. The handicap of a too-short series is especially conspicuous in the discussion of forecasting and the tests of different methods in chapter 7.

Clogg arranges his data by cohorts and finds that cohorts do make a difference that is not to be attributed to time period or age variation. He is aware that measuring the cohort effect without bias is not possible with the kind of data here used, but he demonstrates that an effect is present.

The life-table model of the labor force is represented by a synthetic population that answers the question: what would the labor force be like if individuals passed through the ages of their lives with the same hazards as are shown by the cross section used? It turns out, for example, that the length of time that a white male aged 20 would expect to spend unemployed at 1973 rates is 1.3 years, and he would expect to spend 4.6 years in work below his qualifications (i.e., mismatched); for a nonwhite male the corresponding numbers are 1.9 and 2.7 years; unemployment is higher and over-qualification lower.

Of course, there are many other aspects of un- and underemployment that are not taken up in this book. It makes a great deal of difference how work is distributed, both over families and over time. If unemployment falls on certain families, affecting all of their members, that is different from its falling on individual members of families in which other members are working. And if it falls on all individuals each for 5 weeks per year, that is different from its falling on the same 10% of individuals year after year. But the labor utilization framework would be useful for analysis of the longitudinal data that could tell us about distribution among individuals.

To take account of underemployment as well as unemployment avoids one awkward boundary in labor force measurement. Yet there is another boundary that is important, not internal to the labor force but at its edge. Today a housewife is happily cooking for her family and is recorded as not in the labor force and therefore not eligible to be unemployed. Tomorrow she is still cooking for her family, but the idea of going out and taking a job has entered her mind, and she has written a letter to a possible employer. By writing that letter, irrespective of any outcome, she has entered the labor force and become unemployed. If this were a random occurrence the matter would be trivial, but it is clearly a social trend, both in advanced and in less developed countries. In addition, measures of the labor

force, employment, and underemployment all omit, or at best grossly underestimate, work that does not result in direct pay or profit. They are measuring the extent of the market, and like the official GNP they confound increasing market dominance with the position of individuals within the market.

The measurement of employment and underemployment derives its importance from the full-employment policies of governments in advanced societies. To put the matter crudely yet perhaps without excessive exaggeration, the Current Population Survey asks, in effect, "Would you like to work outside the home for pay?" and if the person expresses such a wish and has taken any action whatever to fulfill it, he or she becomes a part of the government's responsibility to provide work. Increasing participation rates, especially among married women, suggest that work itself comes to have a major place in the person's notion of self, despite the fact that the society is becoming richer. Keynes was one of the few writers who foresaw this; people want leisure, he said, until they get it. And long before him was Marx's phrase, "By labor man develops his slumbering powers." Again and again Marx tells us how important labor and the relations engendered by labor are for the social and mental life of the person. This feature of his thought gains in relevance as his notion of exploitation becomes outdated.

The theory and statistics used by Clogg, especially his categories of underemployment, fit the view that work is something people live for as well as live by. The mismatch of persons and jobs and the part timers who want full-time work, crudely measured though they are in the data now available, are categories that have a bearing on the progress of both postindustrial and preindustrial society.

Working for Capitalism. By Richard M. Pfeffer. New York: Columbia University Press, 1979. Pp. x+395. \$20.00 (cloth); \$5.75 (paper).

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In mid-1974 Richard Pfeffer, whose previous research interests seem to have centered on China, took a sabbatical from the political science department at Johns Hopkins University to work as a forklift operator in a Baltimore piston-rings factory. The decision to do so was triggered by a trip to China that convinced him of the need to bridge the gap between "mental" and "manual" work. Out of his experience of "manual" labor came *Working for Capitalism*, the merits of which became the subject of a bitter struggle at Johns Hopkins. As Pfeffer puts it: "Just after this manuscript had been accepted for publication by both Columbia University Press and by Monthly Review Press, the majority of the Political Science Department voted to deny me tenure. The department denied me tenure expressly because it does not appreciate this book" (p. 11).

Working for Capitalism is addressed to a wide audience, to academics, students, and workers, and can be read on two levels: first, as an account of Pfeffer's personal journey from complacency to uncompromising criticism of American capitalism and, second, as a general statement about the nature of American society. As the narration of a personal odyssey the book can be engaging and interesting. Pfeffer tells us more about himself than academics usually do, his writing style is lucid, and, if he frequently appears to have been naive, at least he has the candor to tell us about it. "Prior to the 1970s I had not really thought at all about the nature of work in the United States or about its meaning for American society. Never in my public school career or in my education at so-called elite universities—B.A., Yale University; LL.B., Harvard Law School; Ph.D. in Government, Harvard University—did any of my courses seriously consider the topic of work and its implications" (p. 1).

In part 1, "The Work Process," we follow Pfeffer as, armed with fake references that conceal his academic background, he looks for a job, suffers inconsiderate treatment at the hands of various interviewers, and finally finds work as a plant trashman picking up and dumping hoppers full of industrial waste. For months he wrestles with his forklift, which the company is too cheap to maintain properly. Finally the radiator blows up, scalding Pfeffer. We hear about his fight to avoid working Saturdays so he can be with his family and about the unwillingness of his supervisor, mostly out of cussedness, to accommodate him. Part 1 also contains an interesting and subtle discussion of the pressures on workers from management and from their own financial circumstances that make the performance of "voluntary" overtime almost obligatory. Pfeffer concludes from his experience that factory work in America involves endless hours of "putting in time" in a meaningless job.

Part 2 discusses his experience with Local 1784 of the International Association of Machinists and Aerospace Workers. Pfeffer quickly discovered that most of the rank and file, though convinced a union is better than no union, have a very low opinion of the local. Here his naiveté becomes hard to take as he suddenly "realizes" that the local is not all it might be and draws sweeping conclusions about unions in America:

Previously, when I had thought, for example, about the Soviet Union, where the state and the Communist Party are dominant, I understood that unions there had to be simply handmaidens to those institutions. On the other hand, when I thought about the United States, where government and political parties are not so dominant, I had more or less assumed, as a good liberal, that American unions therefore must be relatively autonomous. But now I could see clearly that in the . . . United States and the Soviet Union today . . . [unions] are the domesticated creatures of whatever institutions and classes are dominant. [P. 124]

Pfeffer's earlier "liberal" belief that all American unions fought honestly and hard for their members' interests (had he really never heard of the Teamsters or the ILA?) was, of course, as hasty as his later blanket dis-

missal of unions in America based on his experience of one local (in a few footnotes he admits that many unions in America, as a "secondary" function, do fight for their members, but these comments appear to be afterthoughts that are not integrated into his main observations).

Joining with a few grass-roots activists, Pfeffer opposes the union leadership's unmilitant conduct of contract negotiations with the company, writes and distributes leaflets, makes a fiery speech at a union meeting in which he calls for a better escalator clause in the contract, and decides to run for union office in October (three months after arriving at the plant). But union rules require that a member must have worked for a year before being eligible for office. Since, unknown to anyone else, Pfeffer has only another nine months before his sabbatical ends, he cannot wait, so he conducts a campaign to get the eligibility requirement waived (at this point the reader's sympathy for Pfeffer is strained—even if elected he would have had to quit in mid-term to return to Johns Hopkins). The campaign fails, and in February he is laid off.

In part 3 Pfeffer talks more generally about American society. Drawing on Braverman, Garson, Sennett and Cobb, Terkel, Balzer, and HEW's *Work in America* (Cambridge, Mass.: MIT Press, 1973), roughly in that order of approval, he argues, first, that the work and union situation he has analyzed earlier in the book is a result of capitalism. Second, that "liberal" America is a "thinly disguised dictatorship," for it excludes "people from control over their own lives." Furthermore, liberal institutions such as the grievance procedure in American labor relations encourage individual and fragmented, rather than collective, understandings and responses to oppression. And practices such as free elections and a free press give the illusion of freedom without the substance (along the lines of Marcuse's notion of "repressive tolerance").

Perhaps the most serious problem with this analysis is that it presupposes precisely what Pfeffer's study lacks—an account of how these workers view the political and economic institutions of America, of their conception of the power structure. Since Pfeffer says almost nothing about this, the reader is left with the impression that the workers themselves have little to say on this question. Such an impression is strengthened by his occasional remarks about the "fragmented experience" of the American worker. Discussing their jobs, he writes that even older workers have a vision that is "a composite of many fragmented work experiences rather than an organic appreciation of how and why production is organized the way it is" (p. 53).

Yet anyone who has spent some time with blue-collar workers in America knows that macro questions such as who runs the country, who benefits—questions concerning the nature of economic and political power—are a common topic of their conversations. Far from having a "fragmented vision," most workers are only too willing to expand on the larger picture. That there is another side to this issue Pfeffer implicitly concedes, for he cites a passage from Richard Balzer's *Clockwork* (New York: Doubleday

& Co., 1976) that relates how workers on their break discuss, among other topics, "current issues of political economy." Pfeffer cites this passage in the context of an attempt to show that workers' socializing on the job is "fragmented" and limited (p. 333); but it is a serious error, and one I doubt Pfeffer would want to defend, to infer that just because workers have a small amount of time for lunch they cannot, and do not, have a large general picture of America. And we need to know what that picture is in order to evaluate such theories as the "repressive tolerance" of liberalism.

Hermeneutics and Social Science. By Zygmunt Bauman. New York: Columbia University Press, 1978. Pp. 263. \$15.00.

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This is truly a remarkable little book. Zygmunt Bauman's aim is to trace the historical development of the hermeneutic challenge which has haunted the social sciences from their inception. In brief, the hermeneutic challenge is the claim that an objective understanding of subjective mental states not only is possible but also constitutes the *raison d'être* of the social as opposed to the natural sciences.

Hermeneutics and Social Science consists of 10 chapters which can be read independently but which together outline the map of the "hermeneutic circle." The content of the book is primarily sociological, since most of the chapters are dedicated to the analysis of major sociologists: Marx, Weber, Mannheim (chaps. 2, 3, and 4) and Parsons (chap. 6). The impact of Husserl and Heidegger on contemporary hermeneutics, especially through Schultz and ethnomethodology, is discussed at length. Finally, the book concludes with a reflection on the very notion of "understanding" and its relationship to the contemporary scientific conception of truth.

Hermeneutics originated with the Renaissance philological endeavor to discover the "true meaning" of ancient texts which had often been transmitted in corrupt form. Yet the major impact on hermeneutics came from German Romanticism and its claim that the truth of a piece of art was to be discovered in the subjectivity of the artist or the *Volksgeist*. Bauman espouses totally the *problematique* of German hermeneutics and thus does not probe the French tradition. Moreover, within German social thought, preeminence is given to Schleiermacher's program of a "*psychologische Interpretation*," which tried to reach intersubjective understanding through "sympathy," that is, by identifying oneself intuitively with the inner self of another actor.

Against this background of German hermeneutics, Marx stands out as the first to claim that an objective understanding of history is possible. This true understanding of history is achieved by reshaping the world in order to eradicate the roots of misunderstanding, which are socioeconomic, not

psychological or intellectual. Hence "Marx transforms epistemology into sociology" (p. 58); his work can be seen as a "programme of hermeneutics-turned-sociology."

Bauman's discussion of Weberian hermeneutics is equally interesting because he manages to free Weber from the Parsonian interpretation. In contrast to the American understanding of *Verstehen* sociology of the 1950s, Weber never aimed at a value-free interpretation of subjective states of mind but claimed that the objective understanding of rational behavior had become possible because both science and society in modern bureaucratic nations were dominated by instrumental rationality. From such a perspective, sociological hermeneutics can truly claim to be scientific to the extent that it limits itself to the analysis of the rational behavior of individuals living in a rational society. Hence understanding is not "intuitive sympathy" but "intellectual, analytical, and predictive explanation of action" (p. 86).

In the last two chapters, Bauman reassesses the task of hermeneutics in the light of ethnomethodology. The notion of understanding itself is transformed. Schleiermacher's program of psychological interpretation seems obsolete in the light of the ethnomethodological realization that the actor is seldom motivated by rational or even conscious motives. The understanding of an utterance, a behavior, or a text can be achieved when such an utterance or text is located within the "form of life" of which it is part. To understand an actor means to reconstruct objectively the form of life of his behavior or thought, not to immerse oneself in the uniqueness of his subjective moods.

In summary, Bauman has stated well the challenge of hermeneutics as an effort to "reconcile the recognition of the essentially subjective nature of social reality with the attainment of objective . . . description of this reality" (p. 222). His exegeses of Marx, Weber, Parsons, et al., are insightful. Yet a lot remains to be said about the "hermeneutic circle." There is no mention of anthropology or of French structuralism. Probably the challenge of hermeneutics is greater than its German Romantic version, which is the scope of this dense and demanding little book.

A Theory of Criminal Justice. By Jan Gorecki. New York: Columbia University Press, 1979. Pp. xv+185. \$15.00.

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Because "theory" is not used univocally in social science literature, the title of this short but insightful book may mislead. In *A Theory of Criminal Justice* Jan Gorecki does not attempt to describe or explain our criminal justice system, nor does he offer a philosophical account of punishment. Instead, he advances a prescriptive theory, arguing that our criminal jus-

tice system can and should be effectively utilized for moral education, to teach citizens that certain behaviors are wrong. Gorecki then explores the implications of this approach for criminal justice policy.

Sociological criminologists have long shared Gorecki's belief that the individual's value structure is a crucial determinant of his behavior. Edwin Sutherland and Donald Cressey once wrote, "When the mores are adequate, laws are unnecessary; when the mores are inadequate, the laws are ineffective" (*Principles of Criminology* [Philadelphia: Lippincott, 1960], p. 11). Of course, recent research on deterrence suggests that their second claim is probably false—the threat of punishment can affect behavior, even if that behavior is not perceived as wrong. But that is not Gorecki's point. He would argue that their first claim ignores the crucial relationship between laws and mores and would hypothesize that when laws and punishments are inadequate, the mores will also be inadequate.

Gorecki grounds his argument in behavioristic learning theory and, more specifically, in the claim that if a behavior brings punishing effects, the organism will tend to avoid it. He argues that if we follow "a properly arranged application of punishments, the prohibited behavior becomes more forcefully perceived by the society as intrinsically wrong and is avoided as immoral" (p. 3). Yet this educative effect of punishment is not automatic. It only occurs, says the author, when punishments are perceived as just. And justice requires that punishment be consistent, certain, appropriately severe, and applied only to behavior widely regarded as wrong.

Applying these findings to the criminal justice system, Gorecki produces a mixed bag of recommendations. Homosexuality should be decriminalized, because it is not universally regarded as wrong. Drug usage should be decriminalized because to punish the addict is to unjustly punish the innocent, that is, a person who is not responsible for his behavior. We should abandon rehabilitation. It does not work and it results in inconsistent punishments. Plea bargaining should be curtailed or abolished, as it reduces the certainty of punishment and offends our sense of justice. The exclusionary evidence rule should go. It, too, reduces the certainty of punishment, and the public cannot understand why criminals should go free simply because the state may have erred.

I have some difficulties with his analysis. Gorecki assumes, without argument, that the criminal justice system should be used for utilitarian rather than retributive ends. I doubt that the findings of behavioristic learning theory can be so easily transferred to the societal level. Although he rightly advocates decriminalizing homosexuality, it is doubtful that this would have an important effect on the criminal justice system. To say that punishment of addicts is punishment of the innocent is odd. Even if an addict cannot control his usage once addicted, it is perfectly plausible to hold him responsible for becoming addicted. After all, we hold people responsible for accidents caused by intoxication precisely because we realize that people are not in control once intoxicated. Although I share his aversion to plea bargaining, his critique rests on an inadequate analysis of the claim that

guilty pleas are generally involuntary and implicitly suggests that virtually all hard choices are involuntary. His critique of the exclusionary evidence rule (and other due process rights) deserves to be taken seriously but is not totally compelling.

There is much here to reflect upon, not the least of which is Gorecki's critique of what Robert Nozick has called "normative sociology, the study of what the causes of problems *ought* to be" (*Anarchy, State, and Utopia* [New York: Basic, 1974], p. 247). Sociological criminology generally claims that crime is caused by underlying social forces, for example, poverty, inequality, capitalism, alienation. As Nozick suggests, "We *want* one bad thing to be caused by another" (p. 248). Sociological criminologists have often argued that since the principal determinants of crime are located in these problems of the social environment, the solutions to crime must also be found there. Gorecki argues that sociology has developed an antipunitive ideology rooted in the belief that criminality is an unavoidable response to "social forces beyond the criminal's control" (p. 69). In so doing, he suggests, sociology has unwittingly contributed to crime by lending support to poor law enforcement. According to Gorecki, the criminal justice system is, in itself, an important determinant of crime and to treat it as irrelevant is to aggravate the problem.

Gorecki also notes that whereas sociologists often bemoan the stigmatizing effect of the criminal justice system, such stigmatization is precisely the point. After all, if punishment is to educate the society, we want it to declare emphatically that certain things are not to be done and that those who do them merit society's disapproval.

For Capital Punishment: Crime and the Morality of the Death Penalty.
By Walter Berns. New York: Basic Books, 1979. Pp. x+214. \$10.95.

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Franklin Zimring
University of Chicago

For Capital Punishment is an unusual contribution by a political scientist to the debate on the death penalty. According to Walter Berns, the book "could be entitled 'The Morality of Capital Punishment'" (p. 8). In it he argues a case for the death penalty on what are presented as moral grounds.

The author engagingly tells us that he took "instruction in regression analysis" in order to deal with "the deterrence question" (p. ix). But after a highly selective and cursory review of the deterrence literature, it turns out, happily, that we can forget about regression analysis. The argument about capital punishment "does not turn on the answer to the utilitarian question of whether the death penalty is a deterrent; . . . the evidence on

this is unclear and, besides, as it is usually understood, deterrence is irrelevant" (p. 8).

"The real issue," we are told, "is whether justice permits or even requires the death penalty" (p. 8). The answer is that justice requires it. "I am aware," writes Berns, "that it is a terrible punishment, but there are terrible crimes and terrible criminals" (pp. 8-9). Thus elliptically expressed, the argument calls to mind Samuel Johnson's rejoinder to a similar analogy in a different context. "It might as well be," said Johnson, "'Who drives fat oxen should himself be fat.'"

But there is more to Berns's argument than that, although how much more is not entirely clear. This is partly because he has a weakness for what can fairly be called "Nixonian" rhetoric. Thus we are told of "the misguided, and occasionally even absurd; sentimentality" (p. 9) of the opponents of the death penalty; of "the pathologically soft reformer" (p. 80), here exemplified by Tom Wicker; of "our modern reformers [whose] compassion is felt for the criminal [rather than for the victim]" (p. 81); of "our judges [who] do not believe in punishment" (p. 106); and of "American juries, civilized or softened by their life in the commercial or bourgeois society" (p. 134).

Berns agrees with Nietzsche's diagnosis that "there is a point in the history of society when it becomes so pathologically soft and tender that, among other things, it sides even with those who harm it, criminals" (p. 78). It is clear to him that American society has reached that point and that this is largely owing to "the effort of criminologists, judges, 'law reformers,' and the so-called intellectual press" (p. 149).

Berns is also addicted to the rhetorical device known as "shadow boxing." He invites into the ring callow contenders whose entrance is heralded by some such phrase as "criminology has asserted" (p. 113), "the intellectual community" (p. 149), or "modern penology has not understood" (p. 154). Then he plays with the deluded creatures for a couple of rounds before smartly knocking them to the canvas to the cheers of "the ordinary American [who] is still capable of being morally indignant at the sight of crime and criminals" (p. 149).

Moral indignation, or better still, anger, is seen as highly desirable and figures prominently in Berns's argument. "Anger is the passion that recognizes and cares about justice" (p. 152). "Anger is somehow connected with justice" (p. 154). "Criminals are properly the objects of anger" (p. 155). "Anger is an expression of . . . caring and society needs men who care for each other" (p. 155). "Punishment arises out of the demand for justice, and justice is demanded by angry, morally indignant men; its purpose is to satisfy that moral indignation" (p. 174).

It follows, sequentially if not logically, that "to reinforce the sentiments of a people, the criminal law must be made awful or awesome, and . . . the only way within our means to do that is to impose the death sentence" (p. 183). Executions, which should be public, that is, "witnessed by representatives of the people" (although not televised because of the tendency "to make a vulgar spectacle of a dignified event"), are said "to enhance the

awesome dignity of the law and of the moral order it serves and protects" (p. 188).

Berns's emotive exhortation—for he exhorts rather than argues—will presumably please those who, like him, accord the satisfaction of choler the primary place among the justifications of punishment. But it is too polemical in tone to be likely to appeal to those who are denounced for their "moral pretentiousness" (p. 161) or their lack of "moral strength" (p. 6), which they conceal "behind a cloak of pious sentiments" (p. 7).

This is a pity because, despite his enthusiasm for the lopping off of heads or the breaking of necks as a reminder "of the moral order by which alone we can live as *human* beings" (p. 173), Berns does say some sensible things about the inadequacy of a narrowly utilitarian cost-effectiveness approach to punishment. Unfortunately, they are almost submerged beneath the surge of bullyboy bluster.

From our rabid abolitionist perspective, two further aspects of this work are both curious and worthy of attention. First, this is a book by a political scientist about the moral necessity of execution that fails to confront let alone account for the peculiar moral geography of executions across societies. The movement in Western countries away from execution is not confined to the United States and Canada. It includes Britain, all of Western Europe, and most of what is considered the industrialized world. In many cases, a nonexecution policy is maintained in periods of increasing societal violence. Israel, besieged by terrorism, has yet to execute a terrorist. West Germany, perhaps restrained by memories of an earlier regime, refuses to fight the fire of political assassination with fire.

Who does execute? The United Nations surveys on this topic are somewhat out of date, but South Africa dominated the last statistics collected. Since then, and depending upon one's definition of execution, Uganda under Idi Amin and the Cambodians appear to have assumed a position of world leadership. If there is a correlation between execution policy and governmental adherence to human rights, it would appear to be negative and striking. There must be some reason why Berns does not address this issue. We cannot discern it.

In a brief, concluding chapter, Berns addresses the formidably difficult issue of justifying executions in the context of the current administration of American criminal justice and constitutional restraints against cruel and unusual punishment. His conclusion is that the punishment is not cruel but should be highly unusual. Legislation, he says, should be able to tell the difference between Jack Ruby and Lee Harvey Oswald. "It is not beyond the skill of legislators to draft such a statute—for example, it could provide that the death sentence be imposed *only* for 'outrageously or wantonly vile' offenses" (p. 183). His choice of a standard suffers from unfortunate timing: shortly after the publication of this book, the United States Supreme Court ruled that similar language provided insufficient protection against arbitrariness when a Georgia jury used it to impose the death penalty (*Godfrey v. Georgia*, 48 U.S.L.W. 4581, 100 S. Ct. 1759 [1980]).

In electing this standard, and in emphasizing that very very few exe-

cutions can be justified, Berns moves close to the social heart of the matter. The formal abolition of capital punishment raises the unfortunate specter of unilateral disarmament—when criminals can kill law-abiding citizens and the state cannot retaliate in kind. Current public opinion may thus be curiously divided: people want the death penalty but very few feel the intense need or desire for public executions. In this innocent, honest, and almost ambivalent last chapter Berns seems quite close to desiring death penalty legislation only as symbol.

The academic veneer of *For Capital Punishment* will help sell books and lend credibility to otherwise deficient arguments. It is, however, characteristic of literature both for and against capital punishment that cab drivers and college professors make the same arguments. It is true, we hope, that social and political science can make marginal contributions to this monumentally moral issue. This book does not.

Science and Colonial Expansion: The Role of the British Royal Botanic Gardens. By Lucile H. Brockway. New York: Academic Press, 1979. Pp. ix+215. \$21.00.

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There was a time, before Hiroshima, when scientific technology was hailed as an unalloyed blessing for mankind. If today's conventional wisdom holds science responsible for many of our problems and dangers, this skeptical attitude is seldom applied retroactively to 19th-century scientific achievements. In *Science and Colonial Expansion* Lucile Brockway does just that, by showing how the most innocent-seeming of sciences, botany, was in fact a servant of Britain's new imperialism, with all its evils. In making this connection, she not only widens our understanding of the social role of botany but also demonstrates the importance of technological change in imperial expansion, something historians have hitherto neglected.

The extremely slow growth and spread of agricultural populations in neolithic times show how arduous was the selection and domestication of wild plants. It is much more efficient to transfer useful plants to new habitats. Much of the world's economic development and population growth since the Age of Discoveries can in fact be attributed to such plant transfers: rice, wheat, and sugarcane to America, maize and potatoes to Eurasia, and manioc to Africa, to name but a few. Before the 19th century, such transfers were often made by farmers and planters or occurred accidentally. During the 19th century, professional botanists sought to accomplish such transfers deliberately, for what they, and the society they lived in, conceived of as the benefit of mankind.

Brockway traces the origins of British botanical science to the "physick gardens" in which medical men grew pharmaceutical plants, and to the

learned societies which sprang up in the 18th century. After the 1840s, botany became institutionalized. At the center was Kew Gardens, run by one family: first William Hooker; then his son Joseph; finally, Joseph's son-in-law, W. T. Thiselton-Dyer. Attached to Kew by a network of information and plant transfers were dozens of lesser botanical gardens, most of them in Britain's tropical colonies. These institutions were government funded, yet largely autonomous, and their success shows the upward mobility of the new professional classes in Victorian Britain.

Having thus set the stage in the first half of her book, Brockway turns to three case studies of imperialist plant transfer. The first was the removal of cinchona seeds and seedlings from the Andean republics in the 1850s; with their transfer to the Nilgiri Hills of South India, new plantations henceforth produced the raw material for the quinine and totaquine that saved British soldiers and officials and their families from malaria throughout the Empire. The second example is the transfer of the latex-producing *Hevea brasiliensis* from the Amazon forest to Southeast Asia in 1876, and the subsequent rise of the rubber plantations of Malaya, Ceylon, and Sumatra after 1905, just as the automobile was making rubber indispensable to industrial economies. The third example is the transfer of the sisal plant, the raw material for binding twine, from Yucatan to Florida in 1838 and, more important, from Florida to German East Africa in 1893.

The British Royal Botanic Gardens at Kew were involved in all three transfers. In the first two, they sent agents to South America to smuggle out the desired plants and seeds and arranged for their shipment to Kew and from there to Asia; in the case of sisal, an article published in the *Kew Bulletin* provided the Germans with the inspiration and information they needed.

In each case, Brockway considers not only the role of Kew but also the impact of the transfer: on the Latin American nations that lost a valuable monopoly, on the workers in the pretransfer system (Andean mountaineers, Amazonian and Mayan Indians), on Britain and the industrial nations as producers and consumers of plant-based products, on the laborers in the new transfer-based plantations, and on the peoples of the colonies in general. In doing so, and doing it in considerable detail, Brockway has added a vitally important dimension to the history of world relations during the past century.

Brockway set out not just to describe these facts, however, but to judge their morality. The book is based on a syllogism: imperialism was evil; plant transfers were an integral part of imperialism; hence plant transfers were morally wrong. For example, she qualifies the removal of seeds and plants from the Americas as "expropriation," "piracy," and infringement of "territorial integrity," suggesting (p. 35) that "ethical attitudes toward wild seeds have been slow to develop." She would propose a mercantilist attitude toward plants, or patent rights on forms of life.

The examples she has chosen, and the honesty with which she has studied them, offer as much evidence against as for her ethical point of view. The

Nilgiri cinchona plantations brought prosperity to the indigenous Badagas, a situation which leads the author to ask, "Why do we not find the expected immiseration of the local population?" (p. 133). The harvesting of rubber in the Amazon and of sisal in Yucatan were based on the ruin, extermination, or enslavement of the native peoples. The new plantations of East Africa and Southeast Asia created no worse misery than pretransfer methods of labor exploitation; if anything, the collapse of the wild-rubber boom alleviated for a time the genocide of Amazon Indians. Brockway rescues her general indictment of imperialism by calling the pretransfer situation "informal empire"; this, however, weakens her indictment of the plant transfer.

On the consumer side, Brockway's double condemnation of the cinchona transfer, as an expropriation from the Andean peoples and as insufficient to protect the Indian masses against malaria, leaves the reader hard-pressed to imagine a nonimperialist alternative: had the Andean republics kept their monopoly, would they have produced sufficient cinchona bark at a low enough price to satisfy the needs of the Indian masses?

Thus the evidence Brockway presents does not prove that imperialist plant transfers caused more harm than good to non-Europeans. Like so many other aspects of imperialism (e.g., education, medicine, public works), plant transfers were carried out for the benefit of Europe, and what benefits accrued to non-Europeans were very unequally distributed by-products and not the unalloyed blessings the imperialists often fancied them to be. The ethical history of the new imperialism remains to be written.

Brockway's ethics should not distract us from the real merit of her book, which is to introduce us to the complex social history of intercontinental plant transfers. One regrets that in so short a work (196 pages of text) devoted exclusively to Kew Gardens, the author had to slight some important aspects of the question, for instance, the cinchona transfer to Java, which overshadowed the one to India. She notes, correctly, that the economic competition between the British and Dutch empires was moderated by political cooperation and cartel arrangements, but she fails to note that many plantations and business ventures in the Netherlands East Indies were British owned, another instance of "informal empire." Similarly, the tedious but crucial matter of selective breeding and agronomic experimentation in the colonial botanic stations often takes a back seat in her story to the derring-do of plant piracy directed from Kew Gardens.

The book is based on an amazing variety of published sources, scholarly, governmental, and contemporary; one wonders whether unpublished documents might not have revealed other important facets of the story. Brockway's work is not the definitive study of her subject but a most provocative essay on the problems of technological diffusion in a lopsided world.

The Gentlemen's Club: International Control of Drugs and Alcohol. By Kettil Bruun, Lynn Pan, and Ingemar Rexed. Chicago: University of Chicago Press, 1975. Pp. xiv+338. \$12.50 (cloth); \$5.95 (paper).

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Drug control policies have always been integral factors in the patterns and effects of drug use. Recent historical research leaves little doubt that there is no necessary relationship between the severity of controls and the potential for abuse or social injury of any form of drug use. That the voluminous drug literature in social science consists largely of studies on abusers and ill effects is a phenomenon sociologists of knowledge will someday ponder. When drug controls are examined at all they are often taken for granted as natural responses to "drug problems" rather than as legitimate objects of inquiry in their own right.

In *The Gentlemen's Club*, Kettil Bruun, Lynn Pan, and Ingemar Rexed take an important step toward a critical understanding of the origins and consequences of international attempts to regulate the use of drugs. Using convention memoranda, treaty drafts, United Nations resolutions, and a host of other documentary data, they reconstruct the evolution of international drug policies from the Shanghai Conference of 1909 to the 1971 Vienna Convention. In the process we are offered several chapters of rich if imposing historical detail on the structures, goals, and operations of the key organizations that shaped that evolution. These are followed by five fascinating case studies of particular forms of drug control and a discussion of findings and recommendations. Because the book is a precisely delimited and technical historical study it is not for general sociological readership. Nonetheless, it should be of interest not only to students of drug use and social policy but also to political scientists and sociologists of law and social control.

Although Bruun, Pan, and Rexed employ a scholarly style, it is a sordid tale they tell. They situate the production of drugs and attempts to control use in the worldwide political-economic context. By so doing, they uncover an astounding array of incidents in which the goal of easing human suffering is sacrificed to the economic, bureaucratic, and professional interests of a self-selecting and self-serving elite. When it suited Western industrial powers, strict regulations were placed on drugs from developing nations. Yet when controls on manufactured pharmaceuticals were considered, it was decided that "free trade" should not be hampered. Whereas alcohol caused aggressiveness among Africans and thus required prohibition there, it accounted for many of the "sturdy qualities" of the French and so required only mild regulation elsewhere. On the basis of shoddy evidence, cannabis and coca were thought to cause murder and mental illness; but despite mounting evidence of abuse and ill effects, real controls on barbiturates were avoided so as not to "deprive numerous people of their sleep."

While pharmaceutical industry lobbyists got the diplomatic red carpet, dissenting voices and inconvenient evidence were suppressed. When moral and economic interests coincided it was apt to be a drug's properties that caused problems, but when they were in conflict, as in the case of amphetamines, "human weakness," rather than the drug, was thought to be at issue (at least according to the Swiss delegate to the United Nations who doubled as a pharmaceutical executive for Hoffman-LaRoche).

When drugs are knowingly misclassified to suit professional and bureaucratic needs, when conceptions of drug problems are shaped by political power rather than empirical experience, and when the insularity and hypocrisy of the "Gentlemen's Club" is camouflaged by a cloak of scientific legitimacy, one can expect to find, as these authors do, that the international control system has been largely ineffective. And this to the detriment of any real understanding of drug use or the amelioration of its harmful social consequences.

The orderly documentation of this history is the strength of the book. There are weaknesses. Given their demonstration of how ill-founded and prejudicial conceptions shaped policy, I was left thirsting for more theoretical analysis of why such conceptions were accepted so uncritically. Moreover, since the authors focus on why international controls took particular *forms*, they offer scant examination of the basic assumptions and motivations behind controls in general. Since drug use is nearly a cultural universal and its control bears little relationship to social harm, one wants to know something of the conditions under which national elites perceive such drug use as threatening.

Bruun, Pan, and Rexed touch on theory by noting how all organizations maintain legitimacy through myth and mystification, and by analyzing the instrumental and structural sources of Western power just beneath the pluralist exterior of the United Nations. However, this does not fully confront the political-economic roots of drug control beyond their direct links with the international bodies under study. Part of this shyness about theory may stem from their rigorous adherence to data. Yet one suspects that it followed as well from their intent—which was *not* to build a theory on *why* states of the world attempt to control states of consciousness but, rather, to *improve* their ability to do so. But such theory building may entail another book. And since the one they have written is already a solid reference for such purposes, these criticisms are in part a plea for a second volume.

Urbanization and Urban Growth in the Caribbean: An Essay on Social Change in Dependent Societies. By Malcolm Cross. New York: Cambridge University Press, 1979. Pp. xii+174. \$19.95 (cloth); \$5.95 (paper).

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Malcolm Cross's *Urbanization and Urban Growth in the Caribbean* contains a scholarly and useful summary of some of the social science knowledge about social change in that part of the world. Although it includes some of the standard demographic variables that describe urbanization and urban growth, it goes well beyond them. However, readers who do not happen to be wearing the same ideological-theoretical glasses as the author will find the book impaired, because it also contains an overly simple and skimpy version of Third World Marxism that occasionally blinds the author to some of his own findings.

The book's eight chapters begin with an introduction in which the Caribbean area is defined as "the Greater and Lesser Antilles with the addition of the mainland territories of Central and South America [Belize, Guyana, Surinam, and French Guiana] which share more economic and historical features with the islands than they do with their Latin neighbors" (p. 2) and in which the incredible racial, cultural, and linguistic mixing and segregating of peoples from Europe, Africa, and Asia in the region are described.

Chapter 2 gives a brief overview of four "theoretical orientations on urbanization": the rural-urban continuum, dual economy, modernization, and Marxism. Cross quibbles with each of them and concludes that "a framework for analyzing the ties of dependency is the only tenable approach to understanding the patterns of change within the contemporary Caribbean" (p. 22). He points out the importance of colonialism in the relationships between metropole and colony, in the urban/rural divide within the colony, and in differentiation within the colonial city itself; and he argues for the determining influence of the plantation system, which generally characterized Caribbean societies.

The longest chapter is devoted to the economic order. Topics include the rural economy, the peasantry, the extractive industries, manufacturing, tourism, trade and development, and income and employment, the theme being that Caribbean societies have dependent economies. Chapters follow on population structure and change; social structure and social organization; race, class, and education; and politics and policies. The book ends with an attempt to state some implications of the work, which seem to add up to the recommendation that the recent Cuban experience should be taken by other Caribbean societies as a guiding example.

Because the social science literature on the Caribbean, like the Caribbean itself, tends to be fragmented, Cross has done a service in bringing together in one short volume social information about the whole area. For this we can thank him. Yet sometimes his interpretations clash with the

facts, contradict each other, and read more like polemics than sociological explanations. Surely, Cuba might be an exemplar for other Caribbean societies *if* they could convince the Soviet Union to pour as much economic aid into their countries as it has into Cuba. But is that really very likely? Certainly not for most of the Caribbean. And even if Jamaica, Guyana, or some other Caribbean society did succeed in getting on the Soviet dole, would economic dependency, thereby, be any less than it is today? I do not see how. Cross's remark that ties with the Soviet Union are different because they "exclude foreign ownership of resources and only entail loans, technical assistance and trade agreements" (p. 133) is lame.

Puerto Rico, as Cross points out, "has been more successful than any other Caribbean territory in developing a manufacturing export sector" (p. 34) and has one of the highest GNPs per capita in the region. In fact, generally, the politically and economically more dependent territories have done better economically in recent years than the more independent territories. These facts, and others in the book, run counter to a major interpretive argument of the author that dependency—especially the penetration of foreign capital—is a cause of underdevelopment. Other dubious assertions are that dependency is the cause of inequality and of unwanted urban growth. Both are debatable given the extremes of economic inequality and high urbanization of the population in countries with highly developed economies, such as the United States, West Germany, and Japan.

Throughout most of the book, Cross assumes that urbanization and Western consumption patterns, and possibly even formal schooling, are problems, that is, bad for Caribbean societies. I find this condescending, especially since he knows full well that such "problems" are exactly what many, if not most, Caribbean people, given the opportunity, would choose to have, either at home or, if they must migrate to get them, abroad.

But enough. Read the tables and the factual statements in the text. They are convenient summaries of Caribbean societies, and I am happy to have them. But do not read the rest, unless you want a short course in how to let warmed-over ideology and logically garbled theory foul up one's thinking about social reality.

The American as Anarchist: Reflections on Indigenous Radicalism. By David DeLeon. Baltimore: Johns Hopkins University Press, 1979. Pp. xiii+242. \$14.00.

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The American as Anarchist by David DeLeon is a strong effort to put a difficult subject into perspective. The subject is difficult because it deals with anarchists not as members of groups or proponents of principles but as products of native conditions. The bibliography is the best available for

American sources and for differences between American movements and personalities and those elsewhere—a key point in the book's thesis.

The author contrasts indigenous radical antiinstitutionalism with what it perceived as repressive government, the military, and economic centralization. He sees varieties of religious dissent, the vast spaces of America, and traditions of free enterprise as nurturing an unbroken line of anarchist impulses from Roger Williams to Emerson, a line that culminates in the anarchistic philosophy of Thoreau. But the going becomes heavier as one struggles to weigh the actualities of "anarchist" thinking in America.

DeLeon sees such figures as Williams and even Thoreau as part of "Right libertarianism," antibigness in essence. He complicates the issue by consigning Henry George and his tax on land values to this category. Such "anarchists" are first of all individualists, resisting capitalist excesses. But Edward Bellamy's dream of a cooperative world wins him a place among DeLeon's "Left libertarians," even though in Bellamy's *Looking Backward* youth are drafted for industrial service. The problem is to discern the "anarchist" element here. There is less trouble with Benjamin R. Tucker, designated the "chief formulator of indigenous [American] anarchism" (p. 65). Tucker can be identified with a small group of 19th-century witnesses to freedom, headed by Josiah Warren, whose career underscores the paradoxes in the subject. Warren made a highly social effort—almost unique among dissidents, many of whom repudiated their society to some extent—to create a "time store" in Cincinnati, in which goods would be exchanged equitably rather than for profit. Yet Warren, in his implicit disagreement with capitalist principles, is far removed from the explicit anarchists who figured in the Haymarket Riot of 1886.

It would have helped if the author had maintained the old distinction between philosophic anarchists and anarchists of the deed. Such a distinction would have given better contour to the career of Emma Goldman, one of the few memorable anarchists, who began as an anarchist of individual action against alleged oppressors and evolved into one emphasizing freedom, whether in America or the Soviet Union. DeLeon's loose structure of American traditions verging on or contributing to anarchistic impulses is strengthened by his chapter, "Statist Radicalism," which depicts the radical as authoritarian. This chapter serves as a prelude to his last section, "Revival and Reformation, 1960–77," in which antigovernment eruptions are linked with the substance of previous chapters.

DeLeon confuses freedom with irresponsibility, as when (p. 45) he places Thoreau in the same context as Allen Ginsberg. The social leeway that gave status to Ginsberg's *Howl* was indeed the seedbed for a different American anarchy. However, to call the youth eruption of the 1960s a "renewal of old traditions" (p. 114) is to equate Thoreau with Abbie Hoffman: a grotesque presumption actually attempted by the partisans of the time. DeLeon is in sympathy with their aims, though critical of their tactics. A strange sentence of his reads: "Attacks on czars and kings might be acceptable, but an assault on the president, because of his aura of democratic

election, was a severe *error*" (p. 94, italics added). "Error" seems an inadequate word for such idealism of either word or deed.

The missing factor in DeLeon's presentation is the new conditions which made for the 1960s anarchist "revival." Older protests had been against a vigorous capitalism that wielded excessive authority. The new anarchy came not from the strength or principles of the protesters, but, internally, from the weakened American family structure and traditions, and, externally, from the need of government, business, and a nation of immigrants and minority groups to adjust to new world conditions: conditions in which the USSR held the atom bomb and Third World nations could act as if they were principals rather than pawns.

The anarchy of the youth culture in America was indeed unique, but it was a symbol of disorder, not renewal. In the absence of a stable society and sophisticated traditions, national renewal now proceeds slowly and experimentally along legal lines, rather than as a grasp of indigenous American experience. It is interesting that, although DeLeon clearly welcomes the anarchistic impulse as an instrument of freedom, he warns that it must be controlled by an awareness of history. His own study, evocative in style and ambitious in scope, raises questions that demand free inquiry and candid debate.

Forgive and Remember: Managing Medical Failure. By Charles L. Bosk. Chicago: University of Chicago Press, 1979. Pp. x+236. \$15.00.

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The terms "conventional sentimentality" and "unconventional sentimentality" were introduced into the literature of sociology in 1964 by Howard S. Becker in his groundbreaking edited volume on deviance, *The Other Side* (New York: Free Press). "Sentimentality," a concept he attributed to Eliot Freidsen (*Patients' View of Medical Practice* [New York: Russell Sage Foundation, 1961]), was defined as a "disposition on the part of the researcher to leave certain variables unexamined" (p. 4). When it is conventional, sentimentality leads to the "discovery" that conventional good guys are as good as we always knew they were and conventional bad guys are as bad or worse. The unconventional sentimentalist, on the other hand, "assumes, and refuses to examine the assumption, that things are in fact 'worse' than they might be . . . he assumes . . . that the underdog is always right and those in authority always wrong. . . . Whatever form sentimentality takes, its distinguishing mark is the refusal to consider distasteful possibilities" (p. 5). While Becker expressed disdain for both types of sentimentality, he granted that most of the articles in *The Other Side*, if they tended toward sentimentality at all, tended toward the unconventional form. But he considered this the lesser of two evils. "If one outrages cer-

tain conventional assumptions by being unconventionally sentimental, a large body of opinion will be sure to tell him about it. But conventional sentimentality is less often attacked and specious premises stand unchallenged" (pp. 5-6). To the degree that unconventional sentimentality is a minority perspective, Becker is undoubtedly correct. There will be plenty of "outraged conventionals" around who will take it upon themselves to counter specious premises. But to the degree that it becomes a dominant or even a majority view, it may attain the same degree of invulnerability, and thus create the same problems, that he attributes to conventional sentimentality. And there seems little question but that unconventional sentimentality is now and has been for some time a dominant perspective in much sociology. As a component of that perspective, the casting of persons in superordinate or powerful positions in the stereotypical role of villain is virtually endemic within the discipline.

Happily, in *Forgive and Remember*, a sensitive and insightful qualitative field study of the definition, assessment, and control of error among elite surgeons, Charles Bosk has quite self-consciously broken out of the clichés of unconventional sentimentality.

To take the role of the other impels us to look at how the other constructs his world. . . . If, in the study of lower-class deviance, it is unwarrantable to bootleg everyday definitions of right and wrong into our work, it is just as inadmissible to do so in the case of high-status professionals. . . . As sociologists, it is peculiar that we have such appreciation for the "symbolic" interaction and linguistic codes of corner boys, prostitutes, drug users, fences, members of communes, and so on, and so little appreciation of and tolerance for the same dimensions of action among physicians." [Pp. 189-90]

Sociologists, he suggests, are presumptuous when they advocate external regulation of the medical profession, because they have little or no understanding of how social control operates within that profession in the first place. Toward such understanding, an appreciation of the way physicians "detect, categorize and punish error" (p. 27) is essential.

Bosk uses a focus on "error" strategically to pursue a more general concern with three of the central issues in the sociology of occupations and professions: identity creation and maintenance by professional groups; social control of performance within the group; and the patterns of explaining, understanding, and neutralizing error in and failure of that performance by the group. Beginning with a conception of error *as error is understood* and distributed among the attending physicians and house staff he observed in four surgical services of an elite teaching hospital (chap. 2), Bosk analyzes the routine surveillance and control of subordinate error by superordinates through the dynamics and rituals of "attending rounds" (chap. 3); dissects the authority-legitimizing and horizontal-error-control functions of "grand rounds" and "the mortality and morbidity conference" (chap. 4); and outlines the process by which superordinates assess the

error-quotient (my term) of their surgeons in training and describes the fateful career consequences of "unforgivable" error (chap. 5).

What we learn from all this is that, contrary to the conventional wisdom within sociology, surgical training—at least in the setting observed and presumably more generally—is a profoundly moral endeavor. Surgical training is crucially moral training. The boundaries which identify the professional group are moral boundaries. The "unforgivable" error is not technical or even judgmental—surgery is, after all, an imperfect art. The unforgivable error is normative. Normative errors involve failure to discharge role obligations: failure to accept one's responsibilities, to acknowledge technical and judgmental error openly and promptly, to recognize and acknowledge superior skill and training, to learn from one's mistakes. Technical error merely demonstrates that one is still learning; judgmental error that one is not god. But normative error is evidence of a defect of character which may render one unfit for the assumption of professional responsibility and autonomy. The technically acceptable and morally sound trainee is rewarded; the technically inept but morally sound trainee may be transferred without embarrassment into another specialty; the technically brilliant but morally deficient trainee is "dumped." Clearly then, among surgeons, a system of social control over professional performance exists. It is a system based on intense moral socialization, rigorous selection, and then, for those selected, reliance on "conscience." It is a system, that is, of individual rather than corporate responsibility.

It is also a system that has its problems, and the analysis illuminates them. In institutions of the type and prestige of the one studied, it probably works well; further down in the medical hierarchy it may not. Ironically, "further down in the medical hierarchy" is the receiving ground of the "dumped." The system operates to free from the operation of the controls those who are deemed to need them most. What is called for, Bosk argues, is probably not external regulation but the development of a supplementary system of corporate controls embodying—at the level of the professional community—those elements of the individual control system which give that system its strength: hierarchy (authority), face-to-face interaction, public forums, and available sanctions.

Sociologists who would rather analyze than criticize, who view their primary task as one of understanding rather than judging, will appreciate this fine work. Those for whom the epitome of the sociologists' art is the howl of moral outrage will not. I trust the former outnumber the latter.

Nourishing the Humanistic in Medicine: Interactions with the Social Sciences. Edited by William R. Rogers and David Barnard. Pittsburgh: University of Pittsburgh Press, 1979. Pp. xvi+388. \$10.95.

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The charge that American health care has become increasingly depersonalized with the growth in this century of medical technology, bureaucracy, and specialization is heard from a number of critics. Others have responded that the doctor-patient relationship was probably never the ideal one so nostalgically recalled and that whatever loss there might be is more than made up for by the gains in health statistics. Yet current research demonstrates convincingly that the likelihood that someone will contract, or suffer from, or recover from any disease is closely tied to sociopsychological as well as environmental and physiological factors. Humanitarian medicine, therefore, can no longer be treated as a superfluous luxury. Making a patient feel good can sometimes make that patient feel well.

It is extremely difficult and sometimes frightening to respond fully and humanely to the psychological, social, spiritual, and widespread economic needs of patients, even though these needs impinge on their health. Some medical leaders, however, have attempted to tackle this problem. Most typically, their response has been to look to medical school curricula and continuing education. By introducing courses in the social sciences and humanities, they have sought to impress on students and practitioners the importance of understanding the values and motivations of their patients.

The Institute of Human Values in Medicine of the Society for Health and Human Values was created as part of this effort, and its goal is "to encourage the education of physicians and other health workers in the humanities as essential instruments for the responsible practice of their professions" (p. x). As part of its program, five task forces (composed of representatives of medicine and each of five other disciplinary areas) were constituted to explore areas of mutual intellectual interest. *Nourishing the Humanistic in Medicine* is the outcome of two years of meetings of a task force convened to explore "the ground between the humanistic end of the spectrum of the social sciences and medicine" (p. xi).

Most of the 11 people whose articles emerged from these meetings and are collected by William R. Rogers and David Barnard in this book are trained in psychology or psychiatry and religion, with one representative each of anthropology, history, economics, and clinical medicine. (Apparently, sociology was not considered one of the "humanistic" social sciences, although the inclusion of economics makes this distinction even more unclear.) The result is an unusual and original mixture of widely differing articles, loosely held together by the common thread of human values in medicine.

One of the strengths of *Nourishing the Humanistic in Medicine* is that

it should stimulate debate on two important issues related to the goal of educating health workers in "humanism." One of its weaknesses is that it addresses these issues only indirectly.

The first question is how much is to be expected from the individual physician. A paradox of the current discussions about medicine is that entirely contradictory demands are being made on doctors, sometimes by the same people. On the one hand, physicians are accused of having too much power and being in need of control from the communities they serve; on the other hand, physicians are also being asked to have an extraordinary range of clinical, pastoral, and political talents to fight against the social and environmental causes of their patients' illnesses and to educate and support emotionally patients and their families. It is the very rare individual who can do all these things well and comfortably.

Yet this image of physician as "superhero" emerges from some of the best articles in the book, because their authors seek to explore the quality of the doctor-patient relationship and to propose ways of improving it. For instance, historian Barbara Sicherman, anthropologist Anthony Oliver-Smith, and psychiatrist Robert Jay Lifton all offer in very different ways a model of the physician as patient advocate, social reformer, and spiritual guide.

This model requires that a physician be a person with incredible humility, self-awareness, and range of knowledge and insight, as well as being what humanist Samuel Banks, in his article, calls the "master juggler" and the "citizen-leader." The articles force the reader to consider whether this ideal of the superhuman doctor-priest can be achieved and with what effects on patients. Unfortunately, the question is never confronted directly by the authors and is taken up in only a paragraph by the editors, who admit that the creation of such physicians could strengthen the criticism that involvement of medicine in many different areas of life constitutes a form of "medical imperialism."

It would be unusual in any case to find any group of people with such qualities and certainly unlikely in an occupation whose entrants are chosen largely on the basis of their mastery of chemistry and physics. But this leads us to the other important question implied by the collection: can educational programs change the people in medicine and produce any significant changes in health care? This is hardly a new problem, but it deserves to be raised once again in discussing a book which emerges from an institute whose stated purpose is to improve medical care through the humanistic education of health practitioners.

The book is actually surprisingly ambivalent on this question, given its origin. Only Banks offers specific suggestions about the role of social science in medical curricula. Several articles simply ignore the issue of medical education, and one, by economist Frank Sloan, concludes from analysis of national surveys that inequality in access to medical services is of much greater concern to the lay population than is the personal quality of the treatment they receive. A few authors, such as physician John Stoeckle,

do write about the values of humanistic education, but they see the source of changes in medical care as coming primarily from outside the medical school.

In particular, one of the editors, psychologist-theologian William Rogers, suggests that the failure of physicians and patients to engage each other in a mutually open and health-producing way stems from the profound feelings of helplessness experienced by both doctor and patient. He proposes changes in the hospital policies which reinforce helplessness and in the economic and social conditions which make people powerless in their communities. His article is sensitively written, an excellent example of the integration of the psychological and the political: "For we can see direct connections between the helplessness that comes from victimization and unjust social discrimination, and the helplessness that experientially defeats patients in medical settings and simultaneously frustrates the physicians trying to be of service to them" (p. 47). Promoting humane and effective health care will require, we must conclude, much more profound changes than any which can be produced in the classroom.

The editors are to be congratulated for acknowledging in their conclusion the limits of the book's opening premise and for including political and economic concerns in their recommendations for changing the nature of medical care. But the fundamental problem of their work, the potential of humanistic education to create change in health care, remains largely unexplored. Their final assessment of the task force's work is that the meeting of these individuals was "invigorating" and "fun," and that the book reflects "the joy of a genuinely open and caring community" (p. 305). This model of ongoing interdisciplinary discussion is an attractive one and should be encouraged; its impact is, unfortunately, much too limited.

The best use for such a volume as this would be as a starting point for discussions with students in the health professions. A number of articles—such as Rogers's, Lifton's, and ethicist Thomas McElhinney's "Images of the Future and Genetic Engineering"—should provoke considerable debate and reflection. So should psychologist Paul Pruyser's convincing argument in favor of a diagnostic team headed by the patient, and ethicist Ernest Wallwork's concluding call for socialist medicine, based on a philosophical analysis presented in "Attitudes in Medical Ethics." Sicherman's second article, on the emergence of the diagnostic label "neurasthenia" in the 19th century, should raise questions about the cultural and political influences on medical labeling in our own time.

There is much good raw material in this book; the disappointment is that the overriding analytic issues have not been more clearly delineated. One very positive feature is the fine annotated bibliography which incorporates a wide variety of good sources.

Public Health and Private Gain: The Economics of Licensing Clinical Laboratory Personnel. By William D. White. Chicago: Maaroufa Press, 1979. Pp. ix+137. \$14.95.

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This small book will undoubtedly become a standard work on the development of clinical laboratory manpower, a subject seldom analyzed, and it will deserve its place. *Public Health and Private Gain* is rich in data relevant to many research interests.

Despite the word "economics" in the subtitle, the book contains very little academic economics and that little is easily understood. William D. White frames his analysis in terms of the political market—what it costs the actors to agitate for or against certain institutional and governmental controls on entry into and promotion through a highly stratified division of labor, and what the anticipated benefit of the desired outcome may be for the actor. He finds that the original creation of such a division of labor, with rigid requirements for training at each entry level, little or no promotion between levels, and a high turnover of personnel, enabled pathologists to gain and retain administrative control over production and income of clinical laboratories while minimizing time spent in unprofitable administrative duties. This system has subsequently induced high support for licensure among the lower-ranked laboratory occupations, the members of which seek to gain for themselves what physicians have, namely, occupational self-control and higher wages. Although each group claims that its strategy increases quality of output, White is unable to find evidence of this.

White's analysis concentrates on explaining the background of licensure efforts: the development of clinical pathology; the attachment of laboratories to hospitals; the rise of a highly educated, primarily female technologist profession; and the near-Byzantine conflicts within and between a variety of acting groups and institutions. He includes case histories of licensure efforts in California (successful) and Massachusetts (unsuccessful), and an economic analysis of the effect of licensure on wage rates and laboratory costs. He finds, interestingly, that by lowering turnover of personnel, stringent licensure simultaneously increases wage rates and decreases labor costs.

The book is at its best in its careful research of facts and events and the meticulous fitting together of data to reach limited conclusions. Theory is a minor part of the book, and White's failures here should by no means distract the reader from its overall excellence. Yet his failures are instructive. "Theories about the behavior of different groups are ultimately based on theories about the behavior of individual actors" (p. 101), he asserts, and as a result he finds himself unable to explain why people act as they do. Particularly puzzling to him is that state licensure of technologists has de-

pendent on strenuous lobbying efforts of bureaucrats in public health departments who, as government employees, derive no personal economic gain from their actions. California's chief pathologist, a clinician and activist with a public health background, created and defended that state's licensing laws almost singlehandedly, even when he was about to retire and could not use the law to increase his bureaucratic empire. White wonders why this man acted in what he believed to be the public interest when neither consumers nor producers were pressuring him to do so. The head of Massachusetts's state laboratories, an academic pathologist closely tied to Harvard Medical School, believed in voluntary certification controlled by the medical establishment. His lack of support for technologist licensure assured its failure in his state. Why did this man act so differently from his counterpart in California? White's political market model of action does not provide an explanation of individual behavior based on ideals and interests, and he chooses not to pursue other theories which might have taken him out of economics and into sociology.

One wishes that he had chosen differently. He presents ample evidence of group interests in the various occupational conflicts, and his presentation makes it obvious that he is concerned with their importance. His analysis cries out for amplification. The book is, after all, about conflict in and control over the division of labor in society and therefore applies to both small and large issues. White's finding that licensure increases the technologists' occupational self-control but does not necessarily improve the quality of test results begs to be applied to other licensed occupations, including physicians. His analysis of the feminization of technology could both enrich and be enriched by theories of sex stratification. His data on the pivotal role of civil servants in developing institutional staffing regulations (via the Medicare program) as well as occupational licensing laws raise interesting questions about the interrelation of the public and private sectors.

Public Health and Private Gain must be read by anyone with an interest in the health service industry in particular or licensing in general. It should also be well used by anyone interested in occupations and professions, theories of the state, social change, sex segregation, and bureaucracy.

Sociocybernetics: An Actor-oriented Social Systems Approach. Vols. 1 and 2. Edited by R. Felix Geyer and Johannes van der Zouwen. Boston: Martinus Nijhoff Social Sciences Division, 1978. Pp. xi+154; xi+158. \$17.50 per volume.

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Toward the end of the 1970s, a number of books appeared with the goal of providing both a simplifying and unifying framework for biological and so-

cial phenomena (e.g., *Living Systems* by James Miller [New York: McGraw-Hill, 1978]; *Ecodynamics* by Kenneth Boulding [Beverly Hills, Calif.: Sage, 1978]). Although not nearly as tightly integrated or as focused, the two volumes to be reviewed here can be included in the same tradition as the books by Boulding and Miller. The tradition common to these works is what has come to be known as "systems theory." As Anatol Rapoport points out, however, "This designation is, in some ways, a misnomer, because 'system theory' is not strictly speaking, a theory resting on specific substantive hypotheses from which empirically verifiable assertions are deduced. It is rather an outlook inspired by the successes of mathematical physics" (2:125).

Before examining the contents of *Sociocybernetics*, some additional comments about the systems theory perspective are warranted. The goal of the systems theory movement is to develop a set of concepts linked together in a lawlike manner which will be valid over many different levels of phenomena. Levels of living systems could include the cell, organ, organism, group, organization, society, and supranational system (2:85). In this respect, the systems theory movement is most comparable to Herbert Spencer's efforts in the 19th century. The perspective is not reductionistic as are some other contemporary perspectives, such as sociobiology. Emergence is not denied; instead, the goal is to find principles common to all systems.

The two volumes of *Sociocybernetics* contain 15 articles selected from 61 papers on social systems presented at the Fourth International Congress of Cybernetics and Systems held at Amsterdam in 1978. Since it was an international conference, the names of many of the European contributors (with the exception of Tom Burns) will be unfamiliar to most American readers. However, there are some very familiar names here. One of the founding fathers of the Society for General Systems Research and the editor of its yearbook for many years, Anatol Rapoport, is a contributor. Nobel laureate Herbert Simon is included as is James Miller, editor of *Behavioral Science* as well as author of the previously mentioned *Living Systems*.

The collection is divided into two volumes, each of which begins with the same introduction by the editors, R. Felix Geyer and Johannes van der Zouwen. The division is fortuitous because most of the better contributions are concentrated in one volume. The two volumes will be described separately.

Volume 1 provides several illustrations of the drawbacks of applying the systems theory perspective to social phenomena. The first of these shortcomings is the style of prose this approach seems to inspire. This style reached its zenith (nadir?) in the work of Talcott Parsons and can best be described as semantically turbid. Unfortunately, several of the papers in volume 1 are written in this style.

A second shortcoming the systems theory approach often engenders is an exaggerated formalism or mathematization—a manifestation of what Pitirim Sorokin (*Fads and Foibles in Modern Sociology and Related Sciences* [Chicago: Regnery, 1956], p. 103) called "quantophrenia." This

"formal" approach to theory is exemplified in the papers by Nowakowska and Booker and seems to result in an endless array of symbols, subscripts, and equations. A deductive mode reminiscent of the proofs of high school geometry is then employed to move the reader from one equation to the next. At the end of the exercise, one is at a loss regarding the substantive conclusions. This model may help the theorist specify the exact meaning of substantive statements, but it soon becomes a sterile exercise in symbol manipulation whose proper sociological content is nil. In such a discipline as sociology whose error terms often substantially exceed the magnitude of explanatory coefficients, such functional (in a mathematical sense) formalizations are clearly inappropriate.

These two shortcomings—semantic turbidity and excessive formalism—are regrettable because there *are* some good ideas in several of these pieces if one is willing to dig for them. An example is the discussion by Tom Baumgartner, Tom Burns, and Philippe DeVillé of the distinction between ordinary power and metapower (the former being power within the context of a system's rules, while the latter is the greater power of being able to set or change the rules). Unfortunately, the reader may be deterred by the obstacles already discussed.

The final paper in volume 1 is a major exception to the shortcomings mentioned above. This is a paper by Martin Zwick which attempts to interpret the Marxist paradigm in terms of catastrophe theory. Catastrophe theory is a technique developed in biology for the analysis of a discontinuous or discrete dependent variable with a minimum of independent variables. Since a revolution is a discrete event, the convergence of Marxist and catastrophe theory seems particularly appropriate. Catastrophe theory is difficult to explain, and Zwick has done the best job of explanation for the uninitiated I have yet to encounter. His discussion of Marxist theory is also unusual in its refreshing lack of polemics and use of a nonproselytic style. The article should, therefore, be acceptable to Marxists and non-Marxists alike.

The papers in volume 2 generally avoid the shortcomings found in volume 1. In general, they vary from good to excellent and justify the effort it took to put the second volume together. Among the good papers are the ones by James Beniger, Marian Mazur, and Wil Dijkstra and Johannes van der Zouwen. Beniger's is one of only two papers in the entire collection to directly address empirical data (the handling of juvenile drug offenders). Although he claims to have synthesized the system and action approaches to analyze his data, he appears actually to have used the two approaches in a complementary fashion and not to have achieved a true synthesis. Nevertheless, the analysis is well done.

Mazur's paper presents a discussion of feedback in simple systems which employs just the proper number of formulae to make her points.

The Dijkstra and van der Zouwen study concerns the dynamics of the interview situation in social research. Role theory is used as the conceptual foundation in addition to some systems theory notions. Researchers who

utilize interviewing techniques would be well advised to examine this paper.

Four papers fall into the very good to excellent category: those by Bobbie Jones and James Miller, Herbert Simon, Anatol Rapoport, and Richard L. Henshel. Jones and Miller provide the other paper that utilizes empirical data directly (information degradation or loss in communication). They illustrate the use of common concepts and propositions at two levels of analysis (the group and society) and thereby provide an example of what the people in the systems theory tradition are trying to accomplish.

Simon presents an argument against overly complex models of social behavior and gives an example of how simple models can be built which provide answers to most of the questions the more complex models would have addressed. The illustration Simon provides is based on his work on the evolution of hierarchies. As such, it reflects his change in focus from organizational and administrative behavior to the mathematical and computer simulation of human behavior.

Rapoport points out some important differences between the physical and the social sciences which have adverse consequences for model building in the latter. These differences include the lack of firm axiomatic statements in the social sciences from which to make deductions and even an absence of key established concepts. He then shows how models, through their illusion of precision, generate self-fulfilling prophecies. He also illustrates problems associated with uncritical utilization of such models.

Rapoport's study serves as an excellent introduction to the final paper in volume 2, which is also the best in the entire collection. Of immediate relevance to those interested in the sociology of science, Richard Henshel's paper (originally published in *Futures*) describes a process linking the prestige of a social science and the accuracy of its predictions. Involved are self-fulfilling and self-defeating prophecies and the potential of disciplines for providing institutional definitions of the situation. Freud's influence on the West is used as an example of this kind of core situation defining. Henshel offers a truly superior analysis.

Both volumes suffer from two minor flaws: the copy editing should have been better, and a standard referencing and citation style should have been used by the authors. To use a systems theory concept, these flaws indicate that the editing and production procedures were "loosely coupled." The two volumes do not serve as a good systems theory primer. Those interested in good introductions to systems theory are better advised to acquire Miller's aforementioned *Living Systems* or Walter Buckley's *Sociology and Modern Systems Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1967). To those already initiated, I recommend acquiring volume 2 and reading the Zwick paper from volume 1.

Differentiation between Social Groups: Studies in the Social Psychology of Intergroup Relations. Edited by H. Tajfel. New York: Academic Press, 1978. Pp. xv+474. \$50.00.

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Henri Tajfel's collection of papers portrays part of the development of a research tradition aimed at understanding intergroup relations. While earlier reports from the tradition are published in European and Canadian journals, they are more sparsely represented in American journals. So although the work in the paradigm could inform research in social psychology, race relations, and their interface, the ideas and findings have been neglected relative to the contribution they could make. Unfortunately, *Differentiation between Social Groups* is unlikely to remedy this defect. The contributions are wordy and are often overburdened with material of more interest to devotees of the paradigm than to specialists in relevant subfields or general readers.

The heart of the volume is contained in the first chapters, in which Tajfel outlines his theory concerning in-group bias and researchers report studies designed to evaluate and extend the theory. Tajfel argues that individual identity and self-concept are determined in large measure by group membership. By comparing one's own group with other groups on relevant dimensions, one forms the basis for social identity. When two groups simultaneously engage in such comparisons, the strain of each group trying to derive positive evaluations produces social intergroup competition. The evolution of in-group bias is thus fed by social comparison processes through which individuals transform the recognition of group differences into differential treatment of actors. The editor's analysis suggests that when individuals are differentiated into groups and when opportunities for social mobility are tenuous or nonexistent, *group* membership, rather than *individual* characteristics, will be used to define social identity.

Tajfel also describes a basic experimental situation which, with various modifications, is used by some of the volume's contributors in empirical research. It consists of using a trivial task (such as expressing preference for the paintings of one of two artists of similar style) to differentiate subjects and then asking them to allocate rewards to given subjects according to predesigned matrices of choices. Analysis of the choices reveals the respondents' preferences for several reward allocation strategies which are interpreted in terms of in-group bias. Tajfel describes findings produced under the minimal conditions necessary to evoke in-group bias which suggest that even when a history of previous hostility, conflict of interest, and face-to-face interaction is eliminated from the experimental situation, in-group bias remains. Thus, mere differentiation on the basis of a trivial task may be a condition sufficient to provoke discrimination. In subsequent chapters, this idea is refined as we learn that social categorization produces

discrimination *if* such discrimination is the only mechanism available for expressing self-identity (J. Turner, chap. 5). Hence the link of identity between differentiation and discrimination is strengthened.

At this point, however, the volume's flaws begin to outweigh its contributions. Even though the initial discussions are often tedious, the issues raised are important and deserve attention. However, attempts to explore the external validity of the basic conclusions under conditions which vary such factors as status of group members and legitimacy and stability of group status are a mixed blessing. Social psychologists should appreciate the use of experimental procedures to unconfound variables often strongly associated in "real world" intergroup situations. It is only by this method that we learn how minimal are the conditions which prompt discrimination. The same readers should also appreciate the repeated use of the basic experimental setting. However, the patience of even the most empirically oriented social psychologist will be tried by the seemingly endless number of tables portraying patterns of reward allocation and analyses of variance. Many social psychologists have recently adopted a terse style for reporting experimental procedures and findings, a style less burdened by "academese" than reports written in previous years; these authors have not. The result communicates less than the findings warrant.

Researchers not accustomed to experimentation will probably avoid these chapters in favor of others which analyze anthropological data or field situations in terms of Tajfel's theory. For example, they should appreciate the insightful discussions of linguistic differentiation between ethnic groups according to the principles of Tajfel's theory (H. Giles, chap. 15) and of pay differentials in a factory (R. Brown, chap. 16). Often, however, these papers too provide more detail than is desirable.

On the positive side, all readers should endorse the volume's strong integration of classic sociological arguments with the theory and evidence specific to the Tajfel research tradition. Work by Festinger on comparison processes, Gurr on relative deprivation, van den Berghe and others on race relations is well cited. Indeed, the classic studies of intergroup conflict conducted by Sherif and his colleagues form an important point of departure for Tajfel's theory and are analyzed in some detail. The problems I have noted are, of course, not unique to this volume; others have fallen prey to some of the same difficulties. Such a state of affairs, however, may only serve to heighten our frustration with particular works.

Child Influences on Marital and Family Interaction: A Life-Span Perspective. Edited by Richard M. Lerner and Graham B. Spanier. New York: Academic Press, 1978. Pp. xvii+360. \$19.75.

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The aim of this anthology is to provide a new direction for the study of family and for the study of children. Instead of focusing chiefly on family as sociologists generally do, or on children as psychologists generally do, editors Richard M. Lerner and Graham B. Spanier propose a new "unit of analysis." This unit is the "individual within a family system." Moreover, this unit must be understood in a "life-span" sense. Life span goes beyond the well-worn notion of life cycle in several ways, perhaps the most important of which is cognizance of changing cultural and historical factors experienced by different birth cohorts.

Furthermore, while the mode of analysis intrinsic to their approach "requires a circular statistical model, a conception of variables as constantly interrelated, and an assumption of constant change, it is recognized that satisfactory tools . . . do not exist" (p. 17). This methodological lack, they say, is no cause for despair but instead a challenge to develop the techniques necessary to study ongoing dynamic reality.

Given that no studies can be found at this level of theoretical and methodological sophistication, what then is the purpose of the chapters contributed to *Child Influences on Marital and Family Interaction*? They summarize existing knowledge regarding "the reciprocal impact" of family and child. With this "low-level" information in hand, the stage will be set, argue the editors, for subsequent, more elaborate "dynamic analyses."

Hartup's essay, for example, rebuts the "social mould" notion of socialization and argues that we should investigate instead how the child "plays a role in his own socialization." One line of research describing just how that particular phenomenon occurs is found in Lamb's chapter dealing with the impact of their first infant on a couple prior to and after birth. For example, Lamb argues that the first child is likely to force couples into traditional roles regardless of whether they held traditional or nontraditional sex roles prior to its birth. However, Lamb's argument is based on the assumption that the "wife will withdraw from work in preparation for childbirth." He says nothing about alternative day-care facilities, nor does he raise the option of extensive and serious paternal involvement in child care. Furthermore, he does not adhere carefully to the Lerner-Spanier model by asking the next logical question: what is the effect on the developing child of parents who were formerly nontraditional but were "forced" to adopt traditional roles? However, he does seek to apply that model in another section of his essay by arguing that certain children contribute to their own abuse through being premature or by being "difficult" to manage.

Separate essays by Bengtson and Troll and by Hess and Waring probe

intergenerational feedbacks. The Bengston-Troll chapter—devoted to youth and parents—is particularly intriguing because it analyzes these dynamic feedbacks in terms of emerging trends and changing social conditions in the larger society. Hess and Waring are also aware of societal factors, but their major concern is with middle-age and senior-age persons and with the conditions that enhance or weaken intergenerational bonding.

In contrast to the editors' contention that few tools exist to achieve their conceptual ideals, several of the essays present apparently workable modifications of longitudinal-type designs. In the one chapter devoted entirely to methodological concerns, Klein, Jorgensen, and Miller are decidedly more sanguine than the editors regarding the adequacy of available tools: "Little that we have said here points to the need for a methodological revolution or other significant breakthroughs in technique. . . . Our point has been that we can reconceptualize old issues and place them in new perspectives" (p. 132).

In sum, while the volume does not make a definitive case for the editors' particular perspective, it does mesh very well with the growing renewed interest among behavioral scientists in the study of social processes. Scholars interested in processes in general, as well as in their application to family and children, will find that parts of this book make for stimulating reading.

"Illegal But Not Criminal": Business Crime in America. By John E. Conklin. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1977. Pp. xiii+153. \$8.95 (cloth); \$3.95 (paper).

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There is no doubt that white-collar or business crime has become a central focus in criminology. Witness the many calls for its investigation and the increasing efforts to explore its scope, ramifications, causes, and control. John Conklin's *"Illegal But Not Criminal"* makes a modest contribution to our knowledge of this important and complex area. It does not provide a body of original data or develop a new conceptual or theoretical framework for understanding business crime. Instead, it attempts to survey the existing literature and arrive at some appropriate conclusions.

Clearly, the book aims at the student, not the scholar. But this is not to say that it is worthless. On the contrary, the book informs the reader competently about most of the major issues: the costs of business crime, the public's attitudes toward it, its principal causes, the current patterns of its control, and some strategies for effectively reducing it. The discussion of these issues is generally balanced, approaching them from several angles. It includes accurate summaries of the important contributions to the area, such as the works of Sutherland, Clinard, Cressey, and Aubert,

and its conclusions are adequately supported. Generous illustrations make reading it somewhat enjoyable.

While reasonably competent, Conklin's analysis is incomplete, begging a number of interesting questions. Let me provide four examples. First, although he considers at length the causes of business crime, Conklin fails to consider how the label of white-collar or business crime developed. Chances are that businessmen have cheated their customers and competitors for years. Yet the label of white-collar or business crime is of recent vintage. What led to its invention and diffusion? Second, why has research on business crime proceeded slowly compared with research on other forms of crime? Is it simply a matter of what researchers find interesting and worthy of study? Is it because the relative invisibility of business crime makes the collection of data extremely difficult? Is it a reflection of political pressure in the priorities of granting agencies? Third, while Conklin examines aptly the manner in which our economic system facilitates business crime, he fails to consider how that economic system developed. What historical processes produced an ideology that values profit and consumption, a structure that provides illicit opportunities for increasing profit and consumption, and a culture that furnishes attitudes and skills favorable to employing illicit opportunities? Fourth, in what ways do offenders prevent the detection of their criminal activities? In Chapter 5, Conklin examines the prosecution, conviction, and sentencing of those engaged in business crime. But there is an earlier stage in the control process—the detection of business crime. Detection is always problematic, for information about violations can be concealed. Do offenders render their crimes invisible, and, if so, what techniques do they employ?

Superficiality aside, the analysis suffers from a very serious problem involving the conceptualization of business crime. After reviewing some of the debate on defining white-collar crime, Conklin offers an alternate phenomenon, "business crime," defined as "an illegal act, punishable by a criminal sanction, which is committed by an individual or a corporation in the course of a legitimate occupation or pursuit in the industrial or commercial sector for the purpose of obtaining money or property, avoiding the payment of money or the loss of property, or obtaining business or personal advantage" (p. 13). This conception is dubious. It does not encompass the illicit acts of nonprofit organizations, such as charities or foundations. More important, it groups together the illicit activities of individuals and organizations; thus, employee pilferage and corporate price fixing are to be treated alike. But individuals and organizations are fundamentally different, involving different types of decision-making processes, different kinds of responsibility, different patterns of social control, and so forth. To treat individuals and organizations as though they were alike is to gloss over important differences. To treat them alike is also to make generalizing precarious. For example, it would be difficult to show that the psychological conditions causing employee pilferage are relevant to explaining corporate price fixing. Similarly, it would be hazardous to generalize the strategies for reducing individuals' business crimes to organizations. In chapter 6,

Conklin argues that business crime could be reduced if punishment were more severe and certain, and, for the most part, he supports this hypothesis with evidence drawn from studies of embezzlement, employee pilferage, and tax evasion. While severe and certain punishment may deter individuals, it may not deter organizations, which can pass penalties on to others, such as consumers or employees. What would have been more useful is a taxonomy differentiating the basic types of offenders and victims (e.g., individual and organizational offenders and victims) and an examination of the major issues in terms of this taxonomy.

Despite these drawbacks, students should find *"Illegal But Not Criminal"* an interesting and informative volume. In turn, scholars may find it useful in generating discussion.

Communication Yearbook 3. Edited by Dan Nimmo. New Brunswick, N.J.: Transaction Books, 1979. Pp. xii+704. \$29.95.

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More than a quarter of a century ago, communication processes and effects were very much in the mainstream of sociological inquiry. Merton, Lazarsfeld, and their best students were conducting important research in political communication, diffusion of innovation, interpersonal influence, and the like. However, in the years since, sociology has moved in other directions and the sociological study of communication has, with a handful of notable exceptions, given way to the new multifaceted discipline of communication—a scientific enterprise with its own largely nonsociological paradigms and problematics.

For the sociologist who is curious about the current state of communication theory and research, *Communication Yearbook 3*, edited by Dan Nimmo, might be a good place to browse—but there are better. As the third annual in the series sponsored by the International Communication Association, *CY 3*, as it has come to be known, is occasionally impressive, often obscure, and sometimes downright banal. While this volume does give ample proof of the disciplinary vigor and reflexivity of communication research, it also suggests that not all years are uniformly outstanding in the scientific community.

The first section of the yearbook is devoted to discipline-wide overviews and commentaries. Rokeach summarizes recent advances in value research and shows how his work and that of others relates to the classic communication problem of value change. In an exceptionally erudite review, Lanigan draws theoretical parallels between the major divisions of philosophy (metaphysics, logic, etc.), the central constructs of communication theory (intention, convention, etc.), and selected models from the metatheory of communication. Brown draws on Isaac Newton's unpublished Fifth Rule of

reasoning to argue that communication theorists adopt a frankly relativistic stance. In an openly self-critical spirit, Rogers and Adhikarya offer an update on diffusion studies and point to the utility of the revised diffusion paradigm for the study of organizational innovation. In a mini-symposium, five European scholars discuss the current state of communication training and research in their nations, while Williams offers an engaging and provocative look at communication in the year 2000.

While the first section of *CY 3* contains much of interest and value, the second portion of the book, containing subfield overviews, is often disappointing. Craig puts forth a strong, if unexciting, case for an integrated discipline of "cognitive science," but Bochner and Kreuger skip over much of importance in interpersonal communication research to rehash the tired debate about the "crisis" in social science. Foley offers an uninspired scheme for classifying variables used in mass communication research. In a discussion reminiscent of sociology's paradigm debates of the 1960s, Redding surveys organizational communication theory and calls for a better understanding of its "frames of reference." Larson and Wiegele's overview of political communication research does little to synthesize or evaluate the year's output. Wheeless and Hurt's work on instructional communication and Saral's on intercultural communication are almost completely devoid of intellectual content. Finally, in their review of health communication, Costello and Pettegrew come to the startling, albeit incorrect, conclusion that health organizations are organizationally unique and thus a fitting subject for study.

Still, there are a few bright spots in *CY 3*, especially in the final section, which contains examples of recent studies. Porter, for example, warns about the low replicability and construct integrity of the widely accepted concept of communication apprehension. Korzenny, Greeberg, and Atkin show how parental disciplinary styles mediate the antisocial effects of violence on TV, while Collins et al. skillfully probe the implications of the *Miller* anti-pornography decision. Heath examines rhetorical strategies available to vulnerable social movements, while Allen and Chaffee track the interaction of mass communication and the political participation of black Americans.

In sum, *Communication Yearbook 3* is not a "must" addition to your professional library. For the sociologist, or indeed the communications specialist, its predecessors, *CY 1* and *2*, are far more valuable and important.

City and Hinterland: A Case Study of Urban Growth and Regional Development. By Roberta Balstad Miller. Westport, Conn.: Greenwood Press, 1979. Pp. xiv + 179. \$17.50.

Jay D. Teachman
University of Iowa

In this small and readable text Roberta Balstad Miller describes for us the development of Syracuse and its immediate hinterland, Onondaga County,

over the period 1790 to 1860. Miller's book is definitely a historical treatise most easily compared with the work of Charles N. Glaab or Richard C. Wade. It is a welcome break for those of us more accustomed to the highly statistical treatment often accorded urban and regional analysis by sociologists. Indeed, one of the most important aspects of this volume is that it serves to remind those of us concerned with contemporary developments that urban regions are entities which have gone through a process of development over time.

City and Hinterland consists of seven chapters and a useful bibliographic essay. The first and last chapters are devoted to a brief consideration of larger theoretical issues and linkages with other historical analyses of urban regions. Chapters 2, 3, and 4 focus on what is probably the most crucial element in the development of all urban regions, transportation innovations. The fifth and sixth chapters deal with migration patterns as one important indicator of city-region interaction. Each chapter leads logically to the next and builds on the previous analysis.

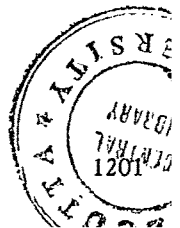
Making judicious use of limited statistical information, Miller describes the process by which Onondaga County was transformed from homogeneous agricultural land into a diverse, specialized city-region over a period of 70 years early in its history. The most obvious causal force in this transformation is the Erie Canal, opened for traffic in 1820. Serving to alter economic relationships between Onondaga County and the East, the canal also forced a restructuring of the county's economic system. Miller shows how the canal focused economic dominance along its route and especially in the city of Syracuse. It is only after the opening of the canal that one can speak of the emergence of a center, Syracuse, and its periphery, roughly measured as the remainder of Onondaga County. According to Miller, a subsequent transportation innovation, the railroad, had less impact on the county's economic structure in that it supplemented instead of replacing the canal system. In other words, the canal set the basic economic infrastructure within which all subsequent transportation innovations would have to operate.

The economic infrastructure Miller describes is one in which Syracuse dominates the county economy and becomes the political and cultural center. Dominance is exercised by strategic location along the Erie Canal and sets into motion a process of cumulative inertia by which one advantage leads to subsequent advantages. The form of dominance is illustrated by changing agricultural and migration patterns. With the establishment of Syracuse as the center of Onondaga County, farmers shifted land out of wheat production for national markets and into produce aimed more at the city market. Migration, too, fostered the growth of Syracuse as the hinterland experienced positive rates of emigration while migrants flowed into the city from all points. It is also noted that this dominance was not neutral, for, as Miller states, "There was a drain of capital and entrepreneurial talent to Syracuse from the hinterland" (p. 130). The growth of the city, therefore, was to some extent carried out at the expense of its own hinterland.

Two additional themes of importance are also made clear. First, Miller recounts the role national population growth and expansion played in the development of Onondaga County. This was a period of inexorable westward expansion drawing large numbers of people through Onondaga County; some of them settled in the area. In addition, as new areas were opened up in the West, a few competed directly with the agriculture and industry of the county. Second, and related to the first, Miller shows us that the economic development of Onondaga County did not proceed in isolation from changes occurring elsewhere on the national or subnational level. Even in its earliest days the county was decisively tied to eastern settlements. "In short, the stimulus to the growth of Syracuse was not solely—or even predominantly—events or pressures (demographic or economic) within the region. The growth of the city was the result of a complex of national forces and local reactions to them" (p. 155).

If one were to criticize this volume, it would be for its lack of sufficient theoretical statement. Even though *City and Hinterland* is purposely historical in nature, it would have benefited from paying more attention to the theory of urban and regional development, especially in the first and last chapters, which touch on issues of theory. A reader unfamiliar with the work of Eric E. Lampard, Allan R. Pred, and others would be unable to see the contributions this book can make to urban and regional development theory. The last chapter mentions several issues of theory raised by the historical analysis but fails to develop them fully. One is left, therefore, with a sense of work unfinished. Specifically, more attention to comparisons with other historical analyses, especially those dealing with the development of western cities, would have provided a stronger position for a theoretical contribution. If this had been done, it would have been possible to gain important insights into the conditions which affect the differential growth of urban regions at different times and places. In particular, the effects of differing initial transportation regimes could have been explored in more detail.

As a historical case study, *City and Hinterland* makes a useful contribution for the reader familiar with other work on urban regions. As such, it may be assigned fruitfully as supplemental reading in a graduate course concerned with urban and regional development or be used for comparative analysis of the growth of urban regions in conjunction with other case studies. For those not familiar with the field, the text remains readable but provides little indication of the theoretical contribution it is trying to make.



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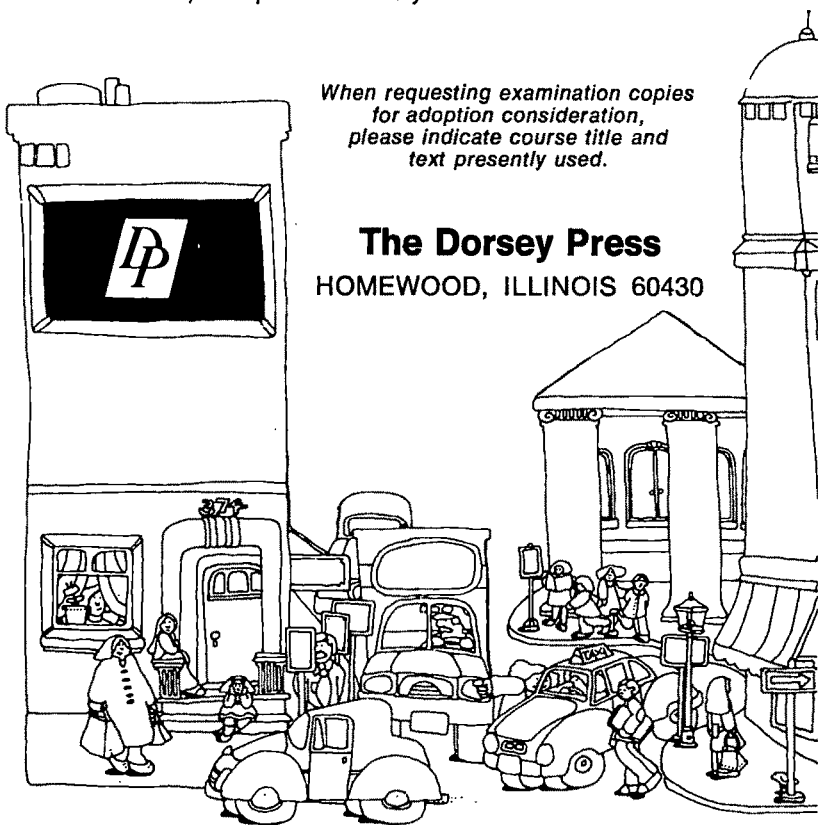
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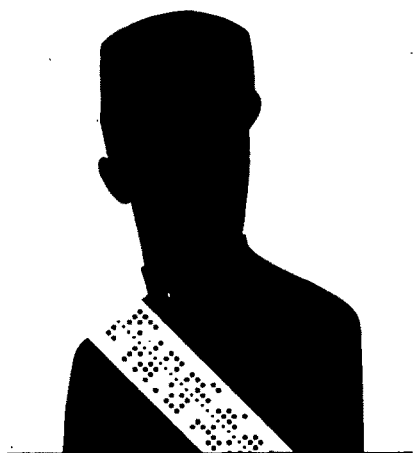
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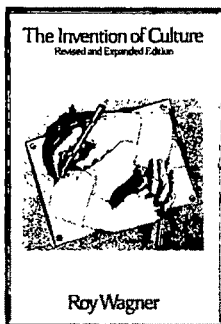
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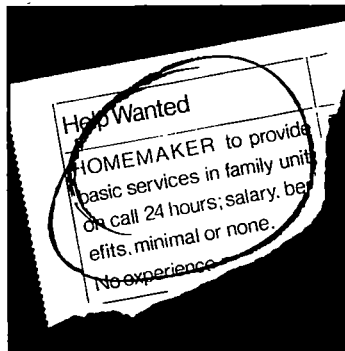
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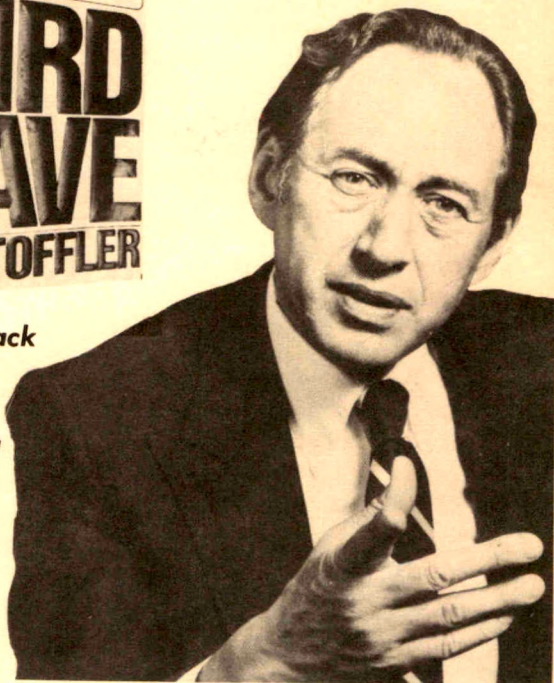
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IN THIS ISSUE

PETER V. MARSDEN is assistant professor of sociology at the University of North Carolina at Chapel Hill. He is interested in stratification, formal organizations, and network analysis.

W. RUSSELL NEUMAN is assistant professor of political science at the Massachusetts Institute of Technology. This article is part of a larger study of public opinion and political sophistication. His research interests include political sociology, public opinion, and mass communications.

LAWRENCE A. SCAFF, associate professor of political science at the University of Arizona, is engaged in research on the social and political thought of Max Weber.

WILLIAM G. ROY is assistant professor of sociology at the University of California, Los Angeles. His research interests center on historical sociology and long-term political transformations. He is currently working on a project concerning the internal organization of the capitalist class at the turn of this century, emphasizing the relationship between the rise of corporate capitalism and the expansion of the bureaucratic state.

JACQUES DELACROIX is senior associate of Associates for the Study of Society, Economy, and Trade. His major research interests include development and population ecology. Recent publications include his book, *Les Clefs du labyrinthe*.

CHARLES C. RAGIN is assistant professor of sociology at Indiana University. He is currently completing studies of strikes and of class and ethnicity in the world system. Also, he is starting a new project on the logic of comparative research.

LARRY ISAAC is assistant professor of sociology at Florida State University. He is currently involved in research which focuses on theories of state activity and its relationship to macroeconomic and distributional outcomes in the postwar United States. Other ongoing research includes projects dealing with the class/distributional consequences of racial discrimination and changing repertoires of black political contention in the United States.

WILLIAM R. KELLY is assistant professor in the Department of Sociology and a research associate at the Population Research Center, University of Texas at Austin. He continues work on racial violence in the United States, especially the consequences of the riots and explanations for why they ended. He is also working on analysis of fertility and family planning and the estimation of time-series models of fertility.

LARRY LYON is assistant professor of sociology and director of the Center for Community Research and Development at Baylor University. In addition to his research interest in community politics, he has published several recent articles on status attainment and mobility.

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Introducing Influence Processes into a System of Collective Decisions¹

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This paper modifies and extends the model of collective decision-making processes proposed by Coleman in *The Mathematics of Collective Action* by introducing a process in which the interests expressed by actors may be influenced by those of other actors. After incorporation of this individual-level process within the framework of Coleman's model, its effects at the system level are explored by comparing results generated by the modified model with those produced by the original model. Analyses of artificial data indicate that the nature and magnitude of system-level effects are contingent on (1) the pattern of interest differentiation in the influence network; and (2) the degree of centralization of the influence network. Depending on these features, the effects of the influence process on collective decision making may be (1) a decline in the system level of resource mobilization; (2) an increase in the level of apparent consensus on collective decisions; and/or (3) a bias in collective decisions toward the interests of the actors centrally located in the influence network.

Among the central problems in the development of a theory of action in sociology is the issue of how to integrate different levels of analysis (Coleman 1975, pp. 85-88), and to analyze consequences produced on one level by processes operating on another level. A field in which this problem is especially central is collective decision making, where there is concern with the consequences of individual-level processes of opinion formation or participation for emergent properties of a collectivity: the collective decisions it reaches, the levels of internal conflict it generates, or the aggregate levels of satisfaction or welfare it produces.

An important effort to address the problem of movement between levels of analysis in collective decision making was made by Coleman (1972, 1973b), who developed an exchange model of action in which actors pursue interests in events through exchanges of the control they possess over those events. After stating foundations for this model, Coleman traces the conse-

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quences of the individual-level processes of exchange, interest formation, and resource mobilization on the collective decisions made, on the levels of internal conflict, and on overall system efficacy in satisfying the interests of individual actors.

This paper modifies and extends Coleman's efforts by introducing a second individual-level process, a process of influence, into the model of action. This modifies two of the assumptions made by Coleman. First, permitting actors to influence the interests expressed by other actors weakens the implicit assumption of individualistic interest formation made in the basic exchange model. Second, a direct consequence of the modification is that another assumption of the basic model, the assumption that actors commit all of their available resources to the resolution of collective decisions, is weakened. Further consequences of the individual-level influence process occur at the level of the collectivity. In contrast to an analysis that does not consider an influence process, effects may be observed on levels of resource mobilization, on collective decisions reached, and on the apparent levels of consensus on those collective decisions. The nature and magnitude of these system-level effects are, however, contingent on the pattern of interactor influences and on the pattern of interest differentiation among actors.

The next section of this paper reviews the foundations of the basic model of action developed by Coleman in *The Mathematics of Collective Action* (1973b). The two following sections introduce the notion of an influence process and show how it can be incorporated within the framework of Coleman's model. Using artificial data, the subsequent section illustrates the system-level effects of considering the influence process and shows the ways in which these effects are dependent on the influence pattern. The section before the concluding summary discusses several issues raised by the modifications proposed in the body of the paper.

COLEMAN'S MODEL OF PURPOSIVE ACTION

In the exchange model proposed by Coleman, there are two sets of units of analysis, actors and events. These sets are related to one another by two types of asymmetric relationships. First, an actor possesses some degree of control over the outcome of an event. Second, events have consequences—positive or negative—for the well-being of actors. Anticipating these consequences, the purposive actor develops interests in the outcomes of events in proportion to the consequences they hold for him or her. These relationships between units of analysis are diagrammed in figure 1,A.

The basic postulate of purposive action embodied in the model is that actors will use the control they possess over events to pursue interests in one of two ways. Control may be committed directly to an attempt to

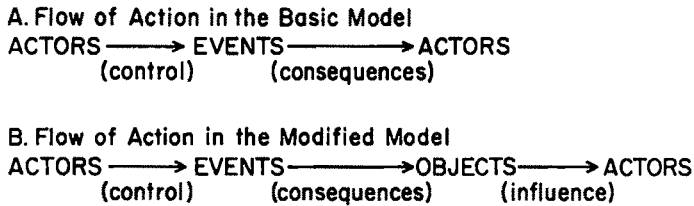


FIG. 1.—Two action sequences

secure an actor's preferred outcome for an event. Alternatively, if there is a poor correspondence among the events over which an actor possesses control and those in which he has interest, unwanted control may be exchanged for control over events that are more consequential to the actor.

The fact that the supply of control over an event is fixed while the demand for control typically differs among events means that the exchange process results in a relative value or "price" of control for each event. Actors are also stratified in this process according to their relative power: powerful actors are those possessing large amounts of control over valuable events. Finally, the model gives rise to predictions about the outcomes of events, when exchanges of control have reached an equilibrium point at which no pair of actors is willing to exchange any further. At this point, actors commit the control they hold over an event to their preferred outcome on that event. The likely outcome of the event, according to the model, is that to which the larger amount of control is committed.

These elements of the model can now be stated more formally. Let us consider a system composed of n actors and m events. The first task is to define the relationships linking these units of analysis.

The interest of actor j in event i is referred to as y_{ji} . Interests are signed according to outcome preference; the sign of an actor's interest in an event indicates the type of consequences—positive or negative—that a "positive" outcome on that event has for that actor. The magnitude of an actor's interest y_{ji} is referred to as x_{ji} . Corresponding matrices Y and X , of order $n \times m$, refer to the set of interests of all actors in all events and to the magnitudes of those interests. It is specified that each actor has a fixed quantity of interest divided among the different issues in the system. This means that the following constraints are imposed on the elements in the rows of Y and X :

$$\sum_{i=1}^m |y_{ji}| = \sum_{i=1}^m x_{ji} = 1.0. \quad (1)^2$$

Next, the proportion of control over the outcome of event i initially held by actor j is denoted by the symbol c_{ij} . The overall distribution of control

² These constraints also imply that the y_{ji} may take any value in the range $-1 \leq y_{ji} \leq 1$, while the x_{ji} must lie between 0 and 1.

over events among actors takes the form of a matrix C of order $m \times n$. The following constraint is imposed on the elements of each row of C :

$$\sum_{j=1}^n c_{ij} = 1.0. \quad (2)^3$$

As noted above, the basic postulate of purposive action included in the model is that actors utilize the resources they control in order to maximize their expected satisfaction of interest. To progress beyond this general postulate, however, requires description of features of the institutional structure specifying how decisions are to be reached, in order to be more precise about how the actors will utilize the resources they control.

Knowledge of the decision rule to be used in aggregating individual preferences into collective decisions is necessary to actors, in that different decision rules dictate different strategies for allocation of scarce resources to event outcomes. Several decision rules, including unanimity, majority rule, plurality rule, and dictatorship, have been discussed and partially analyzed (Arrow 1951; Buchanan and Tullock 1962; Coleman 1973*b*, pp. 141-51). The rule specified in the model considered here is a rather special one termed a "probabilistic decision rule" by Coleman (1973*b*). It specifies that preferences are aggregated through a chance mechanism which selects an issue outcome with a probability equal to the amount of power directed toward that outcome relative to other outcomes for that event.⁴

The conduct of the exchange process can now be described. The capacity of an actor to control an event outcome will not necessarily be consistent with his interest in that outcome, and the actor may therefore be motivated to exchange that capacity for control over an event of greater interest. In a system involving a large number of actors and several events, this possibility for the use of control results in a "market" for control over events, in which the value of control over an event is adjusted upward or downward until the supply of control made available by disinterested actors matches the effective demand for control put forth by interested actors. The rate at which the value of an event is adjusted is proportional to the difference

³ The definition of c_{ij} as a proportion means that the c_{ij} are intrinsically nonnegative and, hence, take values between 0 and 1.

⁴ The substantive importance of this rule is that it rules out a certain form of "free-rider" behavior (Olson 1965) as a rational strategy. Under majority rule, once a majority of the resources favors one outcome of an event, the direction of additional resources toward that outcome does not increase the probability of its occurrence. In such a circumstance, it is irrational for any actor to direct resources toward that outcome, irrespective of the magnitude of his interest in it. Under the probabilistic rule, no outcome is certain unless it is unopposed; hence, an actor can always increase the probability of an outcome by directing resources toward it. From a mathematical standpoint, use of the probabilistic rule increases the determinacy of individual resource allocations and, therefore, increases the tractability of the system.

between the supply of control and the demand for that control:

$$\frac{dv_i}{dt} = k(D_i - S_i), \quad (3)$$

where v_i is the value of event i , D_i is the aggregate demand for control over that event, S_i is the available control over the event, and k is an arbitrary constant.

The supply of control over an event is simply "the value of all control existing in the system for that event" (Coleman 1973b, p. 78). That is, using (2),

$$S_i = \sum_{j=1}^n v_i c_{ij} = v_i \sum_{j=1}^n c_{ij} = v_i. \quad (4)$$

To derive the demand for control over an event, it is necessary to have a notion of how actors allocate their limited power across events. An actor's power may be defined as the value of all the control he possesses:

$$p_j = \sum_{i=1}^m v_i c_{ij}, \quad (5)$$

where p_j is the power of actor j . The greater an actor's interest in an event, the greater the power he will wish to commit to that event. A high value or price for control over an event, however, will reduce its attractiveness relative to other possible choices. This means that an actor's allocation of power to an event—or equivalently the control held by that actor at the end of the exchange process—will vary directly with the magnitude of interest in that event and inversely with its value:

$$c^*_{ij} = \frac{x_{ji}}{v_i} p_j = \frac{x_{ji}}{v_i} \sum_{i=1}^m v_i c_{ij}, \quad (6)$$

where c^*_{ij} is the final control over event i possessed by actor j when exchange terminates.⁵ This means that the value of the actor's final control over event i , or that actor's demand for control over event i , is a fraction of the actor's total power equal to his interest in that event:

$$d_{ij} = v_i c^*_{ij} = x_{ji} p_j = x_{ji} \sum_{i=1}^m v_i c_{ij}, \quad (7)$$

where d_{ij} is the demand for control over event i by actor j . Actors, then, allocate power proportionately in accordance with the magnitudes of their

⁵ Coleman (1973b, pp. 82–88) explains that this allocation of power to an event in proportion to the ratio x_{ji}/v_i does not correspond strictly to a notion of maximization. He does note, however, that if we construe x_{ji} as the proportion of time during which event i is the sole concern of actor j , then proportional allocation of resources does correspond with maximization (cf. also Homans 1974, pp. 21, 31).

interests. Aggregated over all actors j , (7) gives the total demand for control over event i :

$$D_i = \sum_{j=1}^n d_{ij} = \sum_{j=1}^n x_{ji} \sum_{i=1}^m v_i c_{ij} . \quad (8)$$

We can now obtain an equation for the value of an event at the point where exchanges reach equilibrium. The equilibrium point occurs where demand equals supply for all events and the rate of change (3) is zero. By substituting (4) and (8) into (3), setting the result equal to zero, and rearranging, we obtain

$$v_i = \sum_{i=1}^m v_i \sum_{j=1}^n c_{ij} x_{ji} . \quad (9)$$

If we place the constraint

$$\sum_{i=1}^m v_i = 1.0 \quad (10)$$

on the total amount of value in the system, then the value of an event, given by (9), may be defined verbally as the fraction of all power in the system which is oriented toward event i at the close of the exchange process.

Also of concern is the relative power of the actors in the system. The verbal definition of value may be expressed formally as follows:

$$v_i = \sum_{j=1}^n p_j x_{ji} . \quad (11)$$

Substituting this into (5), we obtain an equation for the power of actor j :

$$p_j = \sum_{j=1}^n p_j \sum_{i=1}^m x_{ji} c_{ij} . \quad (12)$$

Since, from (5) and (10),

$$\sum_{j=1}^n p_j = 1.0 , \quad (13)$$

we may interpret (12) as the fraction of the total value in the system controlled by actor j .

The equations for value and power may be expressed compactly in matrix form. The set of m equations like (9) can be stated as

$$V = VCX , \quad (14)$$

where V is an m -element vector giving the value of each event. The set of n equations like (12) can be expressed as

$$P = PXC , \quad (15)$$

where P is an n -element vector giving the power of each actor. Solutions for value and power can be obtained directly from (14) and (15). The vector

quantities of concern, V and P , are given by the first left eigenvectors of CX and XC , respectively, extracted subject to the constraints on power and value given in (10) and (13).⁶

Finally, it is necessary to turn to the question of collective decision making itself and the event outcomes predicted by the model. Because of the assumption of a probabilistic decision rule, predictions cannot be deterministic but instead give the likelihood of each outcome of an event, given the distributions of control and interests. The power allocated to the positive outcome of an event is balanced against the power applied in favor of the negative outcome for that event. If these applications of power are identical, they cancel each other out, and the probability of each outcome is forecast as .5. Otherwise, the probability of the favored outcome increases by half the discrepancy in power applied. Since actors apply power to their favored outcome according to the ratio x_{ji}/v_i (see above, eq. [6]), this means that the probability of a positive outcome for each event is defined as

$$r_i = .5 + .5 \sum_{j=1}^n \frac{y_{ji}}{v_i} p_j, \quad (16)^7$$

where r_i is the probability of a positive outcome on event i . The probability of the negative outcome is of course $1 - r_i$.

These results, particularly those given in equations (14)–(16), are the primary results of Coleman's basic exchange model that will be of concern to us below. This basic model can now be modified to allow for the operation of an influence process that determines the interests expressed by actors in the exchange process.

THE RATIONALE FOR AN INFLUENCE PROCESS

The exchange model just discussed was developed from an individualistic viewpoint, in that it treats the interests of individual actors as exogenous to the operation of the system of action. Though the model is not explicit about how interests are generated, the fact that they are regarded as exogenously determined seems consistent with an assumption that interests

⁶ The right sides of (14) and (15) both involve a vector quantity postmultiplied by a square matrix. The same vector quantity appears alone on the right side of each of the equations. Each of them is therefore equivalent to an eigenvalue-eigenvector problem in which an eigenvalue of 1.0 implicitly appears on the left side of the equation. Because of constraints (1) and (2), all row sums of the two square matrices involved, CX and XC , must equal 1.0 and their first eigenvalues must equal 1.0. The fact that the vectors of power and value are eigenvalues of square matrices was also noted, in less detail, by Pullum (1975). Note that the approach given here is equivalent to the heuristic approach suggested in Taylor and Coleman (1979).

⁷ See Coleman (1973b, p. 90) for an alternative statement of this result. I have defined event outcomes in terms of a balance of power applied rather than a balance of final control applied; the distinction is only semantic. Note that the directed interest y_{ji} has been substituted for x_{ji} in moving from (6) to (16).

arise from individual background variables such as class or status-group memberships, individually held social attitudes or values, and other unspecified individual factors. Such a conception of interest generation, however, specifically excludes the influences of other actors as determinants of the interest defined by a given actor.

In general, I concur with the position on individualism taken by Homans (1974, p. 12). All observable action is taken by individuals, and we must refer to the motives and choices of individuals in explaining group-level outcomes and patterns of social relations. In explaining why individuals take the actions they take, however, it is necessary to refer to the structure of social relations present in a group (Collins 1975, p. 7). Once assembled, a structure of relations affects the actions taken by the individual actors composing it. It does so by constraining the set of actions available to individual actors (see Elster 1979) and by changing the dispositions of those actors toward the actions they may take.

The model discussed above does not include any interactor social structures that constrain the behavior of actors within them. The control and interest patterns do generate power-dependence relationships among actors (Coleman, 1973*b*, pp. 75-76; Emerson 1962; Marsden and Laumann 1977). These relations, however, play no part in the model independent of the structures generating them.

Marsden and Laumann (1977, pp. 235-37) refer to the absence of effects of interactor social structures in the purposive-action model as a problem of embeddedness. "Embeddedness" refers to the fact that exchanges and discussions within a group typically have a history, and that this history results in the routinization and stabilization of linkages among members (cf. Cook and Emerson's [1978] discussion of "commitment" in exchange networks). As elements of ongoing social structures, actors do not respond solely to individualistically determined interests. Instead, the interests they actually pursue may be constrained in important ways by the positions they occupy in these regularized social structures. Simply put, the interests of actors in a regularized structure of social relations are likely to affect, and be affected by, the interests of their intimate associates (Merton 1968; Harsanyi 1969).

It is a common finding in studies of social networks that the attitudes or behaviors of actors may be influenced by the attitudes or behaviors of their close associates. Such network effects have been demonstrated for political attitude formation (Lazarsfeld, Berelson, and Gaudet 1948), adolescent aspirations and ambitions (Duncan, Haller, and Portes 1971), the diffusion of innovations (Coleman, Katz, and Menzel 1966), and a variety of other social attitudes (Laumann 1973, chap. 5). We have no reason to doubt that similar processes may operate within most collective decision-making bodies; in fact, the persuasion involved in such influence processes may be

the essence of what Bachrach and Baratz (1963) term the "mobilization of bias" by those in control of communication channels and political institutions (see also Kingdon 1973). That is, a process of interest shaping by influential actors may serve to maintain a higher degree of consensus than would be apparent if interests were formed on the basis of individual characteristics only.

The existence of influence processes would seem to be something of an anomaly for a model of action based on the premise that actors pursue their interests in events rationally. An influence process involves one type of transfer of control (see Coleman 1973a, pp. 13-14); by permitting their interests to be affected by the influences of other actors, actors permit the use of their resources to be controlled indirectly by the interests of others. Such a circumstance appears to be patently inconsistent with the basic postulate of purposive action embodied in the model discussed above.

There are, however, certain circumstances under which such a transfer of control may be seen as a rational action. Among the most important of these are circumstances concerning (1) the availability of information about the consequences of an outcome of an event for an actor and (2) the certainty of the actor's assessment of those consequences (Lévy-Garboua 1979). Clearly, under conditions of "information impactedness" (Williamson 1975), with quite an unequal distribution of relevant information across interested parties, actors in a disadvantaged position will have little choice but to assess the consequences of possible actions in the light of the information made available by information "gatekeepers" (see Pettigrew 1972). By virtue of their control over information channels, the latter acquire influence over the interests pursued by others.

Additionally, under conditions of uncertainty about the consequences of an event, an actor may rationally seek information about those consequences. Social psychological studies show that these are the circumstances under which social influence is most effective. Festinger (1954) posits a drive to evaluate opinions that predisposes people to accept influence from others in the absence of external, "objective," standards of evaluation. Influence has been shown to be operative both when an individual is uncertain about the accuracy of his judgment (Weiner 1958; Verhagen 1979) and when the object of evaluation is ambiguous (see, e.g., Tajfel 1969). In general, conditions of uncertainty may cause actors to seek information which may influence their definition of interest—in the process using such cues as age or prestige to identify persons who are likely to have information relevant for them (see, e.g., Matthews and Stimson 1975).

A different set of conditions under which a group of actors might rationally accept the influence of others occurs when they have reached an *a priori* decision that their interests are best served if their resources are used in combination, either by a formally coordinated "corporate actor"

(Coleman 1973*a*, 1974) or by a more loosely defined "collective actor" (Laumann and Marsden 1979). In either case, several actors may commit themselves to accept direction from a formal or informal leader of a subgroup in order to maintain the power of the collectivity to which all of them belong, as in a legislative scheme of strict party discipline.

A final set of circumstances under which influence does operate, whether or not it is to be regarded as rational, has to do with personal satisfactions that an individual may obtain from an influence relationship. Harsanyi (1969) suggests that expressive commitments to others are an important basis for the operation of influence processes, and that the symbolic actions such commitments may entail are not to be regarded as irrational, as they may be important sources of psychological satisfaction to individuals. In a similar vein, Back (1951) suggests extrinsic reasons for group membership that permit the group to influence the individual, such as the liking of group members (which might well be interpreted as an "interest" in other persons in the group), or any honor or prestige that might be associated with group membership. Additionally, Tajfel (1969) argues that individuals may submit to influence from others owing to a generalized need for approval or fear of hostile interaction associated with disagreement.

Thus there is substantial support in the literature, both theoretical and empirical, for the existence of social influence processes. The next section modifies Coleman's model of collective action to allow for the operation of such processes.

INCORPORATING THE INFLUENCE PROCESS

The statement that an actor's attitude or behavior has been influenced implies that the actor would have thought or done something different in the absence of the influence. Influence may encourage actors to do things they would not have done otherwise or may restrain them from doing things they otherwise would have done.

The notion of influence, then, implies an analytic distinction between two concepts of interest. The first, which might be termed "intrinsic interest," refers to the goal an actor would have pursued in the absence of influence. Intrinsic interests are egoistically conceived and are presumably generated by the type of model suggested above, which considers individual characteristics and values. In the presence of influence, however, actors pursue goals at some variance with their intrinsic interests, because the direction of their activity has been shaped by the influence process. We can think of these manifest goals as "extrinsic interests," generated by dependencies on other actors as well as by individual characteristics.⁸

⁸ The terminology here is taken from Lévy-Garboua (1979). Initially, I had referred to intrinsic interests as "sincere interests" and to extrinsic interests as "expressed interests." The initially used terminology, however, could possibly confound the notion of an influence

This distinction between concepts of interest means that we must also analytically distinguish two modes of orientation to another actor. One actor may be viewed as attractive by others because they regard resources he controls as useful means for achieving their ends, or because they view him as an object of intrinsic interest or a source of needed information. The first mode of orientation is instrumental; an actor attractive in this sense acquires power in an action system owing to direct control over events. The second mode does not involve pursuit of control over resource bases or events. Instead, an actor who is attractive to others as an object of intrinsic interest or as an information source receives commitments implicitly from these other actors.

The presence of a commitment means that the actor making it will take the attitude or behavior of the actor receiving it into account in choosing his own attitude or behavior. That is, the receipt of commitments gives an actor a capacity to influence the goals pursued by the actors giving the commitments. This influence, however, is not based on an ability to confront these actors with countervailing resources. It operates as persuasion, shaping the goals pursued by actors directly controlling resources, and thereby constitutes a form of indirect control over the use of these resources.

To distinguish between these two modes of orientation to other actors, actors are regarded dually here. In their capacity of directly controlling events, they are "controlling actors." As potential recipients of commitments, and thus as potential sources of influence, they may be called "object actors."⁹

In the formal development below of the exchange model with an influence process, the symbols y_{ji} and x_{ji} refer to the intrinsic interests of object actors. Controlling actors express interests in collective decisions only as a function of the influences they receive from object actors. It is plausible to assume, of course, that one of the primary influences on the interest formation of a given controlling actor will be that actor's intrinsic interest. Thus, my modification does not suggest substitution of a model of interest generation ignoring individual characteristics for the model considering only

process discussed here with a process in which an actor deliberately misrepresents his preference for strategic purposes. I am indebted to Duncan MacRae, Jr., for remarks on this point.

⁹ The analytic distinctions between intrinsic and extrinsic interest, and between controlling and object actors, are necessary only in a case such as that considered here, in which actors may contribute to the formation of their own extrinsic interests. This process need not be viewed in this way: the set of actors controlling resources might be disjoint from the set of objects influencing interest formation. For instance, persons as actors might form their interests on the basis of commitments to organizations (Coleman 1974; Laumann, Galaskiewicz, and Marsden 1978) that have claims on their activity. All of the formal development below is predicated on the assumption that the sets of controlling actors and objects of orientation are identical. The generalization of this development to the case in which these sets do not coincide is, however, straightforward.

individual characteristics; the modification permits both interpersonal influence and individual characteristics to enter into the definition of a controlling actor's extrinsic interest.

The notion of extrinsic interest summarizes a disaggregation of the concept of interest into two components, intrinsic interest and influence.¹⁰ The foregoing discussion indicates also that the modification introduces a third set of units of analysis, object actors. In the modified model, it is assumed that controlling actors control events, that these events have consequences for object actors, and that object actors influence the actions taken by controlling actors. The flow of action in the revised model is diagrammed above in figure 1, B.

Let us now formally incorporate this decomposition of interest into the action system described above.¹¹ To begin, the influence of actor k as an object actor on actor j as a controlling actor can be defined as the relative effect of k 's intrinsic interest on j 's extrinsic interest. Influence thus defined is denoted by the symbol q_{jk} . The set of coefficients q_{jk} can be compactly summarized as a matrix Q of order $n \times n$.¹² The definition of influence as a relative effect means that the following constraint is imposed on the influences affecting each actor j :

$$\sum_{k=1}^n |q_{jk}| = 1.0. \quad (17)$$

Condition (17) refers to the magnitudes of the influence coefficients q_{jk} and thus implies that the q_{jk} are signed and may vary from -1 to 1 . Object actors, therefore, may be influential in one of two ways. First, they may acquire an influence capability through positive commitments. This is the usual conception of influence as persuasion. Second, they may influence controlling actors "negatively"; then a controlling actor becomes more likely to express a preference contrary to that of the object actor. Such a situation might arise, for instance, when the object actor is known to be well informed but has regularly opposed the controlling actor in the past.

The extrinsic interest of controlling actor j in collective decision i can now be defined as a linear combination of the influences received by j and the intrinsic interests of object actors having those influences in event i . This gives

$$y^*_{ji} = \sum_{k=1}^n q_{jk} y_{ki}, \quad (18)$$

¹⁰ For a related decomposition of the concept of control, see Marsden and Laumann (1977) or Feld (1977).

¹¹ The modification presented here is similar to one suggested by Coleman (1977) involving the "investment" of controlling actors in objects and the "importance" of objects. I have carried out the formalization here somewhat further than Coleman does.

¹² If Q is an identity matrix, we have individualistic interest formation and the model is for practical purposes identical to the basic exchange model discussed above.

where y^*_{ji} is the extrinsic interest of controlling actor j in event i .¹³ The $n \times m$ matrix of extrinsic interests thus defined is denoted Y^* . Like the y_{ji} , the y^*_{ji} have a possible range of $-1 \leq y_{ji} \leq 1$. Let us refer to the magnitude of an extrinsic interest y^*_{ji} as x^*_{ji} and to the set of coefficients x^*_{ji} as a matrix X^* of the same order as Y^* .

It is important to note, however, that a constraint similar to (1) on the magnitudes of these extrinsic interests does not hold. Instead, we have the result that for each controlling actor j ,

$$\sum_{i=1}^m x^*_{ji} = \sum_{i=1}^m \left| \sum_{k=1}^n q_{ik} y_{ki} \right| \leq 1.0. \quad (19)$$

This result occurs because some influences on the formation of an extrinsic interest may be contradictory; they may thus cancel one another out in equation (18). The only circumstance under which (19) will be a strict equality is the case in which a controlling actor receives consistent influences from all sources on all events. This occurs when, for each event, all object actors positively influencing a particular controlling actor have the same outcome preference, while all object actors emitting negative influence to that controlling actor hold the opposite outcome preference.

The implication of this result for the exchange model is quite important. Equations (6) and (16) specify that actors proportionately allocate power to obtain control over events. The quantity x_{ji} is indicative both of the relative consequences of event i for actor j and of the proportion of actor j 's power devoted to the pursuit of control over event i (see above, eq. [7]). Since, owing to constraint (1), magnitudes of interests in the basic model sum to 1.0, proportional allocation means that actors utilize all of their potential control in the process of resolving events in the basic model. Complete resource mobilization, however, does not necessarily occur in the modified model.

If we maintain the assumption of proportional allocation, result (19) means that some power held by actors will not be directed toward collective decisions when controlling actors receive inconsistent influences from object actors. The power controlled by actor j not committed to collective decisions will be

$$p_j \left(1 - \sum_{i=1}^m x^*_{ji} \right). \quad (20)$$

The addition of an influence process, then, means that a basic finding of work dealing with participation in political processes (Lazarsfeld et al.

¹³ This is a quite elementary conception of an influence process. One could imagine a more involved process in which influences were exchanged among actors several times, perhaps until an equilibrium point was reached. Instead of defining matrix Y^* as QY as in (18), Y^* might be defined as Q^2Y , Q^3Y , etc. For present purposes, however, the conception given in (18) is sufficient.

1948; Lipset 1960, pp. 211-26) and social participation more generally (Lenski 1956) is drawn into the structure of the purposive action model. This finding is that actors subject to cross-pressures of one kind or another are less likely to participate in collective decisions than people who receive consistent signals from their social environments. Equation (20) gives the extent of nonparticipation in the present context.

In order to analyze the effect of the influence process on the operation of the system of collective decision making, it is necessary to obtain equations parallel to (14)-(16) for the value of events, power of actors, and outcomes of events. Essentially, what is involved here is the substitution of the extrinsic interest x_{ji}^* for the intrinsic interest x_{ji} . A complication appears, however. It concerns the way in which the resources withdrawn from the pursuit of control over collective decisions as a consequence of the influence process are to be treated in the calculation of the power of actors and the value of events. In order to obtain the necessary results, one must be specific about the meanings of the terms power and value.

Above, the power of an actor was conceived as the amount of system value controlled and committed to collective decisions by that actor. Because it was assumed that actors commit all of their resources to the resolution of collective decisions, it was not necessary to draw a distinction between actual control exercised and potential control. Such a distinction, however, becomes quite important when we consider the possibility that some resources will not be committed to collective decisions.

Several authors (Dahl 1958; Anton 1963; Clark 1968; Nuttall, Gordon, and Scheuch 1968; Mott 1970) define power as the potential to control events, leaving aside the question whether that power is utilized as a problem of resource mobilization. If we accept such a definition of power, the question whether an actor actually uses such power in collective decisions becomes unimportant for present purposes. It also means that the assumption about relative power made in equation (13) can be maintained.

The value of an event, however, should not reflect potential. Indeed, it is difficult to imagine the meaning of "potential value." An event will have value only to the extent that actors actually commit their resources to controlling that event. If some power is not directed toward collective decisions, the total value of collective decisions will be less than the total amount of power in the system.

In the basic model, the power of an actor is a function of his position in a network of dependency relations. An actor less dependent on most other actors than those actors are on him has a relatively powerful position in the system (Coleman 1973b, pp. 74-76). The dependency z_{jk} of actor j 's interests on resources controlled by actor k is (see above, eq. [12])

$$z_{jk} = \sum_{i=1}^m x_{ji} c_{ik} \quad (21)$$

in the basic exchange model. Because of constraints (1) and (2), z_{jk} is necessarily the proportion of j 's interest realization controlled by k , and thus

$$\sum_{k=1}^n z_{jk} = 1.0. \quad (22)$$

In the modified model, we substitute the absolute value of equation (18) for x_{ji} in equation (21), obtaining

$$z_{jk}^* = \sum_{i=1}^m x_{ji}^* c_{ik}, \quad (23)$$

where z_{jk}^* is the dependency of j on k in the modified model. The sum of these dependencies pertaining to a particular actor, however, will no longer obey constraint (22). This is because some proportion of actor j 's interest, and therefore of his resources, is directed away from the collectively controlled decisions, as indicated by equation (19).

If, however, the power of an actor is to be defined as the potential value of the resources he controls, we should not calculate power on the basis of the dependencies z_{jk}^* , which assume partial mobilization of resources. Instead, we should calculate the power of actors as if all resources were committed to collective decisions. This, of course, raises the question of how withdrawn resources are to be hypothetically allocated across collective decisions. Consistent with the assumptions made in the basic model, let us assume for purposes of calculating power that the withdrawn resources are allocated in proportion to the magnitude of each actor's extrinsic interests. This means that the hypothetical dependency z^{**}_{jk} of actor j on actor k is defined as follows:

$$z^{**}_{jk} = (z_{jk}^*) / \left(\sum_{k=1}^n z_{jk}^* \right) = \sum_{i=1}^m (x_{ji}^* c_{ik}) / \left(\sum_{i=1}^m x_{ji}^* \right). \quad (24)$$

Substituting (24) for (21) in (12), we obtain the result, parallel to (15), that

$$P = PZ^{**}. \quad (25)^{14}$$

¹⁴ One alternative to the solution for potential power given in (24) and (25) is to define n new pseudo-events $m+1$ to $m+n$ in such a way that event $m+j$ is individually controlled by and individually consequential to actor j (see Coleman 1973b, pp. 62-63). The pseudoevents could be defined in such a way that

$$c_{m+j,j} = 1, j = 1, \dots, n;$$

and

$$c_{m+l,j} = 0, l \neq j,$$

and

$$x_{j,m+j}^* = \sum_i x_{ji}^*, j = 1, \dots, n;$$

and

$$x_{j,m+l}^* = 0, l \neq j.$$

This defines a set of private events, controlled by single actors who are singly interested in the events to the extent that they withdraw resources from collective decisions. This alternative was considered and rejected. It yields implausible results; the power of an actor

This yields a solution for the relative potential power of actors. Powerful actors are again defined as those in control of resources in high demand; they tend to surrender relatively low proportions of their valued control in return for relatively high proportions of the resources of other actors, and thus enjoy relatively favorable "terms of trade" with the other actors. The difference from the basic model described above is that not all of this control is necessarily applied to the resolution of collective decisions.¹⁵

Given a solution to equation (25), we can compute value directly, since the results for the power of actors thus obtained imply results for other vector quantities in the model (see Feld 1977). Valuable events are those in which powerful actors express interest:

$$V = PX^* \quad (26)$$

Because value reflects only resources actually committed to collective decisions, the v_i determined using (26) will not obey constraint (10). To refer to the actual amount of value in the modified system, the concept of the system level of resource mobilization is introduced. The system level of resource mobilization will reflect the proportion of potential power which is actually devoted to the pursuit of control over collective decisions, the

rises with the relative quantity of resources $x_{j,m+j}^*$ withdrawn from collective decisions. This may be because the alternative solution creates a separable interevent dependency matrix (Coleman 1973b, p. 80), making it impossible to compare the value of event $m + j$ with that of event $m + l$. In any case, for theoretical purposes it is preferable to evaluate power as if all resources were devoted to collective decisions, instead of basing it on the control of private events.

¹⁵ There are, of course, many reasons besides the presence of cross-pressures in an influence process that might be given for incomplete resource mobilization. One important way of explaining "resource leakage" (Marsden and Laumann 1977, pp. 237-38) is given by Olson (1965). Olson's theory can be viewed as a "threshold" theory of resource mobilization which assumes the existence of a certain amount of "friction" which must be overcome in the course of activation in pursuit of an interest. Since the friction is constant, irrespective of the magnitude of an actor's interest in an event, it will be overcome more frequently by actors with very strong, concentrated interests in particular events. Such actors will nearly always make efforts to pursue their interests, while actors with smaller, more diffusely distributed interests will be activated much less frequently. To think of this in the context of the model of a system of action, we can assume the existence of an interest threshold x_{crit} below which actors do not pursue control over a collective decision. Actors whose intrinsic interest magnitudes x_{ji} in an event fall below x_{crit} would be assumed (a) to put forth no demands for control over that event in the exchange process; (b) to purchase no control over the event at the value determined by the exchange process; and (c) therefore to devote no power to influencing the outcome of that event in the event resolution process. Such a modification would affect the outcomes of events predicted by the model when the configuration of interests is of the type investigated by Olson. Such a configuration is one in which, for example, a small number of actors have a highly concentrated interest in the positive outcome of an event, while a much larger number of actors have a relatively small interest in the negative outcome. In a system with complete resource mobilization, the outcome preferred by the majority of small interests might be expected to be the one chosen. When these small interests fall below the threshold x_{crit} , however, the concentrated interests would assert themselves and the opposite outcome would prevail.

remaining power being slack or oriented to private events (see eq. [20]). Denoting this new concept by the symbol λ , we have

$$\sum_{i=1}^m v_i = \lambda. \quad (27)^{16}$$

The predicted outcomes of events in the modified model can be obtained using the results above for power and value. Substituting (18), (25), and (26) for the corresponding quantities in (16) gives

$$r_i = .5 + .5 \sum_{j=1}^n \frac{y_{ji}^*}{v_i} p_j. \quad (28)$$

Finally, the addition of the influence process in the system of action makes it necessary to define a new characteristic of actors, their status. Typically, the extension of commitments and the consequent influence capability will be stratified among the object actors in the system. Some object actors will be able to positively influence more controlling actors than others. The most effective influences, in terms of their capacity to make the realization of the object actor's intrinsic interests likely, will be influences on powerful actors. The status of an actor as an object of intrinsic interest can be defined as the degree to which that actor's intrinsic interests are pursued by controlling actors. Basically, high-status actors are those capable of influencing positively the formation of extrinsic interests by powerful controlling actors. We denote the status of actor k by the symbol t_k . The vector T of status scores may be obtained using the influence matrix Q and the result of equation (25):

$$T = PQ. \quad (29)^{17}$$

I now proceed to an analysis of the effects of adding an influence process

¹⁶ Note that (27) differs from a concept in the basic model outlined by Coleman, the total directed power of the collectivity (see Coleman 1973b, pp. 94-96). The total directed power of the collectivity reflects the extent of consensus in the system at the issue resolution stage; it is an inverse function of the overall level of conflict of extrinsic interest. The concept λ defined in (27), on the other hand, refers to the extent to which resources are used in the issue resolution process and is an inverse function of the level of inconsistency in the influence process, which may be due to several sources—conflict of intrinsic interest, reception of influence from actors with different intrinsic interests, reception of both positive and negative influence from actors with the same intrinsic interest. Total directed power and the system level of resource mobilization may vary independently of one another, with the constraint, of course, that total directed power cannot exceed the system level of resource mobilization.

¹⁷ For individual object actors k , we have $t_k = \sum_j p_j q_{jk}$. Because the object actors may be both positively and negatively influential, the t_k will not, in general, obey an equality like $\sum_k t_k = 1.0$. Such an equality will hold, of course, if influences of each object actor k are all positive, as in the examples presented in the next section. For some purposes it may be useful to disentangle an actor's status as a persuader—status due to positive influence—from that actor's "negative" status due to negative influence, by summing separately the positive ($q_{jk} \geq 0$) and negative ($q_{jk} < 0$) components of the equation above for the status of object actor k .

to the purposive action model. The effects of the influence process can be appraised by comparing results obtained for the modified model with those for the basic model.

THE EFFECTS OF INTERACTOR INTEREST DEPENDENCY

Four types of consequences may occur as a result of introducing an influence process into the system of collective decision making. There may be (1) a decline in the system level of resource mobilization, in comparison with the model that does not include an influence process; (2) changes in the predicted outcomes of events and in the certainty of or consensus on these outcomes; (3) realignment of the relative value of events; and (4) change in the distribution of power among actors. Additionally, the influence process introduces a new form of indirect control—status derived through influence—for actors. The specific consequences of the influence process in any given system of action will be contingent on the way in which an influence network is articulated with the distributions of control and intrinsic interest in that system.

This section analyzes the consequences of incorporating the influence process. First, the relevant results of the modified model are reinspected in order to state the conditions under which particular consequences might be anticipated. Second, the analysis is illustrated using a hypothetical three-event, seven-actor system of action, systematically varying the patterns of intrinsic interest, control, and influence. An analysis of the system using the basic model provides a standard of comparison.

Tables 1, 2, and 3 present the patterns of intrinsic interest, control, and influence, respectively, to be used in the illustrative analysis. Panel A of table 1 first presents an interest distribution in which there is little consistency in the preferences of actors across events, and then an interest distribution in which the interest of an actor in one event is highly predictable given knowledge of that actor's interest in a second event (see correlation matrices in panel B of table 1). Table 2 presents two distributions of control. In panel A, all actors have identical shares of control over each of the three events and, hence, have equal amounts of power. In the other control distribution, shown in panel B, control is unequally distributed across actors, and some (e.g., actor 2) are clearly more powerful than others (e.g., actor 1).

Table 3 shows three different patterns of influence. For simplicity, the presentation is limited to cases in which actors emit only positive influences. Two of the influence patterns are systematic. In the centralized pattern (table 3A), a single central actor, actor 4, is influential in determining the interests expressed by all other actors. This relationship is asymmetric; actor 4 accepts no influences from other actors, and hence has identical

intrinsic and extrinsic interests. In the polarized pattern (table 3B), transmission of influence is confined within each of two cliques of actors. Mutual influence relations are present between actors in a clique, but there are no influences between actors in different cliques. When the division in terms of clique membership corresponds to a division in terms of outcome preference on an event (e.g., event 1 in the inconsistent interest distribution, or all events in the consistent interest distribution), the polarized pattern corresponds to the "oppositional" model of communication contacts proposed by Laumann, Marsden, and Galaskiewicz (1977, pp. 606-7; cf. also Coleman 1957).

TABLE 1
INTRINSIC INTEREST DISTRIBUTIONS (Y)
A. INTEREST DISTRIBUTIONS

OBJECT ACTOR	EVENT		
	1	2	3
Inconsistent Distribution			
1.....	+.600	-.300	-.100
2.....	+.500	+.300	-.200
3.....	+.300	+.300	+.300
4.....	-.333	+.333	-.333
5.....	-.200	-.200	+.600
6.....	-.500	-.400	+.100
7.....	-.300	+.500	-.200
Consistent Distribution			
1.....	+.650	+.340	-.010
2.....	+.450	+.320	-.230
3.....	+.450	+.220	-.330
4.....	-.383	-.180	+.437
5.....	-.150	-.140	+.710
6.....	-.450	-.220	+.330
7.....	-.350	-.260	+.390

B. INTERCORRELATIONS AMONG INTERESTS

EVENT	EVENT		
	1	2	3
Inconsistent Distribution			
1.....	1.00		
2.....	.06	1.00	
3.....	-.06	-.48	1.0
Consistent Distribution			
1.....	1.00		
2.....	.98	1.00	
3.....	-.80	-.84	1.0

TABLE 2
CONTROL DISTRIBUTIONS (C)

EVENT	CONTROLLING ACTOR						
	1	2	3	4	5	6	7
A. Egalitarian							
1.....	.143	.143	.143	.143	.143	.143	.143
2.....	.143	.143	.143	.143	.143	.143	.143
3.....	.143	.143	.143	.143	.143	.143	.143
B. Inegalitarian							
1.....	.021	.268	.121	.043	.201	.177	.170
2.....	.056	.400	.010	.176	.021	.140	.198
3.....	.030	.330	.124	.054	.067	.165	.230

TABLE 3
PATTERNS OF INFLUENCE (Q)

CONTROL- LING ACTOR	OBJECT ACTOR						
	1	2	3	4	5	6	7
A. Centralized Pattern							
1.....	.500	.000	.000	.500	.000	.000	.000
2.....	.000	.500	.000	.500	.000	.000	.000
3.....	.000	.000	.500	.500	.000	.000	.000
4.....	.000	.000	.000	1.000	.000	.000	.000
5.....	.000	.000	.000	.500	.500	.000	.000
6.....	.000	.000	.000	.500	.000	.500	.000
7.....	.000	.000	.000	.500	.000	.000	.500
B. Polarized Pattern							
1.....	.500	.250	.250	.000	.000	.000	.000
2.....	.250	.500	.250	.000	.000	.000	.000
3.....	.250	.250	.500	.000	.000	.000	.000
4.....	.000	.000	.000	.500	.250	.250	.000
5.....	.000	.000	.000	.167	.500	.167	.167
6.....	.000	.000	.000	.167	.167	.500	.167
7.....	.000	.000	.000	.000	.250	.250	.500
C. Random Pattern with Mutuality Bias*							
1.....	.600	.200	.000	.000	.000	.000	.000
2.....	.125	.375	.125	.125	.125	.000	.125
3.....	.000	.250	.750	.000	.000	.000	.000
4.....	.000	.200	.000	.600	.200	.000	.000
5.....	.000	.200	.000	.000	.600	.000	.200
6.....	.000	.000	.000	.167	.167	.500	.167
7.....	.000	.200	.000	.000	.200	.000	.600

*Of 11 different random networks that were constructed and used, one is shown here for illustrative purposes.

In contrast to these two systematic influence patterns, table 3C exhibits a randomly generated network of influence relations, constructed to include the mutuality bias present in most sociometric data (e.g., Davis 1968; Holland and Leinhardt 1981; Fienberg and Wasserman 1981).¹⁸ To permit some generalizations about the effect of this type of influence structure, 11 different random networks were constructed; I shall present results for the specific pattern shown in table 3C and aggregate results for the set of 11 random patterns.

Let us begin by considering the factors affecting the system level of resource mobilization. Using (18) and (26) we can rewrite (27), which defines the system level of resource mobilization, as follows:

$$\lambda = \sum_{j=1}^n p_j \left(\sum_{i=1}^m \left| \sum_{k=1}^n q_{jk} y_{ki} \right| \right). \quad (30)$$

This shows that the system level of resource mobilization in the modified model is largely an inverse function of the degree to which cross-pressures appear in the influence process. For example, when there are few cross-pressures, the quantity in parentheses in (30) will be relatively large, and λ will approach its maximum of 1.0. Equation (30) also shows that λ will be lowered when powerful, rather than powerless, actors are subject to highly inconsistent influences.

What conditions generate a minimum of cross-pressures and, hence, are necessary for a high level of resource mobilization? One relevant feature is the pattern of interest differentiation. If there is low consistency of interest in events across actors, then most influence networks will generate a number of cross-cutting relationships; influence relations that reinforce a focal actor's intrinsic interest in one event will frequently conflict with his intrinsic interest in another. So high resource mobilization often requires issue consistency; this is not, however, sufficient to guarantee that λ will be large. It must also be the case that most influence relations serve to reinforce an actor's intrinsic interest. This occurs when positive q_{jk} join actors with similar outcome preferences, while negative q_{jk} link actors with different outcome preferences. When a consistent pattern of interest differentiation appears in conjunction with such an influence pattern, resource mobilization will be high. If, on the other hand, a consistent pattern of interest differentiation occurs in the context of an influence pattern in which positive influences flow between actors with opposed interests, or negative influences join actors with the same interests, resources will be withdrawn from all events simultaneously, and λ will be quite low, often lower than when there is inconsistency in the interest distribution.

¹⁸ The random networks do not include the "expansiveness" and "popularity" biases found in many sociometric networks (Holland and Leinhardt 1981; Fienberg and Wasserman 1981). The practical impact of this omission is that status differentiation among actors is more limited than it would be were such biases included.

There is one exception to the generalization above concerning the necessity of consistency in the intrinsic interest distribution for high resource mobilization. This exception concerns a circumstance in which there are very few sizable influences q_{jk} entering the summation in parentheses on the right of (3) for each actor j . In the limiting case, if each controlling actor is influenced by only a single object actor (e.g., the actor himself, as in the basic model, or a central actor, in a more extreme version of the centralized influence pattern than the one in table 3A), full resource mobilization will occur irrespective of the pattern of interest differentiation.

The effects on resource mobilization are illustrated in table 4A. Note first that resource mobilization is maximal when the polarized pattern of influence occurs in the presence of a consistent interest distribution; the cleavages in terms of outcome preference match the division between cliques. Note also that the centralized pattern generates comparatively small reductions in resource mobilization; here, all actors receive influence from two sources at most, and the possibilities for cross-pressures are more limited than they are for the other influence networks.

The random networks generate the largest reductions in resource mobilization in comparison with the basic model. The most interesting result here, however, is not the extent of this reduction per se. Notice that the standard deviation of the level of resource mobilization for the 11 models with random influence networks is much higher with the consistent interest distribution than with the inconsistent one. This is so because of the point made earlier; with the consistent interest distribution, if the influence network creates cross-pressures for one event, it creates them for all events. So with the consistent interest distribution, λ is either relatively high, with the polarized pattern as the limiting case, or relatively low. The inconsistent interest distribution, on the other hand, may generate few cross-pressures for one event, while creating many for another. Thus it is associated with a level of resource mobilization with a comparatively low dispersion.

Let us consider next the impact of the influence process on the outcomes of events. Comparing (16) and (28), it can be seen that outcomes may be altered to the extent that the influence process alters the distribution of power in the system, or to the degree that the process causes the balance of extrinsic interests on an event to differ from the balance of intrinsic interests. We shall see below that the power distribution is altered only indirectly, and typically in a minimal way, by the influence process. We are thus led to consider the effect of the influence process on the distribution of interests as the major factor generating differences in the outcomes of events between the basic and modified models.

Equation (18) relates intrinsic interests to extrinsic interests through the influence process. The conditions under which the balance of intrinsic interests differs from the balance of extrinsic interests can be seen again to

depend on the nature of the influence network and the way in which that network and the pattern of intrinsic interest differentiation are linked.

The most obvious case here is illustrated by the asymmetric, centralized influence pattern (see table 4B). Here, actors whose intrinsic interests are opposed to those of the central actor are cross-pressured into withdrawing their resources, while actors with intrinsic interests similar to those of the central actor have those interests reinforced and magnified as extrinsic interests. The result is a dramatic shift in the outcomes of events toward the preferences of the central actor. In the case of event 3 in the consistent interest distribution, the effect of the network is strong enough to remove all opposition to the preferred outcome of the central actor, actor 4.

TABLE 4
RESULTS FOR LEVELS OF RESOURCE MOBILIZATION AND EVENT OUTCOMES
A. SYSTEM LEVELS OF RESOURCE MOBILIZATION (λ)

INFLUENCE PATTERN	CONTROL, INTEREST DISTRIBUTIONS			
	Egalitarian, Inconsistent	Egalitarian, Consistent	Inegalitarian, Inconsistent	Inegalitarian, Consistent
None (basic model)	1.000	1.000	1.000	1.000
Centralized (table 3A)633	.677	.694	.635
Polarized (table 3B)558	1.000	.587	1.000
Random (table 3C)555	.686	.508	.633
Random (mean for 11)503	.523	.509	.510
Random (SD for 11)077	.133	.081	.143

B. OUTCOMES OF EVENTS (r_i)*

EVENT AND INFLUENCE PATTERN	CONTROL, INTEREST DISTRIBUTIONS			
	Egalitarian, Inconsistent	Egalitarian, Consistent	Inegalitarian, Inconsistent	Inegalitarian, Consistent
1:				
None (basic model)529	.538	.535	.524
Centralized (table 3A)158	.122	.160	.086
Polarized (table 3B)527	.542	.526	.554
Random (table 3C)577	.576	.518	.517
Random (mean for 11)608	.622	.617	.619
2:				
None (basic model)614	.524	.723	.536
Centralized (table 3A)978	.183	.979	.205
Polarized (table 3B)773	.528	.798	.543
Random (table 3C)785	.555	.886	.492
Random (mean for 11)755	.602	.849	.626
3:				
None (basic model)546	.766	.430	.698
Centralized (table 3A)099	1.000	.058	1.000
Polarized (table 3B)856	.769	.811	.740
Random (table 3C)647	.835	.596	.888
Random (mean for 11)609	.912	.520	.871

* Quantity shown is probability of a positive outcome on each event under the given conditions. Probability of a negative outcome is equal to 1 minus the quantity shown.

When divisions in the influence network match cleavages of intrinsic interest, event outcomes are affected very little, since interests are reinforced in the influence process. Any changes in outcomes that do occur in such circumstances are due to the differential magnitudes of interests of actors influencing one another about an event. This can affect the relative amounts of power applied in favor of one outcome or the other. These conditions are illustrated by the polarized network in conjunction with event 1 of the inconsistent interest distribution, as well as with all events in the consistent interest distribution. Conflict here proceeds with virtually the same intensity and uncertainty found in the basic model.

Finally, let us consider conditions in which influence relationships are mutual, unlike the centralized pattern, and in which relations cut across outcome preferences, unlike the polarized pattern for the events noted. This sort of articulation of preferences with relations is not infrequently found in empirical data on decision making (Laumann et al. 1977). Typically, under such circumstances, as in (18), the outcome more frequently favored in intrinsic interests will be more frequently taken into account in the determination of extrinsic interests. As a result, the balance of the extrinsic interest distribution shifts, giving increased prominence to the outcome favored by intrinsic interests. Thus, in circumstances involving extensive cross-pressures, we should expect a closer approach to unanimity in collective decisions reached (cf. French 1956). This corresponds well with experimental results from social psychology showing increases in the levels of group consensus after discussion and influence attempts are permitted (Back 1951; Tajfel 1969; Zaleska 1979).

For the most part, the results in table 4 do indicate that addition of the influence process serves to increase the level of apparent consensus on event outcomes in situations where actors are subject to cross-pressure. This holds true for events 2 and 3 of the inconsistent interest distribution and for most events in both interest distributions analyzed in conjunction with the random influence patterns. The increase in consensus is larger when the distribution of intrinsic interests, as indicated by the event outcome in the basic model, is skewed toward one outcome or the other, because actors holding the less favored preference are more likely to be cross-pressured into withdrawal.¹⁹

The results in table 4 contain one exception to the generalization just stated. This involves event 3, in the analysis using an inconsistent interest

¹⁹ Compare, for instance, the mean outcome for the 11 models using random networks with the outcome in the basic model for events 2 and 3, in the system with the consistent interest distribution and the egalitarian control distribution. For event 2, the positive outcome is slightly favored ($r_2 = .524$); the mean r_2 for the 11 random networks rises by about .08, to .602. On event 3, with greater consensus in intrinsic interests, the increase in consensus is also greater (r_3 in basic model = .766; mean r_3 in 11 models with random networks = .912, an increase of about .15).

distribution and an inegalitarian control distribution. Here, the negative outcome is more favored in the basic model, but the positive outcome tends to be more favored when cross-pressures are created by the polarized and random networks. This is due to the combination of the highly concentrated interest of actor 5 in the positive outcome, with the concentration of control over the event in actors 2 and 7 (see tables 1 and 2); when the influence process causes the latter actors to withdraw resources from this event, the very strong intrinsic interest of actor 5 asserts itself.²⁰

The alterations in the outcomes of events just discussed are largely a result of the way in which the influence pattern is joined with the outcome preferences of object actors in (18). Changes in the relative values of events, on the other hand, can be traced to the linkage between influence relations and the magnitudes of interests in events (cf. eq. [11] and [26]). Many of the conditions mentioned previously can result in realignment of values. Thus, with the centralized pattern, the relative values of events come to resemble the magnitudes of intrinsic interest of the central actor. In the case of an inconsistent interest distribution, the polarized network serves to increase the value of events on which there are few cross-pressures. When the polarized network matches up with cleavages in a consistent interest distribution, the relative values of events remain much the same, as they do also in the case of the random influence networks (though as indicated above, in the latter case the overall amount of value in the system is reduced). Illustrative results are given in table 5A.

The source of an actor's power is control over valuable events. This makes others dependent on that actor if they are to realize interests. The influence process does not change the distribution of direct control over events. By altering the relative demands for control over events, however, the introduction of that process may indirectly affect the distribution of power, if control over events is differentially allocated among actors. With an egalitarian control distribution, the introduction of an influence process cannot affect the distribution of power at all. With an inegalitarian control distribution, a realignment of relative value like that shown in table 5 for the polarized influence pattern can result in alterations in the distribution of power. In table 5B, we see that actor 2 has less power in the model with the polarized network than in the basic model, because of the devaluation of events 2 and 3, in which the resources of actor 2 are concentrated. Actor 5, specializing in control over event 1, gains power by virtue of the increase in relative value of that event.

As far as event outcomes are concerned, any redistribution of power that

²⁰ See n. 14 for related material. Though the mean r_3 for the 11 models with random networks exceeds r_3 in the basic model, and is hence contrary to my generalization, it should be noted that the outcomes in the results are highly dispersed around this mean. In five of the 11 trials, the probability of the negative outcome increases as predicted by the generalization, while in six r_3 increases contrary to prediction.

occurs as a result of the influence process is less important than the introduction of discrepancies in status among actors (see table 5B). High-status object actors are able to shape the extrinsic interests of controlling actors through the influence process and thus obtain indirect control over events. The clearest differentiation of status, of course, is shown by the model with the centralized influence pattern, in which all influence relations are asymmetric.

TABLE 5
ILLUSTRATIVE RESULTS FOR VALUE OF EVENTS,
POWER OF ACTORS, AND STATUS OF ACTORS, IN
SYSTEM WITH INEGALITARIAN CONTROL, INCON-
SISTENT INTEREST

A. VALUE OF EVENTS (v_i)				
EVENT	INFLUENCE PATTERN			
	None (Basic Model)	Centralized (Table 3A)	Polarized (Table 3B)	Random (Table 3C)
1.....	.409	.222	.405	.223
2.....	.347	.258	.100	.196
3.....	.243	.215	.083	.089

B. CHARACTERISTICS OF ACTORS				
ACTOR	INFLUENCE PATTERN			
	None (Basic Model)	Centralized (Table 3A)	Polarized (Table 3B)	Random (Table 3C)
Power of Actors (p_j)				
1.....	.035	.037	.028	.036
2.....	.329	.335	.298	.331
3.....	.083	.081	.103	.078
4.....	.092	.095	.066	.097
5.....	.106	.094	.152	.106
6.....	.161	.160	.169	.160
7.....	.194	.198	.183	.192
1	Status of Actors (t_k)*			
	None (Basic Model)	Centralized (Table 3A)	Polarized (Table 3B)	Random (Table 3C)
1.....018	.114	.063
2.....166	.182	.230
3.....041	.133	.100
4.....548	.087	.126
5.....047	.167	.189
6.....080	.172	.087
7.....092	.145	.204

* The concept of status does not appear in the basic model. Implicitly, this means that intrinsic interest and extrinsic interest are the same, since each actor entirely determines his extrinsic interest; it also implies that status is equivalent to power in the basic model.

In this section, the consequences of considering influence processes have been illustrated. Some key variables that affect the different system properties have been discussed. There are, of course, many other features of possible influence networks that might be considered (see Mitchell 1969). Nonetheless, the variables used here—the extent of consistency in the interest distribution, the extent of asymmetry in the influence network, the degree to which cleavages in interest match divisions among cliques in an influence network, the degree of equality in the distribution of control—are likely to remain among the most crucial conditions specifying the effect of the influence process on the system of action.

DISCUSSION

The foregoing analysis indicates that the influence process introduced in equation (18) may have a number of interesting consequences in the model of a system of action. We have seen that some of these consequences correspond to observations made about actual or experimental decision-making groups. This section considers some alternative conceptualizations of the influence process and their consequences and comments on a related treatment of topics covered here.

One alternative way to view the influence process is as a transfer of direct control, and thus as a decomposition of the concept of control, instead of interest, in the basic model.²¹ In this view, influence is a process in which controlling actors transfer proxies to “deciding actors,” who then apply the proxies in the issue resolution process in accordance with their interests. Such a conceptualization of decision making and influence might be appropriate, for instance, to decisions made by shareholders at annual meetings of corporations. Because this view of influence cannot involve a transfer of “negative control,” it avoids the problems of cross-pressures on actors and incomplete resource mobilization.

While this alternative conception has some appeal for the analysis of certain types of decision making, it also has drawbacks. It does not force us to consider the question of the bases for and processes involved in interest formation, as does the process used above. The alternative view does not accord with the fact that in many cases actors do retain control over resources while being subject to extensive influence (e.g., in a system of strict party discipline in which parties or party leaders serve as the object actors). Finally, the alternative conception permits controlling actors to transfer control to deciding actors with opposed interests. It is certainly possible to construct a rationale, as has been done above, for why actors might accept influence in interest formation from several, possibly opposed,

²¹ This conceptualization of influence as a decomposition of control rather than of interest was suggested to me by one of the anonymous referees.

sources. It is rather more difficult to provide reasons behind a controlling actor's decision to transfer proxies to deciding actors with opposed preferences. If controlling actors do not transfer proxies to deciding actors with opposed interests, then for practical purposes the alternative view can be seen as a special case of the conception proposed above (e.g., with an influence pattern in which each controlling actor receives influences from a single source only).

While retaining the view that influence is part of a decomposition of the concept of interest rather than of control, others might nonetheless dispute the results here that indicate a decline in the level of resource mobilization in the revised system of action. These results depend quite heavily on the assumption of proportional allocation of power to controlling the outcomes of events. An alternative conceptualization of the influence process suggests that actors "proportionally reallocate" resources from events for which they are subject to extensive cross-pressures to events on which they receive less ambiguous influences. This suggests defining extrinsic interests as

$$y^{**}_{ji} = \left(\sum_{k=1}^n q_{jk} y_{ki} \right) / \left(\sum_{i=1}^m \sum_{k=1}^n |q_{jk} y_{ki}| \right), \quad (31)$$

rather than via equation (18). As with the first alternative view mentioned, this would eliminate reductions in resource mobilization but leave undisturbed most other consequences found to follow from the influence process.

I prefer the view of the influence process in (18) to that in (31), despite the fact that some may see the withdrawal of resources ensuing from (18) as irrational. The primary basis for this preference is that the results of assuming (18) are in accord with empirical observations and theorizing about the ways in which actors respond to situations involving cross-pressure. Lipset (1960) surveys literature on voting participation that indicates that persons subject to cross-pressure are less likely to use their political resources. Janowitz (1978) indicates that the increasingly complex pattern of social stratification has made it more difficult for individuals to calculate their self-interests and links this phenomenon to the recent decline, over time, in electoral participation. Additionally, there is a substantial literature on the formally similar condition of role conflict (Stryker and Macke 1978); withdrawal from the situation is one of the responses often used by individuals as a solution to the cross-pressures in such situations. Thus, my preference for the influence process (18) over the alternative (31) is based on the fact that (18) models accurately one of the sources of the resource leakage that does occur in empirical political systems (see Dahl 1961; Marsden and Laumann 1977), whether or not this leakage is seen as rational.

It should be noted that there are some conditions under which influence processes may create only limited resource leakage. Kingdon's (1973) study

of congressional voting decisions finds the attitudes of fellow congressmen to be the most important predictor of the way in which individual voting decisions are made. The study notes, however, that congressmen take pains to consult with like-minded colleagues, thus minimizing the possibility of cross-pressures that make decision difficult. Coleman's (1957) classic study of community conflict outlines the way in which an influence structure can be altered over time in such a way that cross-pressures are eliminated and resource mobilization is heightened. Thus, if cross-pressures are dealt with through transformation of the influence pattern via the blocking out of one or more of the inconsistent stimuli, resource leakage due to cross-pressure may be reduced (Pinner 1968).

One modification of influence process (18) that retains its implications for participation levels while weakening somewhat the rather stringent conditions under which complete resource mobilization may occur is the alternative of event-specific influence processes. Use of this alternative weakens the assumption, implicitly made above, that the channels through which influence flows are the same for all events. This means that extrinsic interests are defined as follows:

$$y^*_{ji} = \sum_{k=1}^n q_{jki} y_{ki}, \quad (32)$$

where q_{jki} is the influence of object actor k on controlling actor j for event i . With influence process (32), the presence of an inconsistent interest distribution does not require incomplete resource mobilization; the influence networks for the different events need not produce cross-pressures. The use of (32) is possibly too great a weakening of the assumption of a single influence process made by (18); it may be more appropriate to utilize arena-specific influence processes governing interest formation for a specific class of similar events (e.g., economic issues, civil rights issues). This is in accord with observations that patterns of consulting among congressmen do seem to differ by issue arena (Kingdon 1973).

In two recent papers, Burt (1977, 1979) treats topics related to those considered above. In discussing relational equilibrium in a network, Burt (1979) develops a model in which interaction is treated as a "good," and actors express interests in interaction with certain other actors or with actors substitutable for those others. Because of the possibility that interaction with one actor can be substituted to fulfill demand for interaction with another, Burt distinguishes between interest and "compound interest," a distinction that parallels the contrast of intrinsic and extrinsic interest.

Though there are some formal similarities between these treatments, there are important distinctions as well. Burt's approach, concerned with interaction as a good, deals essentially with private events, while my concern lies with collective decisions. As a result of this difference, Burt con-

siders only positively signed interests; he considers events having quantitatively, rather than qualitatively, defined outcomes. This, in turn, means that his analysis leads neither to the conclusions shown above about lowered levels of resource mobilization nor to the conclusions about the ways in which the influence process may affect the outcomes of events. Another point on which the present approach and Burt's differ concerns the mechanism used in decomposing the notion of interest. Burt generates substitutability relations among actors on the basis of their structural equivalence and uses these relations in moving from an actor's interest to his compound interest. In the approach used here, the mechanism differentiating extrinsic from intrinsic interest is the presence of an influence relation, not necessarily a similarity of attributes or relations, among actors. Finally, Burt's analysis, being concerned with substitutability of actors and with relational equilibrium in a network, does not introduce the concept of status as a distinct form of control held by actors.

In another paper, however, Burt (1977) does introduce a concept similar to that of status, which he refers to as "power as influence." Not employing a unidimensional conception of status like that discussed above, Burt's approach permits consideration, if necessary, of several dimensions of status based on a network of influence relations.²² Both Burt's approach and mine contribute to explaining why, beyond the fact that they control valued resources, centrally located actors in a network tend to be especially prominent, in that they assume leadership roles, have reputations as influential actors, and participate in collective action (see, e.g., Coleman et al. 1966; Laumann and Pappi 1976; Galaskiewicz 1979). The difference between the treatments is that the approach used here explicitly incorporates influence as a form of indirect control over the use of resources into a model describing event resolution processes and traces the system-level consequences of this modification of Coleman's basic exchange model.

This paper has made an effort to impose one type of social structural constraint on the operation of the purposive-action model. Further work should attempt to introduce additional constraints of social networks. A particularly important area where this might be done concerns the assumption that actors have unrestricted access to one another in the process of exchange prior to the resolution of events. One might attempt to introduce effects of a social network specifying the pairs of actors who may contact one another directly in the exchange process. Such a revision would modify the operation of the exchange process itself, instead of altering one of the inputs to that process as this paper has done.

²² Obviously, the use of issue- or arena-specific influence networks suggested above would require a multidimensional conception of status.

SUMMARY

This paper has modified one of the implicit assumptions on which Coleman's (1973b) system of collective decisions is founded: the assumption of exogenous, independent interest formation. It has done so by introducing an individual-level process in which one of the inputs to the system, the pattern of interests expressed in events, is altered prior to the beginning of the exchange process. This involves consideration of an actor's position in an influence network and his consequent ability to pursue individually defined interests.

We have seen that the modification introduced has a variety of effects on the system of collective decision making. First, the weakening of the assumption of independent interest formation has the consequence of weakening a second assumption of Coleman's model, the assumption of complete resource mobilization. Effects on the system level may also be observed on the outcomes of events, the value of events, and the power of actors when results are compared with those which would be obtained if intrinsic interests were pursued in the absence of the influence process. The nature of these effects is contingent both on the pattern of influence relationships connecting actors and on the pattern of interest differentiation among actors. Finally, the modification introduces a new way in which actors can affect collective decisions. They can affect outcomes of events indirectly, by influencing the goals, or extrinsic interests, pursued by other actors, instead of only via direct control over event outcomes.

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Differentiation and Integration: Two Dimensions of Political Thinking¹

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Conceptual differentiation refers to the number of discrete elements of political information individuals utilize in their evaluation of political issues. In contrast with the more commonly used textbookish political knowledge indices, this measure corresponds more closely to knowledge-in-use. Conceptual integration is defined as the spontaneous and explicit organization of ideas and information in terms of abstract or ideological constructs and represents an expansion of Philip Converse's research on levels of ideological thinking in mass publics. These two related dimensions of political information processing emerge from a detailed content analysis of depth interview transcripts. The analysis reveals substantial variation in the way citizens relate the condition of their own lives to those of their fellow citizens and to political authorities. As expected, education plays a central role in explaining these patterns, but there are some surprising interactive linkages between education and patterns of political thought. One especially intriguing finding is that conservatives have significantly lower scores than liberals on indices of differentiation and integration. The ramifications of these findings for survey research methodology and theories of mass political behavior are discussed.

It has been noted that belief systems have never surrendered easily to empirical study and quantification (Converse 1964). In fact, empirical work in this field may have crossed a watershed when two sets of researchers working independently derived rather similar measures of political sophistication—the irony being that the two measures were scored in opposite directions.²

Given that one researcher's sophistication is another's simplistic think-

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² The comparison is between the INDSICAL measure of Marcus, Tabb, and Sullivan (1974), which attributes higher sophistication to those who use a greater number of dimensions of judgment, and the numerous other constraint measures based on Converse's work, which associate sophistication with the use of a single, abstract, liberalism-conservatism dimension for conceptualizing political issues.

ing, we may benefit from an attempt to rethink the dominant approach to measurement which relies so heavily on inferences from correlation matrices of political opinion items. The present study puts forward an alternative approach to the measurement of patterns of political thinking based on a rigorous content analysis of the natural language of political discourse. The study involves 137 hour-long, loosely structured depth interviews concerning national politics. Each interview was tape-recorded and transcribed. Trained coders combed through the transcripts recording each spontaneous reference to a political object or issue and the linkages the respondent made between them. There was striking variation in both the number of political references made and patterns of linkage. Many interviewees repeatedly responded to political questions in strictly personal terms. The primary finding of the study was the identification of two complementary dimensions of political thinking: *conceptual differentiation*—the ability to identify and discriminate among the various political issues, actors, and events which jostle each other for attention in the news media; and *conceptual integration*—the explicit organization of political ideas and issues in terms of abstract or ideological constructs. Before turning to a more detailed description of the research design and findings, however, it may be helpful to review briefly the correlational research tradition in the study of political opinion structure which predicates this research.³

MEASURES OF POLITICAL ATTITUDE STRUCTURE

The seminal article in this field is clearly Converse's "The Nature of Belief Systems in Mass Publics" (1964). Expanding on central findings of *The American Voter* (Campbell et al. 1960) and Herbert McClosky's work (McClosky, Hoffmann, and O'Hara 1960) on different styles of political thinking in political elites and masses, Converse contrasted the level of organization of political opinions in a sample of Congressional candidates and a national cross-sectional sample. The research was organized around the concept of constraint, defined as "the success we would have in predicting, given initial knowledge that an individual holds a specific attitude, that he holds certain further ideas and attitudes." He went on to explain, "If a person is opposed to the expansion of Social Security he is probably a conservative and is probably opposed as well to any nationalization of private industries, federal aid to education, sharply progressive income taxation and so forth" (1964, p. 207). Converse's measure of constraint

³ Readers familiar with the recent flurry of attention in the scholarly literature to the methodology of measuring attitude structure and the possibility of a shift over the past decade in the level of attitude constraint in the mass public may wish to move ahead to the next section.

was the average interitem correlation coefficient for a set of survey items concerning prominent political issues. The constraint in belief systems for the elite group was found to be twice that of the mass sample (Goodman and Kruskal τ coefficients of .53 and .23, respectively). Converse was cautious not to interpret the data as evidence that his elite respondents were more "logical" in their thinking. But clearly the higher level of constraint was seen as an indication of cognitive sophistication. "In our estimation, the use of such basic dimensions of judgment as the liberal-conservative continuum betokens a contextual grasp of politics that permits a wide range of more specific idea-elements to be organized into more tightly constrained wholes. We feel, furthermore, that there are many crucial consequences of such organization: With it, for example, new political events have more meaning, retention of political information from the past is far more adequate, and political behavior increasingly approximates that of our sophisticated 'rational' models, which assume relatively full information" (1964, p. 227).

Converse went on to discuss a broad array of related issues and alternative approaches to measurement, but the correlational measures of constraint have attracted the most attention and have recently become the basis for an intense debate in the journals over two issues: (1) Are the beliefs of the mass public really significantly less constrained than the elite's? (Luttbeg 1968; Brown 1970; Bennett 1975; Farah and Miller 1974) and (2) Has the pattern of low constraint and sophistication among mass publics changed since the quiescent Eisenhower era? (Pomper 1972; Nie and Andersen 1974; Bennett 1973; Nie, Verba, and Petrocik 1976; Miller et al. 1976; Popkin et al. 1976; and RePass 1976).

The use of interitem correlations as an indicator of sophisticated cognitive structure did not pass without serious and sustained criticism.⁴ Recently, Popkin et al. (1976), Bishop and Oldendick (1978), Sullivan, Pierson, and Marcus (1978), and Petrocik (1978), among others, have noted that the correlations used in this literature are extremely sensitive to changes in question format. Since item formats change dramatically from survey to survey, comparison across studies and over time is extremely problematic. In addition, RePass has made the point that surveys in which more of the items have common referents exhibit higher interitem correlations. He notes, for example, that one recent election study had three items referring to Vietnam, which may have artifactually increased the apparent constraint in the foreign policy area (1976, p. 829).

These are important technical problems for which, we hope, technical solutions will be found. But there exists a much more difficult and fundamental issue: Is opinion constraint really a valid indicator of ideology or

⁴ Characteristic of this critique are Brody and Page (1972), Kessel (1972), Popkin et al. (1976), and RePass (1976).

cognitive sophistication? If efforts to reduce and control for measurement error in constraint indices are successful, will we be in possession of a theoretically meaningful instrument or simply a highly refined measure of some other phenomenon?

Our discussion thus far has touched on two distinct constructs for which constraint has been used as an indicator—ideology and cognitive sophistication. Both carry considerable intellectual baggage. Ideology is one of a number of frequently used concepts with frustratingly diverse and multiple meanings. Minar (1961), Putnam (1973), Mullins (1972), Bergmann (1951), and Johnson (1968), among others, have put together definitional lists. Drawing on their detailed compilations and discussion it is possible to identify four primary components of the construct: ideological thinking is (1) politically oriented—most public events and issues are perceived and interpreted in political terms; (2) structured around abstract concepts—cognitive links are made between specific issues and abstract theoretical principles; (3) closed—opinions are rigid and resistant to new (especially contrary) information; and (4) emotionally charged.

Political sophistication has also been defined and operationalized in diverse ways by different scholars, but most definitions emphasize the first two components identified above: a political orientation and the structuring of political thought through the use of abstract concepts.⁵ Our attention here will focus on this overlap between the sophistication and ideology constructs.

It seems to make strategic sense at the current stage of inquiry to maintain a clear distinction between the definitional components of ideology and sophistication and to pursue unique measures of each. One individual may study history and politics and after a thoughtful review of issues and events come to an “ideological” position. In contrast, another may simply be repeating slogans and abstractions absorbed uncritically from friends and associates. The componential approach allows us to explore empirically why an “ideologue” may be more or less sophisticated and why a sophisticated observer of politics may be more or less “ideological.”

This brief review of the components of the ideology and sophistication concepts may help illustrate why constraint measures tend to make rather awkward indicators of either. Constraint measures require the assumption that increased correlational constraint between a number of specific issue items indicates that the opinions have been deduced from more abstract principles. There are, however, numerous potential sources of constraint other than abstract thinking. For example, in his original article Converse

⁵ The breadth of definitions and measures of political sophistication is rather striking (cf. political involvement, Berelson, Lazarsfeld, and McPhee [1954]; political information, Lane and Sears [1964]; political cognition, Himmelstrand [1960]; political competence, Almond and Verba [1963]; political rationality, Shapiro [1969]).

elaborates the distinction between logical, psychological, and social sources of constraint. He notes that while Americans may have absorbed the notion that "communists are atheists, very few may understand the historical and philosophical roots of such an observation and may well be repeating an often-heard phrase or simply associating 'communists' with everything wicked and evil" (1964, p. 212).

TWO DIMENSIONS OF POLITICAL THINKING

If the correlation-constraint approach to measuring political attitude structure dominates the literature, it is less a result of the method's proven validity than of its methodological convenience—every opinion poll and survey of more than one item offers another opportunity for analysis. A fruitful inquiry into the nature of belief systems, however, requires a more sensitive approach to measurement, one that allows the respondent the opportunity to structure his or her own beliefs rather than simply respond to a sampling of prescaled, fixed alternatives.

Robert Lane's (1962) study of political ideology based on extended depth interviews and the detailed study of respondents' natural language offers one model for research. But such research is prohibitively expensive, and 10–20 hours of interviewing per respondent is simply impractical for larger samples. Converse's (1964) content analyses of respondents' comments on why they might vote for or against the major political parties and their presidential candidates offer another, more viable model for large-scale research. But his method focuses only on the current candidates and campaign and may fail to tap very deeply into a potential wealth of political thinking and experience on which citizens could comment if given the opportunity.

This study seeks a middle ground, a viable, general-use, and theoretically grounded measure of cognitive structure in mass publics which draws on the strengths of the work of both Converse and Lane and maintains the distinctions between the various components of the ideology and sophistication constructs. Two dimensions of analysis emerged from a careful reading of their parallel inquiries and the related literature concerning the structure of mass political cognitions—conceptual differentiation and integration.

For our purposes conceptual differentiation is operationally defined as the number of discrete, concrete elements of political information the individual utilizes in the course of an hour-long depth interview. It is an indication of a political orientation—the interpretation of issues and events in political terms. It is also akin to political knowledge, but actual knowledge would be better measured by a focused exam. Conceptual differentiation might better be described as knowledge-in-use. We focus on patterns

of cognitive discrimination, the ability and inclination of the individual to identify and separate the various issues, political figures, units of government, interest groups, events, and social trends. Only spontaneous, volunteered references to a specific issue or political entity are coded in this measure.

Intuitively, one would expect that an undifferentiated view of politics would be self-perpetuating because without a certain minimum awareness of basic political processes and institutions, political news from television and newspapers would be a meaningless and confusing jumble of strange words, unfamiliar faces, and vaguely familiar reporters standing in front of buildings in Washington.

The second dimension of analysis, conceptual integration, is complementary to the first and reflects the other common component of sophistication and ideology—the use of abstract concepts in the structuring of belief elements. It is complementary in the sense that an individual must differentiate elements of the political domain to some minimum degree in order to have elements to integrate. Conceptual integration is operationally defined as the spontaneous and persistent use of abstract concepts to structure beliefs and opinions in the course of the depth interviews on American politics.

The complementarity of the concepts of differentiation and integration may be useful in trying to understand the process by which some citizens come to have a full and sophisticated understanding of the political process and others do not. Growth in political sophistication seems to involve a spiraling back and forth between an increasingly differentiated understanding of the political process and more frequent use of abstract anchoring concepts to put the discrete pieces of information in some kind of manageable and accessible order. This new structuring of the political domain in turn allows the individual to assimilate, retain, and interpret further political information. As an analogy, it might be helpful to imagine what a game of chess looks like to the uninitiated—the chess board a confusing array of strangely shaped pieces which jump and zigzag around until someone miraculously wins. Gradually, however, through observation and the asking of an occasional question the observer becomes able to differentiate the pieces and their characteristic movements and ultimately to understand how the individual movements fit together into unified strategies and styles of play. The notion of a spiraling process between differentiation and integration in an individual's acquisition of knowledge in a particular sphere has numerous antecedents in the fields of education, psychology, and political sociology (Piaget 1952; Bruner, Goodnow, and Austin 1956; Zajonc 1968; Schroder, Driver, and Streufert 1967; Berelson, Lazarsfeld, and McPhee 1954; Gardner and Schoen 1962; Whitehead 1929).

There are some strong suggestions that the variables of conceptual differentiation and integration in the political sphere may have some natural discontinuities, some cutting points of special significance for political behavior. One possibility, with roots in Marx's conception of the lumpen-proletariat (1852) and survey research's version of know-nothingism (Hyman and Sheatsley 1947), is the idea of a self-perpetuating and unmoved bottom stratum of individuals who successfully defend themselves from any increasing interest in or information about politics. Another cutting point that suggests itself at the higher end of the integration dimension is a working understanding of the liberal-conservative continuum, which seems to be a requirement for the successful processing of the daily outpouring of political information from the news media. To address these issues, we turn to an analysis of the depth interviews.

THE BAY AREA SURVEY DEPTH INTERVIEWS

A series of transcribed hour-long depth interviews on attitudes toward the political system conducted by the Berkeley Survey Research Center in 1972 offers a special opportunity to refine a new measure of political conceptualization. The original purpose of these interviews was to validate several new scales of political alienation-allegiance and possible dissatisfaction with the quality of life. Data on a battery of over 500 closed-ended items had been collected in a previous interview and a self-administered questionnaire. The depth-interview technique of measurement validation was explained to respondents, and then the interviewers proceeded to review a number of broad questions on politics, allowing the respondent to set the pace and tone of the interview.

Several characteristics of the Bay Area Survey depth interview make it an especially attractive medium for exploring political conceptualization. Initial questions were diffuse and general, allowing the respondent to define the salient issues. But there were also extensive follow-up probes to clarify, for example, whether individuals dissatisfied with their economic situation blamed themselves, their boss, or the political or economic system in some way for their fate.

After respondents were given ample opportunity to mention issues and events, a number of the more prominent issues of the day were raised by interviewers, including economics, crime, race relations, the environment, and the quality of education. A particularly interesting section of the interview probes the respondent's thoughts on some rather abstract principles of politics, including political freedom, equality, democracy, and the legitimacy of political institutions in America. Interviewers were instructed to probe and challenge each comment in an attempt to bring out whatever reasoning lay behind the various opinions, thus presenting

an excellent chance to explore patterns of logic and the individual's ability to organize facts and ideas.⁶

The interviews averaged about an hour in length and were typed in full from tape recordings, resulting in single-spaced transcriptions which averaged about 20 pages in length. Four advanced graduate students in the fields of political science, sociology, and law were recruited to code the transcripts for patterns of political conceptualization.

The first task was to count and code each spontaneous reference to a political object or issue. The unit of analysis was a passage, that is, the original question and response and the one or two follow-up probes concerning the same topic. Some were brief and involved yes, no, or I-don't-know responses. Other passages dealing with high salience issues ran several pages in length.

The coders were looking for references to common identifiable political issues such as unemployment or high taxes, and the mention of political figures, groups, general constituencies, events, and, of course, units of government. When such references were made the first coding decision was whether the statement was, in fact, volunteered or whether the respondent was simply repeating a term or issue raised by the interviewer.

Once a volunteered reference was located, the second step in the coding process was to establish that the reference was made in a political context. This was often the most difficult part of the process. Take, for example, a respondent raising the issue of crime. If the reference was to "increasing crime in the streets, the government ought to do something about it," or if it concerned lenient judges or an unworkable penal system, it was obviously political in nature. If instead the comment involved an incidence of crime in which the respondent or a relative was personally involved, it was not clear whether the individual actually saw the issues as a social or political problem requiring the coordinated response of the community. The key analytic concept here is "supraindividuality." Thus if an event or object was seen by the respondent as being caused by or requiring the response of more than one individual, it was judged to be a political reference. This was not a hard and fast coding rule. The coder had to make each decision in the context of the particular interview. References to such clearly political entities as Congress or the Constitution and the use of such terms as "socialism" or "free speech" were automatically coded as political references.

The final step in the coding process was to insure that references to specific objects and issues were counted only once. Our interest is in the number of distinct political objects and issues mentioned by the respondent and not in the frequency with which various issues were raised. Coders

⁶ An outline of the interview schedule can be found in the Appendix.

transcribed the issues and terms on special coding sheets to insure that each was counted only once even though it might be referred to at several points in the interview.⁷

CONCEPTUAL DIFFERENTIATION

The key notion underlying measurement of conceptual differentiation is specificity. How many specific political issues, actors, and events will a respondent bring up in the course of an hour-long interview? Among the 137 respondents there was an impressive range of from 1 to 94 political references made in the course of the interview. The average was 26.7, the standard deviation, 16.5.

One might justifiably ask how an hour-long interview on politics can be conducted without a respondent's mentioning more than one political object. The answer is straightforward enough. The respondents talk about themselves. Their mode of thinking, it turns out, is overwhelmingly self-centered and concrete. Are they satisfied about the way things have been going in this country? Their response concerns their job, family, friends, and neighbors. Each politically oriented probe elicits a response which reflects only the individual's own life. A probe about the respondent's economic situation elicits comments on the price of bread at the market last week or a decision to put off buying a new TV. Questions concerning racial problems may elicit a detailed description of the "black lady who was elected head of our PTA." There is no reference to social or political causes or consequences.

Such patterns of thought which translate all political and social questions into personal ones, however, are not the modal response. The political discourse of most people reflects some mixture of social and personal concerns. The analysis proceeds, then, to explore this mix. What kinds of political objects are most salient to the mass citizenry? Are there some distinctive clusters and patterns of political discourse?

Political issues.—The coin of the realm in the political speech of the mass citizenry is clearly the "political issue"—a topical policy question or cluster of policy questions usually identified in the media and interpersonal discussion by a key term or phrase such as "busing," "taxes," "civil liberties," "crime in the streets," or "the energy crisis." It was not necessary that respondents actually take a position on each issue or that they mention a particular key term. A respondent need only raise the issue in some way. Typically the flow of the interview would go as follows:

Q: Could you tell me some of the things about America you're well satisfied with?

⁷ More detailed information about the coding process and scoring of the indices of conceptual differentiation and integration is available from the author.

A: Well, I'm glad to see we're *out of Vietnam*, and it looks like latest announcements are that we might try to solve our *trade deficit problems*, I think also *racial relations problems*, I think maybe we're making progress there. . . .

In this case the original question is very broad, basically asking the respondent to list issues which are salient to him.

In another pattern, the question raises a general issue area such as the environment, and the respondent translates that key term into more specific issues that are meaningful to him.

Q: How about the environment? Are you satisfied with the quality of the environment around here?

A: I think we're moving in the right direction toward the environment to try to *restrict automobile traffic* into San Francisco, for example. It's interesting that many of the new office buildings are being built without any new parking facilities whatsoever. It's a step to encourage people to take *mass transit* and BART [Bay Area Rapid Transit]. . . .

If this respondent had answered simply that he thought the environment was getting better or worse, he would not have been credited with raising an issue because in such a case he would only be responding directly to the question. The essence here is not having an opinion, but being aware of currently discussed policy questions. In one case an elderly gentleman mentioned prohibition, which is not at the moment, in most circles, a hotly debated topic. This case, accordingly, was coded as a reference to a historical event. On the average, respondents volunteered references to about 10 issues in the course of the depth interview.

A distinction was made between specific and general issues. In order to qualify as a specific issue the reference had to concern a particular bill or proposal recently considered by the voters or a legislative body. Examples of specific issues would include a school bond referendum, a proposed new freeway, or a bill in Congress. As one might expect, most references were to more general issues, at the rate of about four to one.

In the course of discussing issues and events, various political actors might be mentioned. The reference might be to the president or Congress, an organized interest group, an issue, or a private citizen. One fairly straightforward way of organizing such references is as follows.

Units of government.—We are concerned here with distinctions among the judicial, legislative, and executive branches of government, between the two houses of Congress, among federal, state, and local authorities, or among any of the various federal agencies and bureaus. For some individuals, the term "government" may refer to an undifferentiated bureaucratic monolith. There may exist no notion of differentiated responsibility or of checks and balances. The bulk of the citizenry, however, do differentiate units and levels of government. Accordingly, the number of

references to the State Department, the IRS, FBI, the Supreme Court, the mayor, the local zoning commission, and the like, was used as an index of the extent of such differentiation. On the average, about four such references or distinctions were made.

Organized political groups.—The modal reference here would be to a political party or an interest group of some sort, such as the AMA, the Home Owners Association, the John Birch Society, or the NAACP. Almost all interviews involved some volunteered reference comparing the Democratic and Republican parties. Since references to the major parties were so often linked together, they were counted as one reference (i.e., one distinction). References to all other parties and interest groups were each counted as an additional reference. At times, individuals might forget the proper name of a group, get it confused with other groups, or ask the interviewer if she could recall the name. Thus, the American Independent Party might be referred to as "that other party . . . you know, Wallace and those people." As long as the referent was clear, it was included in the differentiation index. The average respondent mentioned between one and two organized political groups in the course of the interview.

General constituencies.—One of the favorite topics of political pundits and students of public opinion is the notion of issue publics or potential issue publics, a group of citizens who by reason of their racial, geographic, ideological, religious, or social characteristics are likely to be affected by and concerned about a particular issue or piece of legislation. When our respondents singled out some collectivity as actually or potentially having been influenced by or influencing a political decision, it was coded under this category. The reference might have been very broad—to poor or rich people, for example—or more specific—to people on fixed incomes, blacks, Mexican Americans. There were in this mass sample about twice as many references to broad constituencies as to actual organized nongovernmental groups.

Political figures.—Is Archie Bunker a political figure? His name was mentioned several times as typifying an approach to politics. Despite the fact that he is a fictional character, it was decided to include such references here because of their prominence in popular culture—in some weeks Archie Bunker may be responsible for getting more individuals to think about political questions than the president and leaders of Congress combined. Most references to prominent political figures, however, are much more straightforward and easily recognized. Most references were to the president or former presidents. With the exception of the governor of California, all individual political figures who were mentioned by more than 10% of the sample had occupied the presidency. A little more than four references to various political figures were made in the average interview.

A final and somewhat smaller category was devoted to *political events* such as Watergate or a recent presidential trip, and ongoing governmental programs such as Medicare or the Work Incentive Program for welfare recipients. Also included were references to broader historical trends, such as increasing bureaucratization or a weakening of the role of religion in American life. There were about five such references in the average interview.

Figure 1 summarizes these patterns of political discourse. A series of factor and canonical correlation analyses were conducted on the indices of these different elements of political discourse, and the results provided strong evidence of unidimensionality and communality. No significant sub-patterns, such as a prominent covariance between, for example, interest groups and specific issues, were in evidence. For the remainder of this discussion conceptual differentiation will refer to a simple additive index of the total number of respondent references to all categories.

The intercoder reliability of the index is not easily assessed because of the complexity of the coding task. A rough index was computed by assigning pairs of coders to single interviews with a resultant intercoder correlation for the index of $r = .84$.⁸

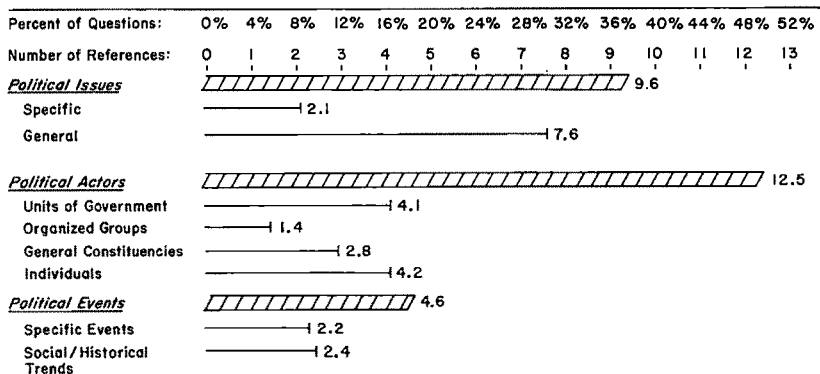


FIG. 1.—Units of political discourse—mean frequency of occurrence in the depth interviews.

⁸ Because of the difficult nature of coding ambiguous references to semipolitical issues and events, there was concern about intercoder reliability and the validity of the coding process itself. The usual indices of intercoder reliability are not useful under these circumstances because they are, for the most part, based on the proportion of intercoder agreements to the total number of coding decisions. There is a difficulty, in this case, of determining both the numerator and denominator. The average interview may include reference to 500 or more objects, individuals, and events, most of them nonpolitical in nature—Uncle Herman, the leaking faucet in the bathroom, the new Pontiac, getting a promotion at work, and so on. Basing one's calculations on the fact that coders correctly identified and accordingly did not code these nonpolitical utterances would lead to artificially high indices of agreement. On the other hand,

CONCEPTUAL INTEGRATION

The measurement of conceptual integration included a rather different type of content analysis of the interview transcripts. In addition to scrutinizing passages and enumerating each reference to an abstract concept, coders rated the interview as a whole, functioning in this case as expert judges. They were asked to characterize the predominant pattern by which respondents organized, linked, contrasted, or put in context the various political issues, actors, and events mentioned in the course of the depth interview. The typology used closely parallels Converse's five levels of conceptualization (Campbell et al. 1960, pp. 216-65; Converse 1964, pp. 214-19). Given the prominence of Converse's distinctions, it seemed appropriate to test their generalizability beyond the election context to a broader evaluation of styles of political thought. His typology proved to be a remarkably valid, robust, and viable approach to the measurement of conceptual integration. Only 2% of the interviews were judged unclassifiable, and an additional 8% were noted to involve ambiguities but were judged codable. Because the classification of an hour-long interview entailed the evaluation of a much more complex stimulus, the intercoder reliability fell somewhat below the figures reported by Converse for his initial study. Coders assigned respondents to identical or adjacent categories 82% of the time, while in the earlier case they were classified in identical categories 82% of the time. Converse's definitions of each of the five levels were revised slightly as indicated in table 1 to make them somewhat more general and appropriate to the evaluation of a full-length and broad-ranging depth interview. The liberal-conservative continuum was frequently and characteristically used as a conceptual yardstick by respondents in the highest category, but its use was not a prerequisite for inclusion in that level (as it was in Converse's original system).

The following examples illustrate the spontaneous and unambiguous use of political abstractions in the day-to-day political discourse of citizens whose active political participation for the most part was limited to voting

requiring that each political utterance be given precisely the same code may lead to an underestimate of true reliability because, for the most part, the subcategory distinctions were not used in the analysis. For example, a respondent may mention a problem concerning property taxes. One coder may designate it as a specific local issue. Another may code it as one of the frequent references to high taxes, a general issue. What is important for the bulk of the analysis is only that the total number of coded political utterances (the conceptual differentiation score) be accurate. Accordingly, the most reasonable estimate of coder agreement was taken to be the average pairwise difference in standard deviation units, or $(\sum X_1 - X_2)/N \cdot 1/\sigma_x$, where X_1 and X_2 represent the conceptual differentiation scores for each pair of coders, N represents the total number of pairwise coder comparisons, and σ_x is the standard deviation in x for the full sample. Using this formula for the 15 intercoder comparisons, it was found that, $123/15 \cdot 1/22.7 = .361$, indicating that coding discrepancies accounted for a relatively small proportion of the variance.

TABLE 1
PATTERNS OF CONCEPTUAL INTEGRATION IN DEPTH INTERVIEW TRANSCRIPTS

LEVEL	CONVERSE TERMINOLOGY AND PERCENTAGE (1956)	REVISED DEFINITION	OF TOTAL SAMPLE		WITHIN LEVEL: MEAN NUMBER OF REFERENCES				
			Making at Least One Such Reference (%)	Average Number of References	TOTAL SAMPLE CLASSIFIED IN LEVEL (%)	Abstract Concept References	Peripheral Reference to Abstract Concept	Group Interest References	Free- floating Issue References
I.....	Ideologue (2.5)	Unambiguous use of abstract concepts to structure and link political actors, issues, and events	61	1.5	13	3.9	12.5	9.7	22.9
II.....	Near ideologue (9)	Peripheral or unclear use of abstract concepts	89	5.6	15	1.6	7.4	5.7	13.2
III.....	Group interest (42)	Structuring of political issues and objects based on group interest	98	5.5	34	1.1	4.5	6.0	11.1
IV.....	Nature of times (24)	Primarily free-floating reference to political issues, occasionally structured with reference to incumbent's political performance	99	12.2	30	.9	4.0	3.8	10.1
V.....	No issue content (22.5)	Residual category	8	.7	1.0	2.3	4.9

and an occasional campaign contribution. These remarks, of course, were typically neither particularly profound nor original, but they do reflect the ability of the individual to put issues in a more abstract context.

Q. If you were trying to imagine an ideal system of government, how close do you think our present system of government comes to that ideal?

A. Well, I don't know of anything that is more satisfactory. I have some pretty *reactionary* ideas. I would go back to the idea that if anyone's going to vote on taxing property owners they should be property owners who would be paying the taxes. That idea went out a couple of hundred years ago, but it's still a pretty good idea. . . .

Q. Are there any other areas of life that we haven't talked about that you think are very important?

A. Yes. The medical. I think there should be more research done on it. . . . Finding the why-nots of the human body, is important to the future.

Q. Do you think that is the responsibility of government?

A. Yes, I think the government should have that responsibility—that is a big responsibility of the government. They should apportion more money into it.

Q. If they had to raise taxes to do these things—would you want it done?

A. Yeah. I would be in favor of it. That sounds like *socialism*, but that is the way it has to be. The type of socialism that is bad that I am talking about is the complete authoritative power of the president of the United States . . . not programs that have to be implemented for the welfare of the people.

In both examples the respondents' vocabulary is tied to the liberal-conservative continuum. In other characteristic examples of conceptual integration the emphasis was more historical, as in one case where the respondent contrasted America's role in Vietnam with that of England during the American revolution. In another case the respondent anchored many of her remarks with references to abstract principles of freedom of speech and freedom of the press, including an extended explanation of why the right of reporters to protect their sources is essential to a healthy democracy.

The second level is interstitial, reflecting peripheral, vague, occasional, or especially restricted use of abstract concepts. This second category appears to be populated by two types of individuals—those who have a sophisticated grasp of most political abstractions and concepts but are not inclined to use them often and those whose use of abstract concepts reflects limited understanding or some level of confusion. Several respondents, for example, restricted their use of the liberal-conservative dimension to spend-save issues; another equated those terms exclusively with the politics of the young versus the old. Political thought characteristic of level II is not necessarily unsophisticated, just contracted and less

explicit than that of level I. For example, individuals might refer to democracy or the principle of freedom of speech in passing without making it clear whether they had a full understanding of the historical and philosophical roots of those concepts, or they might use the terms simply as representative symbolic phrases signifying American ideals of government.

As indicated in table 1, the hour-long interviews generated substantially higher estimates of the use of abstract concepts and conceptual integration in the mass population than Converse found. This estimate for level I is about five times the size of Converse's original parameter, levels I and II combined about $2\frac{1}{2}$ times the size. Part of this difference may be due to the more active political climate of the 1970s and the more extensive opportunity in the depth interviews for individuals to demonstrate their approach to political issues and current events. Nonetheless, those making significant use of abstract political concepts represent less than a third of the citizenry. It is likely that the length and breadth of these interviews approach the point of diminishing returns and that interviews two or three times this length would be unlikely to generate percentages for levels I and II combined that would exceed a third of the sample.

If a substantial number of citizens do not make consistent use of such constructs as the left-right continuum or similar abstractions to organize their assessment of American politics, what do they use? There are, apparently, two answers—two more concretely focused anchoring points for organizing political discourse. In one case, corresponding to level III, citizens organize their response to politics on the basis of affiliation with a prominent social grouping. Passages within the depth interviews characteristic of this level include a pattern of defining liberalism and conservatism in group interest terms:

- Q: In politics we often hear the terms "liberal" and "conservative"; what do those terms mean to you?
- A: Well, it means that the Democrats are liberal and the Republicans are conservative. That's the way I look at it, and I find that it's just true.
- Q: And what is there that makes the Democrats liberal and the Republicans conservative? What are their characteristics?
- A: Well, the Democrats are for the people and the conservatives are for big business and the big financial interests in the country. And, they are governed by those big financial interests. And you see, they believe that they should control the finances and the big business in the country. And then they should hand out the jobs to the people. That's been always the way. But the Democrats don't feel that way about it. That's why we have unions.

Equally often group interests are more precisely focused in narrower and straightforward self-interest terms. A retired army sergeant, for example,

answered the questions on his satisfaction with American government, the quality of life in America, race relations, the need for political leadership, and patriotism with specific references to the interests of retired military personnel—a total of 18 references within the hour-long interview. This group-interest mode of cognitive organization characterized roughly a third of the sample.

Another alternative to a reliance on abstract concepts to organize political discourse is characteristic of level IV. Some citizens organize their response to government by a seemingly straightforward mechanism of electoral reward and punishment based on the incumbent's ability to generate peace, prosperity, and a sense of administrative competence. Among these respondents, references to issues are seldom linked to abstract concepts or to each other. They are occasionally linked to social groups but most often exist as free-floating political observations. To the extent that issues are structured, they are seen as being either successfully or unsuccessfully resolved by recent government action. Skeptical of abstract arguments of political philosophy on how the problems should be approached and who might differentially benefit, these respondents reflect the stereotype of "the man from Missouri," demanding to be shown the concrete result. One respondent, for example, came right to the point:

Q: Do you think there is anything you can do about the things you are dissatisfied with?

A: No, I don't. Just keep voting and trying to find the right candidates and just trying to do what I should do . . . live a decent moral life and do what I can in the community.

Q: In some way, can you have an effect?

A: A slight effect, yes. But it takes a while. If you vote somebody in, you are not sure what he can do. No man can promise anything, but I can certainly work to defeat him if I find somebody who promises something I prefer more. Or if he disappoints me, I can work very hard to defeat him next time. I always help in politics.

This group, according to estimates generated from our depth interviews, represents a little less than a third of the adult population.

The fifth and final level identifies those consistently apolitical respondents who may make an occasional reference to a political issue or two but show little evidence of any of the patterns of cognitive organization identified above.

Table 1 illustrates another aspect of this approach to the study of conceptual integration. The levels of conceptualization can be seen as independent dimensions rather than as mutually exclusive, hierarchical categories. Much of the attractiveness of Converse's original discussion of this typology is its parsimony and clearly ordered organization of "types" of belief systems in mass publics. Indeed, in its more generalized form,

it has proved to be a remarkably robust analytic tool. But because references to political abstractions, groups, and issues were measured independently, we are in a position to test the unidimensionality and cumulative nature of these phenomena and to explore the possibility of natural discontinuities or cutting points in the distributions. The four rightmost columns of table 1 list the number of central and peripheral references to abstract concepts, references to group interest, and free-floating issue references in each of the five levels of conceptualization.

A visual inspection seems to indicate two rather distinct cutting points setting off levels I and V from the middle mass. The small group classified in level I appears to rely heavily on abstract concepts to structure their comments (abstractions appear at a frequency three to four times that of the rest of the sample). Yet they make on the average 3.7 more references to group interest than those in the group interest category and twice the number of issue references than the rest of the sample. The small apolitical counterpart at the other end of the continuum reflects an equally unique behavioral pattern, in this case a strong lack of interest in matters political or abstract.

A visual inspection also reveals that the categories are cumulative but, as it turns out, not enough to qualify as a Guttman scale. Generally the prevalence of passing references to abstract concepts (a more difficult criterion in the Guttman sense) among the lower scale types generates too many scaling errors to satisfy the traditional Guttman criteria.⁹

Because of the prominence of abstract references at all but the lowest level of conceptual integration, we reviewed the use of abstractions and their occasional linkage to the overarching liberal-conservative continuum. The first step was an attempt to identify clusters of abstract concepts by enumerating natural terms and phrases respondents use to denote them—a complete lexicon for the 137 depth interviews. The task turned out to be not unmanageable, in fact only 287 distinct political terms, phrases, or clichés were counted. All of these references were spontaneous, volunteered by respondents rather than by interviewers, so they should reflect the salience of these organizing concepts to the public rather than to the inquiring scientists. Six groupings of prominent concepts, as summarized in table 2, were identified following Herbert McClosky's typology of political orientations (1975). The most intriguing finding was the dominance of the status quo versus change dimension. Since this part of the analysis is especially sensitive to the substantive focus of the depth

⁹ Guttman scale statistics were computed using unity as the cutting point and again using the sample mean for each variable as the cutting point. In the former case, the coefficient of reproducibility (CR) = .95, minimum marginal reproducibility = .86, coefficient of scalability = .66, and the average interitem correlation coefficient = .37. In the latter case, CR = .79, minimum marginal reproducibility = .54, coefficient of scalability = .55, and the average interitem correlation coefficient = .60.

TABLE 2
THE SPONTANEOUS USE OF ABSTRACT CONCEPTS IN MASS PUBLICS: SOME PROMINENT CONCEPT GROUPS

Dimen- sion	Average Number of References per Interview	Concept Group	Definition	Typical Terms and Phrases
1.....	1.2	Status quo vs. change	A basic dimension of the liberal-conservative continuum focusing on patterns of political change	Revolution; militancy; reactionary; extremism
2.....	.6	Political structure and process	Abstractions focusing on due process, issues of the balance of power, and governmental organization	Two-party system; pressure group; pork barrel politics; power structure
3.....	.5	Governmental responsiveness to public opinion	Abstractions focusing on democracy or its absence	Majority rule; dictatorship; town meeting; one man one vote
4.....	.4	Law and order vs. individual rights	Also prominently associated with liberalism-conservatism, constructs dealing with the tension between authority and individual freedom	Freedom of speech; inalienable rights; law and order; subversive activities
5.....	.2	Government intervention vs. economic individualism	Concepts dealing with general principles of government involvement in economic life	Free enterprise; laissez- faire; socialist economics; capitalism
6.....	.2	Equality/inequality	Concepts dealing with patterns of economic, political, and social inequality and with ameliorative strategies	Social Darwinism; affirmative action; quota system; civil rights
7.....	4.0	Miscellaneous other	Concepts, terms, and phrases either spanning the dimensions above or denoting other concepts	Justice; isolationism; pacifist; propaganda

interview and since the groupings themselves are ad hoc, these results are suggestive rather than definitive. But they do hark back to de Tocqueville's characterization of the American perspective as practical, centrist, and suspicious of utopian ideologies of radical reform (1840, pp. 1-3).

Table 2 illustrates that although the American public does not routinely use the left-right spectrum to identify a richly articulated and overarching philosophy of governance, they do find occasional use for related terms to identify (and most often to condemn) noncentrist political perspectives. Americans, despite a mode of language which reflects a cynicism about the motives and abilities of politicians, bureaucrats, and government, are in general rather pleased with the functioning of their political system as a whole. Even critical events such as Watergate seem not to have shaken this faith (Sniderman et al. 1975). For many respondents, especially in the middle mass (levels II, III, and IV), a collapsed form of the liberal-conservative continuum proves useful. Ignoring left versus right, they simply identify political actors and issues as more or less distant from the status quo. A more philosophical conception of modern liberalism which emphasizes government intervention, redistributive strategies, and abstract conceptions of equality is notably less prominent.

THE COVARIATION OF DIFFERENTIATION AND INTEGRATION

Differentiation and integration, as we have noted, are complementary processes. It is hard to imagine an attentive, politically oriented individual who in following the political news of the day has not developed some appreciation of the various abstract structuring concepts which are the stock in trade of journalists, columnists, and editorial writers. We expect that an increasingly differentiated view of the political arena will generate an increasing need for some means of conceptual organization, perhaps some variant of the liberal-conservative continuum. In turn, the image of a citizen with a fully articulated and sophisticated understanding of political abstractions who is unable to differentiate the executive from the judiciary, for example, is rather implausible. It is an issue, of course, easily tested with the data at hand.

The indices of differentiation and integration were indeed correlated, $r = .67$. The scatterplot for the two indices resembles a textbook example of bivariate homoscedasticity. Only a few scattered respondents reflected a deviant pattern of high integration and low differentiation or the reverse. In regression terminology, our respondents on the average would make two references to issues, actors, or events for each abstraction mentioned.¹⁰

The issue of heteroscedasticity and nonlinearity, incidentally, is of spe-

¹⁰ Index of differentiation = $13.8 + 1.9 \cdot \text{index of integration}$, standard error of estimate = 12.3.

cial theoretical relevance in this case. There is a certain intuitive appeal to the idea of a critical mass in both the differentiation and integration variables, some kind of threshold or take-off point of differentiating the most basic elements of the political system which must be reached before the natural spiraling process of increasing differentiation and integration comes into play. Such threshold effects suggested themselves in the continuous distributions for the five levels of integration in table 1, for example. Similarly, if another critical threshold existed in the middle or higher levels of conceptual integration, we might expect that increasing differentiation would not be associated necessarily with additional integration. Thus a basic repertoire of abstract concepts, so to speak, would suffice, and the covariation between differentiation and integration would be less distinct in upper levels, that is, heteroscedasticity. But visual inspection and a series of statistical tests revealed no evidence of either nonlinearity or heteroscedasticity. If a critical threshold exists, it is not evident in the interplay of differentiation and integration, or it takes a form more subtle than these measures can discern.

DEMOGRAPHIC PATTERNS

As students of public opinion, the impulse is deeply ingrained in most of us to seek out the demographic basis of opinion distributions and trends. Most political poll data, for example, routinely break down opinions by race, sex, educational level, income, and region of the country. Chapters on the role of each of these variables have become *de rigueur* in empirical studies of public opinion and voting. Given all that attention, our collective intuitive estimates ought to be fairly accurate. Extrapolating from the numerous studies which correlate demographic variables with political participation and interest (Campbell et al. 1960; Key 1961; Milbrath 1965; Flanigan 1972; Verba and Nie 1972; Asher 1976), we would expect lower levels of differentiation and integration among blacks, women, poor people, both young adults and senior citizens, those with lower class origins, and those in blue-collar occupational settings, who presumably spend much of their time manipulating objects rather than ideas. In matters political, especially as they concern the use of political abstractions, one would expect the individual's level of education to be of special importance. Because the manipulation of abstract concepts is central to the educational process at all levels, we might expect that education would be especially highly correlated with conceptual integration.

Table 3 reports the results of the series of multiple regressions which attempt to unravel the causal origins of differentiation and integration in mass political thought. The bottom line (both literally and figuratively in this case) reports the multiple r^2 or percentage of variance explained

TABLE 3
DEMOGRAPHIC PATTERNS OF DIFFERENTIATION AND INTEGRATION

	UNIQUE VARIANCE				CONJOINT VARIANCE			
	Differentiation		Integration		Zero-Order		Residual	
	Zero-Order r	Regression β	Zero-Order r	Regression β	Zero-Order r	Regression β	Zero-Order r	Regression β
Social status:								
Education.....	.26	.24	.31	.27	.28	.17	.27	.27
Occupation status.....	.10*13*10*
Income.....	.19	.10	.29	.20	.22	.1815
Ascribed status:								
Race (black).....	-.23	-.22	-.21	-.17	-.24	-.05	-.21	-.21
Sex (female).....	.00*	...	-.07*	...	-.04*
Age (young adult).....	-.03*06*	...	-.06*
Age (senior citizen).....	-.07*00*00*
Father's SES.....	.18	.06	.22	.09	.21	.05	.11	.11
Multiple r34433139
Multiple r^211180915

* Nonsignificant.

by all demographic variables combined, revealing, as regressions of this sort go, rather low coefficients. These cognitive phenomena, it appears, are not easily predicted from a battery of demographic variables. The primary factors appear to be education, income, and race. In order to explore the possibility of a unique linkage between several demographic variables and one of these two indices, the unique variance of each index was extracted by running an integration-differentiation regression, taking residuals and rerunning the demographic regressions on residualized dependent variables. The results are included in table 3. As expected, education is most strongly linked with conceptual integration. In fact, it may not be related at all to the unique variation in the differentiation index.

Overall, we can conclude from these analyses that the patterns of political thought under scrutiny here are relatively weakly linked to the usual demographic variables but that, among these variables, level of education seems to be most significant. As we shall see shortly, however, the influence of education on styles of political thought is more complex than we had first anticipated.

EVIDENCE OF A SPIRALING PROCESS—PATTERNS OF POLITICAL THINKING IN THE LIFE CYCLE

An interactive spiraling process of this type is a very difficult causal pattern to untangle by means of a single survey study. We would benefit, of course, from repeated measurements over time. But even then, because we have no theoretical basis on which to estimate lagged effects, we might move only a small distance toward a clarification of the causal process involved. The best available alternative strategy seems to be an analysis of growth in differentiation and integration in the various age cohorts. We expect, given the spiral hypothesis, a steady increase in both indices with age. But the initial analysis of zero-order correlations between age and the indices of differentiation and integration indicated no significant relationship. Because of the possibility of a suppressor effect resulting from the usual negative correlation between age and education and because of the possibility of retrogression associated with senility and the status of senior citizens, nonlinear and interactive effects were explored through a series of analyses of covariance. Still, the results were nonsignificant and unpromising. At this point the spiral theory which had stimulated the analysis in the first place was reexamined for a further clue on how the mechanism might work. Was it possible that education served as a catalyst and that the effects of age would be different for the lesser and more educated strata of our sample? Figure 2 illustrates the rather striking results of the ensuing analysis.

The upper portion of figure 2 graphs the relationship of age with

conceptual differentiation for three groups, corresponding to those whose highest level of education is grade school, high school, or college and above. The solid line designating the pattern for grade school respondents actually declines with age, starting a little above the overall sample mean for the differentiation index and dropping well below it.¹¹ This seems to suggest that the spiraling pattern can work in two directions. Those who enter the work world early but with little formal education seem as capable

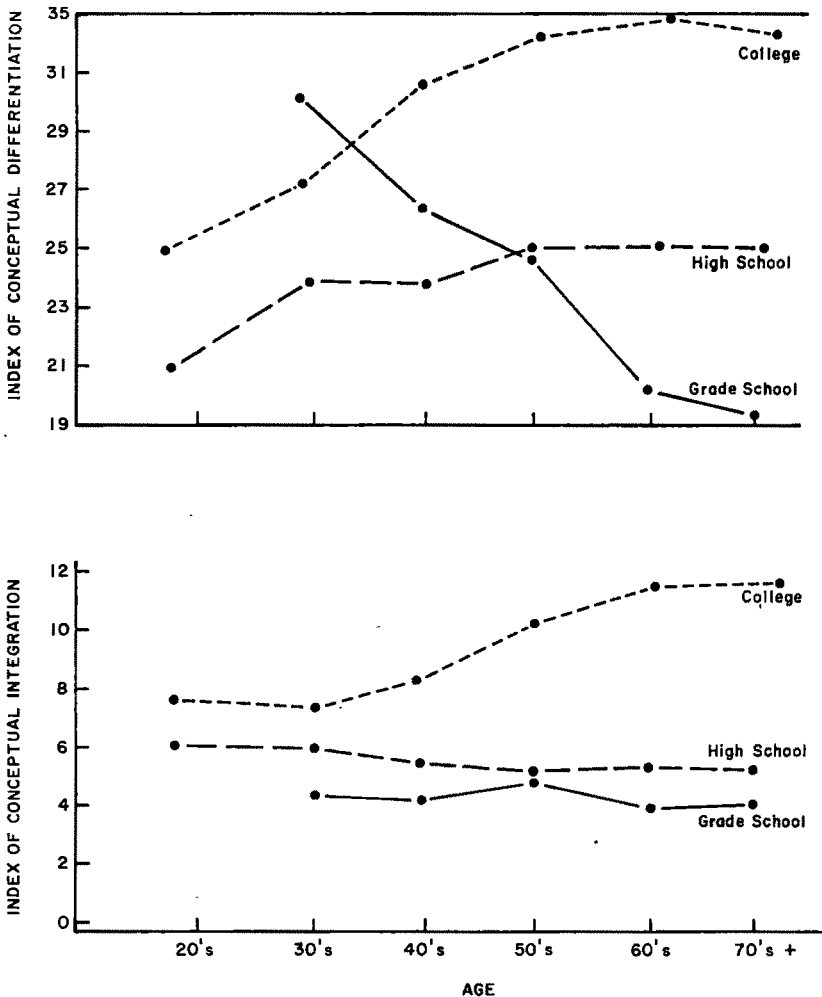


FIG. 2.—Interaction effects: education and age on differentiation and integration

¹¹ In order to minimize sampling fluctuations inherent in this small sample, the data in this figure and in figure 4 have been smoothed by the traditional moving average technique, which averages the means in each reported point with the adjacent means.

as any to be attentive to and differentiate the various objects of political life. But without the help of integrative abstract concepts, the political realm becomes threatening and confusing to these individuals. As a result they retreat into a less differentiated and more simplistic conception of politics. In contrast, just the reverse spiraling process is evident for the college educated, with the high school educated falling in between with no linear trend. A similar pattern is evident in the bottom portion of figure 2. In this case conceptual integration does not decrease over time for the grade school subsample, but we see a dramatic and fairly steady growth among the college educated. Also, we can see from the fact that these trend lines do not intersect that the relationship between education and integration is a much stronger one.

We cannot be sure that the interaction effects dramatically illustrated here are actually the result of accumulated exposure to political life through increasing age, because given the nature of these data we are unable to separate out age and historical cohort effects. It is often suggested, of course, that young men and women coming to political consciousness during the Depression, the Second World War, or the Vietnam era would be likely to have fundamentally unique perceptions of the political process. Of course, long-term panel data on these variables would be necessary to resolve this issue.

COGNITIVE STRUCTURE AND POLITICAL BEHAVIOR

The discussion thus far has not emphasized a necessary link between styles of political thinking and political behavior. There are several reasons for this. First of all, despite the vigor of ongoing policy debates, on a day-to-day basis there are not many opportunities for political behavior per se among the mass citizenry. Of course, newspapers and television continually offer up political news which many of us absorb more or less passively. But, except for the political campaign season every several years, an occasional brouhaha at the local school board, or a political petition, not much opportunity for the average individual to act on the basis of his or her political perceptions presents itself. During campaigning, political activities are highly routinized and usually professionally organized. There are ample opportunities for symbolic activities such as the wearing of buttons and displaying of bumper stickers, but the bulk of the public plays the role of observer rather than participant. Second, as we have noted, the different styles of structuring political thought identified by Converse's terms "group interest" and "nature of the times" serve as functional alternatives to abstract thinking. Less politically sophisticated individuals whose party identification and political preferences are organized on a proxy mechanism, following the lead of unions

or other organizations or more politically active friends, may have a functionally consistent set of opinions and may vote as often in their own best interest as more sophisticated citizens who laboriously study the issues and candidates in making up their minds. We will return to this difficult issue of the ramifications of cognitive styles in public opinion for the functioning of the political system in a concluding section. But first, in an attempt to probe a little deeper into the character of the causal status of the differentiation and integration variables, let us take a further look at the depth interview data.

A caveat is in order here. The issue of causal direction in the linking of political belief patterns and behavior is complex. Again, we cannot assume that, for example, an increasingly differentiated conception of the political realm causes strong party identification. Perhaps there is an interactive or spiraling process involved here, too. Much of the ensuing discussion will simply identify patterns of covariance without an attempt to certify direction of causality.

As a basic strategy of analysis we will explore the individual effects of differentiation and integration, although in most cases we might expect them to have a similar impact. The working assumption is simply that increasing differentiation (reflecting a movement from an apolitical to a political orientation) will be tied most closely to political participation, while increasing conceptual integration will be more closely tied to patterns of ideology and opinion.

We turn first to issues of political ideology and party identification. Two indices of ideological orientation were available. One was a simple question which asked respondents to identify themselves on a five-point scale from very liberal to very conservative. The other was a 35-item scale of attitudes toward specific issues in the liberal-conservative domain (McClosky 1975). A nonlinear pattern was expected with the most liberal and most conservative exhibiting the highest levels of both differentiation and integration. One might expect a more distinct pattern for integration because of the salience of abstract principles to ideological thinking. But nothing in the research literature on mass ideology would lead one to expect more differentiated thinking among individuals on the left than on the right, or any greater dependence on abstractions per se. Thus a symmetric U-shaped curve was anticipated in both cases.

But as figure 3 illustrates, there are some distinct differences that were not anticipated. It seems that, as defined by our measures of differentiation and integration, liberals are distinctly more sophisticated than conservatives in mass publics. The ideological self-identification measure is not particularly helpful because so few people identify themselves as very liberal or very conservative. In this graph we see evidence of nonlinearity for the ideological elite who identify themselves as leaning far to the left

or right on the political spectrum. But among the middle mass, the great majority of respondents who simply identify themselves as liberal, moderate, or conservative, there is a distinct linear trend of decreasing differentiation and integration moving from liberal to conservative. The ideology attitude index, based on an actual compilation of opinions and beliefs (and a better-balanced distribution of respondents), demonstrates the linear trend more dramatically.

What would explain such a pattern? Liberals mention on the average seven more political objects than conservatives and are about one and a

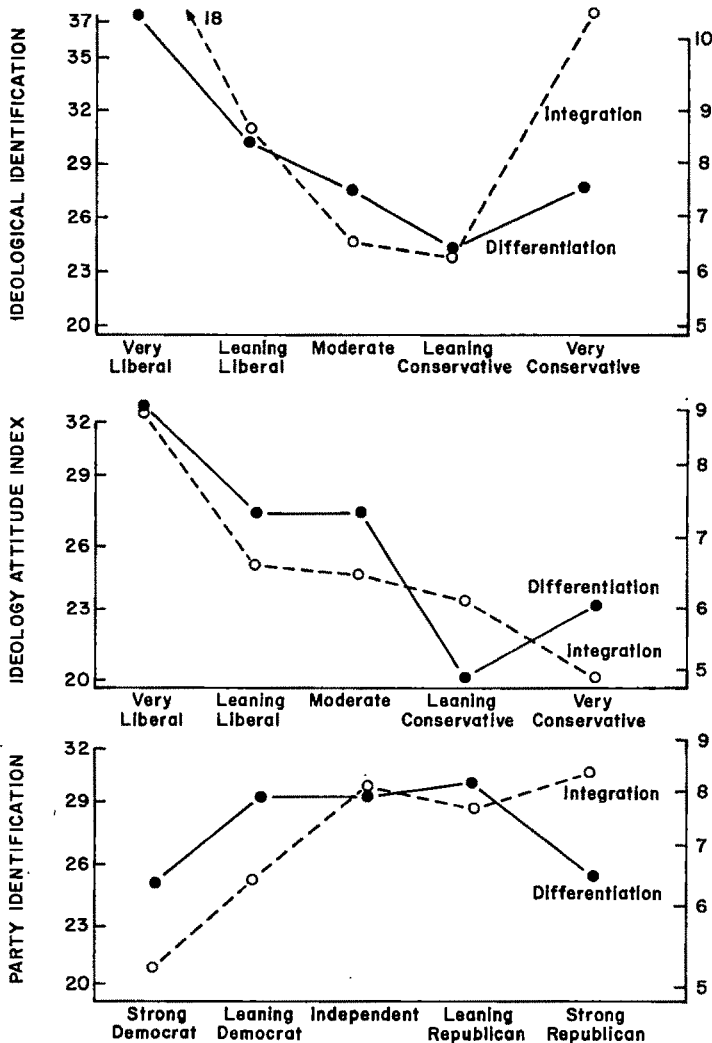


FIG. 3.—Ideology and party identification

half times as likely to refer to a political abstraction. Is it a spurious artifact of some sort? Probably not. Analysis of the distribution of education across these ideological categories indicates no significant confounding effects. We would not extrapolate from these findings to argue that the political leaders, scholars, and columnists of the right are any less sophisticated or disinclined to use abstract concepts. But it seems that in the way ideological packages filter down to the mass publics, the anchoring concepts of conservatism are perhaps fewer in number, less abstract and less conducive to a differentiated perception of governmental process. This makes sense on an intuitive level. One thinks of the rugged individualist of modern conservative thought who exhibits little sympathy for government intervention in our day-to-day lives, the growing federal bureaucracy, and the proliferation of abstract catch phrases based on "wars on poverty," "affirmative action," and the subtleties of *détente*. We might think again of the stereotypical man from Missouri who demands to be shown the concrete results and has little taste for the abstractions of modern politics. His is a conservative posture, perhaps populist in flavor, which reflects a cynicism toward the undifferentiated symbol of Washington/federal bureaucracy/high taxes/etc. Perhaps such reflections overinterpret the data. There may well be a simpler explanation. But at the very least, these striking differences between liberals and conservatives in the mass population deserve further scrutiny.

Given the more highly differentiated conception of politics among liberals, we might expect Democrats to exhibit higher scores than Republicans on differentiation, but that turns out not to be the case. The pattern in the final graph in figure 3 is less distinct but indicates that Democrats have no higher level of differentiation and perhaps a lower level of integration than Republicans. So it seems that whatever the mechanism which differentiates the cognitive styles of liberals and conservatives, it does not translate into an equivalent pattern for party identification.

We turn next to patterns of electoral participation. The hypothesis here is fairly straightforward. One would expect, naturally enough, that increased skills in differentiation and integration are directly associated with political participation. The dependent measure is the index of electoral participation based on the frequency of involvement in campaign activities, including displaying buttons or bumper stickers, attendance at political rallies, volunteer work, political contributions, and persuading friends or neighbors to vote for a particular candidate, in addition to voting. The average for the overall sample was about three such activities over the past several years, and, as figure 4 illustrates, there is a fairly dramatic linear correlation between both cognitive indices and electoral participation.

The hypothesis of a threshold effect of some sort had been suggested. Perhaps some critical mass of information or a critical level of integrative

more general questions about the role of public opinion in the democratic process.

First, one important conclusion likely to be drawn from such labors with transcribed depth interviews is an overwhelming sense of the procrustean nature of survey research. It is a point which is perhaps not easily conveyed by the few brief quotations and the summary statistics cited above. But it is an impression, no doubt, which has struck other analysts who have had the opportunity to work with the remarks of their respondents in unedited natural language. By following the dynamics of the interview process in a transcript one sees repeatedly how the initial response of the interviewee to a particular question, such as one about attitudes toward racial inequality or the energy crisis, might easily be misinterpreted. The responses to follow-up probes and the ways in which the respondents organize their answers reveal much more than any simple agree-disagree continuum could possibly capture.

Depth interview work of this sort is expensive and time consuming, but one of its strongest contributions might be to clarify the interpretation of more routinely gathered poll data. One of the most surprising findings of this study was the correlation between liberal political attitudes and high levels of conceptual differentiation and integration. Further work is now underway to determine whether liberals and conservatives use abstract concepts in distinctly different ways and whether such a distinction, if it exists, might help explain patterns of political behavior.

A second conclusion concerns the relationship of the individual to his or her political environment. Recent books by Bennett (1975) and Page (1978) have emphasized this point. If after a careful examination of hour-long interviews we still find two-thirds of the mass electorate making only marginal use of political abstractions to structure their evaluations of the political system, it is not necessarily the result of their own cognitive shortcomings. If issues are vaguely defined and the linkage between candidates and issues is unclear, it may well be a result of a pattern of candidate behavior and media coverage, which itself is vague and shallow. The columnist has so many column inches, the television reporter so many seconds to capsule the major issues of the day. Given the current structure of the news media, one could hardly expect them to get much beyond the main points of an issue. Correspondingly, few candidates have found success by handing out long manuscripts spelling out their policy positions on each issue of the day. Further research on cognitive patterns will benefit from parallel analyses of mass political thought and trends in media coverage.

Clinical psychologists working in the area of cognitive structure have argued that a sudden increase in the perceived complexity of a problem can lead to a pattern of withdrawal (Schroder, Driver, and Streufert 1967).

So a potentially sudden shift in media coverage or candidate behavior could have a disruptive effect on the democratic process, especially if the citizenry as a whole came to the sudden recognition of how little the politicians and experts understand about our collective problems. There are some signs that we may be approaching a crisis of this sort now. But a gradual expansion of coverage of political events, perhaps a spiraling increase between differentiation and integration, would seem to be a step in the right direction.

Third and finally, there is an issue which underlies this entire analysis. To what extent are the less sophisticated and attentive citizens in the electorate more easily propagandized and manipulated? This concern has arisen repeatedly in social science research, including the propaganda research of the 1940s, the concern with mass society in the 1950s, as well as more recent research on media and politics. The results reported above, however, give no indication that those with lower scores on conceptual differentiation or integration are any more easily manipulated by political symbols or arguments. The intermediate levels of cognitive organization reflecting an orientation toward group interest or a straightforward mechanism of electoral reward and punishment represent, after all, rather reasonable political postures. In fact, the man-from-Missouri stereotype reflects a cognitive state more likely to be influenced by concrete results than by rhetoric. This suggests some intriguing possibilities. It may be that the more sophisticated and abstractly oriented citizens are actually more rather than less susceptible to the manipulative strategies of political elites.

APPENDIX

A Brief Outline of the Bay Area Survey Depth Interview Schedule

Global evaluations of American society.—Six questions here concerned what the respondent was satisfied and dissatisfied with in American society and government and who should take the credit or blame.

Quality of life.—Nine questions here concerned satisfaction with living and work conditions, the economic situation, crime, race relations, and the environment. Respondents were asked to contrast their personal situation with that of their community and the country as a whole.

Political values and perceptions of government.—In this section 10 questions concerned evaluations of the American system of government, political freedom, equality of opportunity, the responsiveness of government to the citizenry, participation in the political system, and the trustworthiness of public officials.

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Max Weber and Robert Michels¹

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This paper investigates the unique intellectual partnership of Max Weber and Robert Michels. Drawing on published work and unpublished correspondence, it shows the extent and nature of the influence exerted by Weber on Michels's inquiry into the sociology of parties and organization. Beginning as a syndicalist and renegade Marxist, Michels sharpened his critical perspective under Weber's guidance. The "structural" and sociological analysis in his major work, *Political Parties*, developed within the categories and norms of Weberian social science. However, substantive disagreement arose over the central "problematic" of modern social theory: for Michels it was "democracy," for Weber "domination." This disagreement accounts for their contrasting interpretations of the organizational phenomenon. The paper concludes with an evaluation of the import of Weber's critique.

On New Year's Day 1906, Max Weber dashed off a note to Robert Michels, offering to meet him soon in Heidelberg. The invitation came shortly after publication of Michels's first article (1906a) in the *Archiv für Sozialwissenschaft und Sozialpolitik*, the influential journal coedited by Weber. Praising this analysis of socialism and citing his "uncommon interest" in Michels's "projected work," a reference to the research that led to *Zur Soziologie des Parteiwesens in der modernen Demokratie* (1911; translated as *Political Parties* [1915] 1962), Weber worried only that their introductory meeting would occur on one of his "black days" when all efforts at sociability were "completely useless" (AMW 1).² From such hesitant beginnings a regular, continuous exchange of views quickly developed, which lasted until 1915, when political disagreements about the war and a messy dispute over Michels's publicist activities in Switzerland put an end to their friendship. But for 10 years the conversation between

¹ I am indebted to Robert Eden, Wolfgang J. Mommsen, and Edward B. Portis for helpful comments on an earlier draft of this paper. I also wish to thank the Fondazione Luigi Einaudi, Turin, and the Zentrales Staatsarchiv der DDR, Merseburg, for providing access to their archives.

² The following abbreviations for references to archive sources are used in the text: AMW—Archivo Max Weber, Fondazione Luigi Einaudi: Nos. 1–134, containing Weber's letters to Michels; NS—Nachlass Gustav von Schmoller, Zentrales Staatsarchiv Merseburg, Rep. 92; and NW—Nachlass Max Weber, Zentrales Staatsarchiv Merseburg, Rep. 92.

these two social scientists ranged over many of the political and social preoccupations of the age: socialism, syndicalism, political parties, bureaucracy, political developments in Russia and the Western democracies, academic freedom in Germany and Italy, modern political and sexual morality. During this period Weber read all of Michels's important work, including some unpublished manuscripts, and commented on it, sometimes at length and in great detail and always with an unusual blend of criticism and praise, candor and encouragement.

The personal friendship between the two men was quite well known to contemporaries and has been common public knowledge since the publication of Marianne Weber's biography of her husband ([1926] 1975) and a few of Michels's autobiographical statements ([1925] 1957, pp. xxvii-xxix; 1927*c*; 1932). However, the precise nature of their scientific collaboration has remained a subject of conjecture and some controversy.³ This paper investigates the intellectual relationship between Weber and Michels, using the latest evidence to support a more complete and critical comparison than those offered heretofore. Their manifold relationship is of interest for elucidating one of the most important episodes in the articulation of Weber's social theory, for explaining the formation of Michels's famous thesis about "oligarchy" in organizations, and, more generally, for documenting the development of social science out of "idealism" and moral philosophy. The evidence shows that Weber exercised a controlling influence both on the form of Michels's inquiry into political parties and on the substantive claims made for the causal significance of "organization"—precisely those aspects of the inquiry that have survived and been considered most valuable. In contrast, Weber's own substantive contributions, particularly the sociology of "legitimate domination," developed in tension with Michels's "ethical" perspective, in repudiation of the "problem of democracy" that Michels formulated and doggedly pursued in his later work.

SCIENTIFIC AND POLITICAL INTERESTS

Michels's initial contact with Weber occurred at a crucial juncture for him, both academically and politically. Having established his reputation

³ For discussions of Weber and Michels, see Mommsen 1974*b*, esp. pp. 111-21, 144-45, 420-21; Roth 1963, pp. 249-57; and a revised version in Bendix and Roth 1971, pp. 246-52. The best study of Michels is Röhrich 1972; the author correctly surmises that "with Michels' investigations Weber stands in the background as the questioner, as the *Realpolitiker* whose questions determine the direction of many an answer" (p. 52). See also Conze 1957; Lipset (1962), who mistakenly reports that Weber's "discussion on the structure of legal political parties is a summary of *Political Parties*" (p. 21); May 1965; Linz 1968; Mitzman (1973), pt. 4, where surprisingly Weber's importance is not mentioned. Older studies include Burnham 1943, pt. 5; Meisel 1958, pp. 183-89; and Hughes 1958, chap. 8.

as an accomplished writer, lecturer, and activist in the socialist and syndicalist movements, Michels spent most of 1906 attempting to secure a permanent university position in Germany and to this end enlisted Weber's advice and assistance. Despite Weber's intervention, the attempt failed on account of Michels's Socialist party membership and two other amusing alleged "infractions" that became known only later (see Weber [1908a] 1974). By the end of the year he had decided to relocate at the University of Turin, where he had studied previously. On the political front, the first of Michels's significant studies of the German Social Democrats was published in the *Archiv's* September issue (1906b). Its publication, assisted by Weber, was really a political act, intended to coincide with the annual Party congress scheduled for Mannheim (AMW 8, May 21, 1906). The critique caused a stir, as Michels (and perhaps Weber) evidently hoped, and in the Mannheim debate Karl Legien and August Bebel declared Michels persona non grata for his syndicalist leanings. Citing Michels by name, Legien bluntly delivered the orthodox point of view: "The general strike is general nonsense" (*Protokoll* 1906, p. 246). Weber attended this meeting (although Michels did not), saw Legien and Bebel in action, and reported to Michels his observations about the Party's "weakness" and lack of revolutionary élan (AMW 18, Oct. 8, 1906), observations that are in fact borne out by the cautious, defensive quality of the recorded speeches.⁴ In the third- and fourth-day debates on the "political mass strike," only Karl Liebknecht rose to answer Legien and Bebel, chiding them for sounding so "unradical," a response, ironically, that echoed Weber's private and public comments at the time. It was also an indication of the peculiar way in which Weber's judgment could, now and then, coincide with that of activists on the Left.

According to Michels's later account (1932), he had entered the Party a reform-minded socialist affiliated with the Marburg faction; an uncompromising critic of revisionism; and a defender of local autonomy within the Party, a position in which he was temporarily supported by Bebel. The quarrel with the Party's Berlin leadership evolved out of Michels's advocacy of socialism as an "ethical" program of ideas and action—or, as he once said, a theory aiming toward the "withering away of the state"—and not merely a potentially fruitful method of analysis (1908b, pp. 131–32). His frustrated attempts to win the Party to this position led

⁴ Weber's brief notation to Michels was repeated in sharp language at the *Verein für Sozialpolitik* meeting of October 2, 1907 (see Weber 1908b), setting off a further exchange on this topic, recorded in letters of October 15, November 6, and November 7, 1907 (AMW 49–51). These letters are four of 30 to Michels partially transcribed by Marianne Weber (NW 30/4, 178; 30/7, 46, 52–53) and are used by Mommsen 1974b. What is interesting about the 1907 exchange on the nature of the SPD is Michels's initial failure to understand Weber's criticisms of the Party's "embourgeoisement."

to an association with Hubert Lagardelle, Georges Sorel, and the Parisian syndicalists during the period in which Sorel was composing the *Reflections on Violence*. Intrigued by the aggressively anti-authoritarian, anti-institutional stance of the theory of "direct action," which appeared truly "revolutionary" by upholding the highest *moral* principles of "Marxian" socialism, Michels began to doubt the political purpose and tactical judgment of his German colleagues. What he perceived as their overbearing arrogance in the face of criticism only confirmed his suspicion of veiled authoritarianism. Michels vented his anger in emotional assaults on the Party, some published in French, apparently without being restrained by Sorel's pointed warning: "The boycott practiced for ten years by the official socialists against everything I write will follow you." Unlike some of Sorel's other prophecies, this one proved correct. By 1907 Michels had surrendered his Socialist party membership in both Germany and Italy. But he had also started to drift away from the syndicalists, who had themselves by then shown similar signs of demonstrating that revolutionary action is less easily sustained than revolutionary phraseology.⁵

For Weber, the decade of his close association with Michels was marked by a struggle to regain his health and to realize the promise of his earlier scholarship and political acumen. If anything remained constant in these years, it was his pursuit of critical, scientific knowledge, of "intellectual integrity." Precisely this image of Weber as the Promethean "man of knowledge" attracted Michels and informed his description of himself as Weber's "kindred soul," similarly committed to "vivisection if it is in the interest of science."⁶ Or as Michels wrote about himself in a remarkable retrospective confession, "More suited to the scientific analysis of politics than to its practical application, he arrived through a slow process, actually unconscious of it himself, at a vivisection of the Party, a painful dissection of something living" (1932, pp. 362-63). Thus, for Michels these were years of critical self-reappraisal, withdrawal from practical politics, and a shift in theoretical perspective. One can see the guiding hand of Weber in this process, providing *conscious* direction to the investigation and alternative solutions to the intellectual puzzlement.

⁵ Weber's knowledge of syndicalism and Sorel's views, evident in many passages in his writings (e.g., Weber [1917] 1949), can be traced directly to Michels. Furthermore, young Michels, the uncompromising syndicalist, is surely one of Weber's models for the famous passages on the "ethic of ultimate ends" ([1919a] 1946a, pp. 118-28).

⁶ The original dedication of *Political Parties* reads, "To my dear friend Max Weber, that upright man who does not shy away from vivisection if it is in the interest of science, with greetings from a kindred soul" (Roth 1963, p. 250, n. 3). This dedication is dropped from the English edition and the second German edition. Michels's acknowledgments become more guarded later, after his political views have changed once again; he then mentions only the "close relationship" with Weber that was "not without influence" ([1925] 1957, p. xxviii; 1927c, p. 112).

This common attraction to "science" was only one source of their cooperation, however. For if in Michels's view Weber symbolized the struggle for freedom from intellectual compromise and ideological illusion, in Weber's eyes Michels represented the prototypical *Gesinnungsethiker* determined to show that "the world is stupid and base, not I" ([1919a] 1946a, p. 127). As Weber wrote to Michels's wife, Gisela, "He is a 'moralist' from head to toe. . . . His 'ethos' is clearly and consciously the most precious possession he has and from it flow the essence and reason of his life. Moreover, in his political evaluations he is so exclusively ruled by this highly personal ethos that he once completely failed to understand the distinction that I mentioned ad hoc between vocation (affairs [*Sache*]) and 'life'" (AMW 67, Dec. 25, 1909).⁷ Such an ethos gave rise to both astonishing naiveté and extraordinary insight, to which Weber was simultaneously attracted and repelled, as in his later assessment of believers in an "ethic of ultimate ends" or "intentions." "In fact even though I often differ from him on particulars and also on matters important in themselves," acknowledged Weber, "I too repeatedly find so many affinities in our fundamental *mode* of seeing and comprehending things. And that happens to me so rarely . . ." (AMW 67). In politics Weber, the *Realpolitiker*, charted a course far different from Michels's, but the coexistent path of the alter ego always loomed as a *possibility*, for Weber saw in himself a similar repudiation of the "stupidity and baseness" of the world in favor of the stubborn radicalism of the "outsider."

MICHEL'S SEARCH FOR A THEORY

Michels's social theory began as a version of "syndicalized" Marxism, temporarily appropriated to make sense out of the allegedly anti-democratic tendencies in Social Democratic theory. This theory was useful, as Michels later explained, in the "struggle of ideology against organization for its own sake" (1932, p. 357). "Ideology" in the early pre-Weberian phase of his work was meant to signify the *ideals* of "equality," "rights," and "freedom" as expounded by Rousseau and modernized in the *Communist Manifesto*. These were primary, self-evident concepts for Michels and, thus, required no clarification. Genuine "democracy" in turn was simply the "direct" rule of equal, active participants. Any other condition was ipso facto "undemocratic."

⁷ It was such an "ethical" stance that informed Michels's views in the fierce debate over socialism and ethics that broke out in the aftermath of Bernstein's *Evolutionary Socialism* (1899). "Why the quarrel?" protested Michels; "*Socialism is ethics*. Outside of socialism there can be no ethics, and outside of ethics no socialism" (quoted in Vorländer [1906], p. 758). Needless to say, Weber's analysis of different value spheres was meant to counter such idealized ethical rationalism.

Michels's self-described "idealism of the purest kind" (1932, p. 362) is quite evident in this line of thought. However, what strikes one most forcefully about his early writings is not so much the unrepentant idealism as the unstable combination of pure motives, precise practical detail, and theoretical confusion. "Motive" is surely the mediating element, the one that leads Michels on a search for a theory—what comes to be designated an inductive theory—that can create coherence out of the competing worlds of practice and ideology. Even the "problem" the theory is supposed to address has a kaleidoscopic quality about it, shifting in the early writings from "capitalism" to "nationalism" to "leadership." Various corresponding solutions are attempted—ranging across Marxism, socialism, and syndicalism—and all are found wanting. The next, post-1906 attempt, involving what could be called a paradigm shift, stated in final form in *Political Parties*, is worked out within the framework of Weberian social science.

Weber's remarks at the moment of political and intellectual crisis became important, for they set forth a specific, consistent research program that captured Michels's attention. In public comments Weber urged a careful dissection of the apparatus of the Social Democratic party (SPD) and advanced the thesis that the Party's "inexorable development" led toward preservation of that apparatus "for its own sake" (1905; 1906b, pp. 389–90; cf. Bendix and Roth 1971, pp. 248–49). This particular thesis and an accompanying catalog of researchable topics were amplified in subsequent correspondence with Michels: a study should begin with "legal" or "formal" structure and its "practical meaning," avoid all ideological exegesis, and concentrate throughout on the "purely technical-structural side" of politics. Showing greater caution than in his intentionally exaggerated public statements, Weber admitted that the SPD presented certain "peculiarities," for it "champions a 'Weltanschauung' [and] is not *only* a technical 'machine,' like the American party." "But," he added, "it is nevertheless *also* a 'machine' and must be one. Now how the objective 'ideals,' the indispensable 'machinery,' the thereby given 'hierarchy' and (the full truth!) 'bureaucracy' in the Party reciprocally influence each other—in fact that also appears to me (indeed you [Michels] intimate something similar too) a highly interesting problem" (AMW 4, March 26, 1906).

One important consequence of this particular combination of a *Weltanschauung* with bureaucracy, Weber suspected, was a supreme rigidity that limited the Party's ability to maneuver and to expand its base of support. He encouraged Michels to probe further "the motives and results of the 'inflexibility' of the Party toward bourgeois admirers," a natural recruiting ground should the Party follow the path of the American "machines." As was often the case, Weber could not resist a personal comment on this issue, an apology for his own politics: "It is the decisive situation for

certainly very many members of the 'bourgeoisie' that they are not in a position to be effective in workers' circles or in the area of completely *nonpolitical* work having to do with general 'enlightenment' because the Party anxiously controls that and does not like to see it, and on the other side because they are not able to subscribe to the [Party's] creed" (AMW 9, June 3, 1906).

Weber might have chosen to write on these topics, rather than on the Russian Revolution (in relation to which he did in fact discuss party politics and bureaucracy), had he been convinced that access to the necessary information could be obtained by a confessed "class-conscious bourgeois" (AMW 50, Nov. 6, 1907). However, as he wrote Michels, undoubtedly with Eduard Bernstein in mind as well, "Even now a cool, disinterested 'anatomy' of the Party is missing, one that does not immediately ask 'cui bono?', and it can be produced neither by an outsider nor by someone directly involved as a 'Party' participant in the *internal* Party struggles" (AMW 4). Robert Michels, cosmopolitan critic, former citizen of Cologne, and ex-Socialist candidate for the Reichstag, would be ideally suited to the task. Most important, Michels agreed to the charge.

More than a list of topics and questions was required if Weber's articulation of an explicit alternative to "syndicalized" Marxism was to become compelling. In this regard, his contributions to Michels's work were threefold: (1) Weber insisted on applying the criteria of confirmation from his version of social science; (2) he developed a specific interpretation of the Social Democratic party, one that Michels could react against, accepting, rejecting, or modifying it; and (3) he offered his own critique of what Michels came to call the "problem of democracy." One might say that the first contribution has to do primarily with the formal features of the inquiry, the second and third with its substance. The theoretical differences separating Weber from Michels become more accentuated as one moves from the first to the last category.

WEBER'S ADVOCACY OF SOCIAL SCIENCE

Weber's larger conception of social science is well known. It was advanced on numerous occasions, provoked considerable controversy then and now, and was certainly familiar to Michels. But this conception need not detain us here. What is of interest is the way in which Weber posed the *choice* for Michels between social science, on the one hand, and alternative forms of knowledge and action, on the other.

In the correspondence this occurred in several different places, each passage stating the problem in characteristically sharp form. "Your essay⁸

⁸ Here and in the quotations in the next paragraph, Weber is referring to an unpublished essay, probably "Struktur der sozialdemokratischen Partei Deutschlands," cited

could be *either* of two things," Weber wrote in one case: "(1) a *confession* and 'appeal' . . . or (2) it should represent a *scientific* analysis and *not* only an ad hoc (ad usum Delphini, so to speak), given 'illumination' of a situation for practical-political purposes." Having proposed such a categorization of knowledge, Weber then insisted that "a *scientific* work is not a work that 'separates light from shadow,' as you write, and there is *no* 'justice' there, only *facts* and *causes*. Granted, with each of us 'representation' and 'valuation' are always in danger of flowing together, but one must *want one* of them. You want the one, and *don't* achieve it . . . whereas you *achieve* the *other*, but in a politically ineffective way" (AMW 60, Aug. 11, 1908). As summarized in other passages, the choice was between wanting to issue "subjective appeals" and aiming for "impartial history," or between "ethics" and "science" (AMW 62, Aug. 21, 1908).

Far from ruling out value-laden appeals as pointless or irrational, Weber sought to convince Michels that categories of thought must not be confused. On the one hand, an appeal was to be judged principally not by its ability to identify causes and explain facts—nor even by the intensity of its convictions—but by its political effectiveness. Was it compelling or persuasive for the intended audience? Speaking of the *Archiv's* readership, Weber asserted, "I am *absolutely sure* about the article's effect: 0!" As an effective appeal it would have to be more "coolheaded," "controlled," and "historically to the point" (AMW 59, Aug. 4, 1908). Without such emendations, Weber predicted, "*the* reader to whom your appeal is directed will *in part* say, 'I've known that (at least *almost* all of it) a long time,' *in part* he will feel, 'He doesn't understand *me* and serves *so* absolutely 'different gods' than I do that the matter doesn't move *me*'" (AMW 60). Such a conclusion would signify frustrated political purpose.

On the other hand, from Weber's standpoint a thoroughgoing scientific analysis required not only that rival gods be discarded but also that the subject matter under investigation be approached in a certain way. Michels's long-standing tendency was to treat political issues, actions, institutions, and concepts solely through "moral" categories, a practice that Weber thought clouded his understanding of actual political relationships. According to Weber, it is precisely in opposition to this tendency that one must see the impact of "scientific" norms on political (or ideological) judgments, particularly in a social science with "political" content. Hence, whether one approved or denounced the Social Democratic party's justifying cultural values and ethical claims about its "science" was quite beside the point. For a scientific investigation the pertinent questions were, To what extent and by whom are such claims made? To what degree is

by Michels (1908a, p. 1621, n. 6) as forthcoming in the *Archiv*. It was never published, however, and one can assume that Weber's criticisms led to its withdrawal.

the Party, despite its pretensions, developing into a "machine"? What are the probable cultural and political consequences of such a development?

The contention, popular in some circles at the time, that socialism could be refuted scientifically was treated by Weber with scorn, just as was the countervailing idea that the social science practiced in the universities was mere bourgeois obfuscation (AMW 35, Feb. 1, 1907). This was entirely consistent with his experimental, pragmatic view of scientific work. Weber never wavered from stressing to Michels that the consequences of accepting the independent norms of scientific criticism were, first, radical criticism of "mythical" elements in the language of science, the language of definitions, concepts, and types; second, identification and control of value judgments; and third, repudiation of all "utopian" formulae in *any* political ideology—socialist, syndicalist, democratic, or nationalist.

Reflecting the position of his methodological essays, Weber's advice is strewn with references to the uncritical use of "collective concepts" and "value judgments" that permits one "to violate the plain facts" (AMW 61, Aug. 17, 1908), the worst of scientific sins. "Will of the people," "common good," "class interest," "state" are all fictitious entities, too often personified or treated as independent causal agents. Political discourse would continue to use such terms, and with good reason, but a self-conscious science would insist on decomposing all collective nouns into individual, historical components of action. In Weber's words, "'Class as a totality,' 'class interest,' etc., are entirely unclarified collective concepts. *Confusions* hide behind them from the Communist Manifesto on" (AMW 126). Difficulties are compounded when such conceptual language is used to conceal value judgments. Responding to one of Michels's attacks on the state bureaucracy, Weber noted: "The bureaucracy is more and more *necessary*, [you write,] *but* more and more '*harmful for the public welfare*': the *worst* kind of 'value judgments.' *Who* makes up the 'public welfare'? *What* does 'harmful' mean?" (AMW 126).⁹

Weber's criticisms cut to the very heart of Michels's analysis, for that analysis was built in large part on a deliberately naive reading of "democracy" as a desirable moral vision rather than as a historically conditioned system. Faced with this utopian commitment, Weber sought to

⁹ AMW 126 is Weber's critique of *Political Parties*, an undated letter written no doubt toward the end of 1911; Michels alludes to it in his preface to the second German edition ([1925] 1957, p. xxviii). At another point Weber quotes Michels's claim, "The Party is only a means to an end" (Michels 1911, p. 381; [1915] 1962, p. 353), and reminds him that it is "in truth a sentence that refers to an 'ought to be.'" In the second edition ([1925] 1957, p. 366) Michels instead writes, "The Party should be only a means to a higher end." Parenthetically, this passage and others show that in preparing the English translation of his book Michels did not take Weber's 1911 criticisms into account; textual changes made in response to Weber appear only in the 1925 German edition.

trap his colleague in a dilemma: either one could profess the idealist ethics of Tolstoy ("my kingdom is not of this world") or of syndicalism ("the goal is nothing to me, the *movement* everything") or one could affirm and adapt to the achievements of "objective culture" (AMW 59). This statement of choice foreshadowed Weber's later quest for a distinction between an "ethic of ultimate ends" or "intentions" and an "ethic of responsibility." As with that distinction, Weber wanted to counsel responsibility and integrity. To the extent that a commitment to social science defeated the contrasting utopian prospects, one was compelled to reject them in favor of the material and technical achievements of modernity. "Whoever wants to live as a 'modern man,'" Weber warned, "*renounces* all those ideals that hover darkly around you. . . ." He added that Michels would on his own "execute the critique that has long since brought me to that way of thinking and *thereby* stamped me as a 'bourgeois' politician, so long as the least that one as such *can* want doesn't get pushed into the limitless distance" (AMW 59).¹⁰

Confirmation of Weber's prediction would be, as Michels said, a slow process, and it was never fully realized. The vocations of prophet and scientist were carefully separated by Weber, only to be recombined in Michels's moralizing ambition. For instance, responding to a spirited defense of self-sacrificing and unappreciated intellectuals, "voluntarily déclassé comrades," in *Political Parties* (1911, p. 315; [1915] 1962, p. 301), Weber accused Michels of "too much preaching" (AMW 126), a characteristic charge that echoed throughout their exchanges. However, some measure of an altered self-image is suggested in Michels's confident assertion to Schmoller shortly after publication of his most famous work: "I have taken my leave from everyday politics" and "fully devoted my efforts to science" (NS 158, 43-44, Nov. 1, 1911). In writing *Political Parties*, Michels gave free reign to his Weberian intentions, to "dispassionate exposition of tendencies and counter-operating forces" (1911, p. vii; [1915] 1962, p. 6). What these tendencies and forces actually were was not a matter of indifference to the presuppositions of Weberian science either. "Substance" had to respect the implications of adopting a particular "form."

PARTIES AND ORGANIZATION

Returning to the study of political parties proper, the question then becomes, How did Michels apply the norms of social science, as defined by Weber, to his particular subject matter? For Weber, the implications

¹⁰ These remarks were directed specifically at Michels's major essay, "Die oligarchischen Tendenzen der Gesellschaft" (1908b), that "attracted *considerable* attention here" (in Heidelberg), according to Weber.

were clear enough: he advocated the specific thesis that the Party would "inevitably" move toward the machine type and begin to treat politics as the art of achieving tangible, material success. This thesis depended heavily on James Bryce's classic, *The American Commonwealth*, which Weber recommended strongly to Michels on several occasions, once even with the offer to send him a copy (AMW 4, 9, 60, 126). No doubt Weber was struck by Bryce's comparative perspective and by his analysis of the party machines in America. Crediting the Americans with discovering "that the victories of the ballot-box, no less than of the sword, must be won by the cohesion and disciplined docility of the troops, and that these merits can only be secured by skilful organization and long-continued training" (1910, 2:76), Bryce described the 19th-century machine in detail and arrived at a bittersweet reassessment of the realities of "democratic" politics.¹¹ If fed such a strong dose of realism, Weber supposed, not only might Michels admit the machine to his analysis but he might also rethink the meaning of "democratic rule" under modern conditions.

A glance back to Michels's major published work in the *Archiv* from 1906 onward illustrates the fate of these suppositions, for Michels's thinking about Social Democracy reveals a progressive subordination of ad hoc explanations to the significance of "organization" and its practical implications, both sociological and psychological. As mentioned previously, Weber was a careful reader of Michels's writings at this time, working through "two chapters" and a "rough draft" of a projected "book"—the earliest version of *Political Parties*—and the articles that were published periodically in the *Archiv*, thus supplying a progress report on Michels's thought.¹² Weber's perspective and some of his language appear in this work. Thus, paraphrasing Weber's advice, Michels also urges that a scientific critique "must turn to consideration of the *organizational basis* and *tactical method* that give direction to Party politics and that reciprocally condition and limit the spheres of power of Social Democracy's various ranks" (1906b, p. 556). The new sociological approach then reveals a "party of action" requiring "organization" (1906a, p. 80), func-

¹¹ In AMW 126, written in 1911, Weber suggests that Michels would have profited from using the "large edition" of Bryce's book, probably that of 1893–95. The book was published in 1888, revised in 1893–95, and expanded and revised again in 1910; abridged editions were also published, one of which (1907) was used by Michels in writing *Political Parties*. According to Roth, "Since the nineties, Weber had been strongly under the influence of M. Ostrogorski's monumental volumes on *Democracy and the Organization of Political Parties* . . ." (1963, p. 252). The letters to Michels suggest, on the contrary, that Bryce's influence was primary; Ostrogorski is in fact never mentioned.

¹² Weber's comments are found in the letters from March to August 1906 (AMW 3, 4, 6, 8–14). He also read "Die deutsche Sozialdemokratie" (1906b) during this period, promising Michels a "verbal response" to it, conveyed apparently on August 18, when Michels and Jaffe visited Weber in Heidelberg.

tioning as a "status escalator" (*Klassenerhöhungsmaschine*) like any "bureaucratic organization" (1906*b*, p. 543) and becoming a "centralized mechanism" that "endeavors to mold [people] into disciplined Party members, small cogs for its complex machinery" (1907, p. 230). Here then is Weber's party machine, evolving full-scale in an organizational structure parallel to that of the bureaucratic state.

Michels's new "sociological law"—"organization as such requires oligarchy" (1908*b*, p. 111)—has been absorbed by modern social science, even distinguished as a classic contribution, and it requires no detailed elaboration. One should note, however, that to the extent that Michels's completed thesis stressed the technical-structural aspects of party activities, it marked a clear victory for Weberian science. Thus, organization was accounted for by "tactical and technical necessities," including technical specialization, functional differentiation of tasks, large size and scale, professionalization of leadership, disciplined rank and file, efficiency and speed in decision making—all under conditions of democratization and struggle for political advantage. As Weber had insisted, such factors stood "beyond good and evil" (Michels 1911, p. viii) and embraced *all* associations and rational modes of action, regardless of overt ideology.

Of course, the trend toward organization (and, therefore, oligarchy) in parties, not to mention in the state and economy, was open to political interpretation. Michels saw it as the coup de grâce for any lingering belief in democratic-socialist ideals, and he despaired at the possibilities for reviving them. Weber also perceived it as a danger to the revolutionary aspirations of the Party; but unlike Michels, he concluded that it was a positive integrative development that should help eliminate irrational fears among the middle class and foreshadow greater "realism" among socialists. Furthermore, Weber became convinced, contra Michels, that, instead of exacerbating the crisis of leadership, parties organized as machines could function far better than ideological parties as "a recruiting and proving ground of mass leaders as statesmen" ([1918] 1958, p. 391; 1968, p. 1459; cf. Bendix and Roth 1971, p. 251; Mommsen 1974*b*, p. 118). In this view, assuming electoral competition and authentic legislative politics, organization could actually strengthen rather than undermine stable democratic accountability.

There was another side to Michels's effort too, found in a crude psychological reductionism taking "human nature" as the "organic" source of such "natural" propensities as the "need for leadership" and the "gratitude of the masses," a line of thought stemming from Le Bon, Pareto, and Mosca—whose contributions Michels freely acknowledged—not from Weber. On the contrary, Weber and other contemporaries, such as Bernstein (1908), directed some of their strongest objections at Michels's psycholo-

gism and its unverifiable generalizations.¹³ For Weber the important content would remain sociological and structural.

"DEMOCRACY" VERSUS "DOMINATION": RIVAL PROBLEMATICS

Michels conceived his inquiries as a fundamental critique of the possibilities for achieving "popular sovereignty" or "ideal democracy" under modern conditions (1908*b*, p. 73; 1911, pp. vi, 1-13; [1915] 1962, pp. 5, 43-51). Ironically, this "problem of democracy" supplied its own unproblematic answer: "The logical consequence of this hypothesis and scientific conviction [concerning oligarchy] must then—perhaps—consist of completely renouncing every investigation into the limits of oligarchic powers (state, ruling class, etc.) over the individual and of having nothing more to do with pondering the creation of a social order that makes possible the application of the theory of popular sovereignty" (1908*b*, p. 127). In time the "perhaps" was dropped, and Michels joined forces with the "scientific opponents of democracy" (1911, p. 375; [1915] 1962, p. 353).

For Weber, however, it was not so much democracy as bureaucratization that Michels had opened for investigation. While praising the writing of *Political Parties* as an "act of *great* moral courage and moral self-discipline that does not avoid the face of reality, no matter how it looks" (AMW 126), Weber expressed strong reservations about the broader conclusions Michels wanted to draw for the fate of leadership, knowledge, and authority in modern democratic systems. For example, Michels's assertion that "political organization leads to power" which is then "always conservative" (1909, p. 314; 1911, p. 351; [1915] 1962, p. 333) was countered by the reminder that "the power of the Trust Directors has a revolutionary effect, the power of the Jacobins does too" (AMW 126). Or objecting that it was false to defend, as Michels did (1911, pp. 76-86; [1915] 1962, pp. 107-14), the "indispensability" of professional leadership on grounds of cultural or educational superiority among leaders and incompetence among the masses, Weber pointed out that, while leaders' "specialized knowledge" might increase relative to that of their followers, such a tendency said nothing about their "overall educational development" (AMW 126). Moreover, selection as a competent professional leader did not presuppose equivalent competence among followers.

¹³ See, e.g., AMW 126, which includes a notation on a statement in *Political Parties* that modern associations for cooperative production would follow the course charted by old craft guilds "according to immutable psychological laws" (1911, p. 144; [1915] 1962, p. 163). Weber replied, "In my opinion misuse of the concept: '*psychological*' law. Craft unions, etc., are destined to decline because of the condition of their *objective interests*. No '*psychology*' is needed for that." In the second edition Michels wrote instead "immutable *sociological* laws" ([1925] 1957, p. 148; my emphasis).

Using a familiar analogy, Weber noted, "In certain senses the *shoemaker rules over me*, in certain *others I over him*, *despite* his indispensability and personal competence" (AMW 126; see [1918] 1958, p. 388). As for the entire thesis of indispensability, Weber concluded with the characteristic suggestion: "The most objective [*sachlichste*] leader is the most indispensable" (AMW 126; see [1919a] 1946a, pp. 115–16).¹⁴

For Weber, the mere appearance of power, leadership, or professional specialization in an organizational setting could not in itself eliminate all possibility of democratic rule. Following a recent critic of Michels, he might have said, "I shall contest not his facts, but the conclusions he draws from them" (Plamenatz 1973, p. 52). Thus, unlike his colleague, Weber was prepared to see democratization as a typical development in modern societies, just as he was later able to characterize the "often-described organization" of the SPD as "strictly disciplined and centralized" but "within democratic forms" ([1918] 1958, p. 374). However, the divergence in perspectives was much wider than this, for Weber thought that the so-called problem of democracy was itself wrongly posed, admitting of no solution in either the idiom of social science or the practice of a modern "rationalized" and "disenchanted" world. Michels's "ideal democracy" was a historical fiction, his initial desire "to eliminate the domination of man over man" (AMW 59) an intellectual gaffe—and in any case "scientific" opposition to "democracy" was an impossibility. As Weber wrote in his most trenchant comment, "The *concept* of 'domination' [*Herrschaft*] is not clarified in your work. . . . Your analysis [of it] is too *simple*" (AMW 126). This was as much a statement of Weber's own substantive orientation as a criticism of Michels's.

Domination, not democracy, was the problem for Weber's social theory. In fact, it is appropriate to see the analysis of bureaucracy and the sociology of domination, set forth in parts 1 and 2 of *Economy and Society* and written in reverse sequence between 1910–14 and 1918–20 (see Roth 1968, pp. lix, xciv), as a kind of reply to Michels. A major part of Weber's long-term project was to clarify the meaning of domination, to identify the conditions under which and the sense in which different types of domination, democratic and other, were both practicable and in fact practiced. The aim of this clarification was to articulate a more differentiated, historically defensible, and conceptually precise social theory, avoiding the

¹⁴ Responding to these major criticisms, Michels made numerous textual changes in the second edition, especially in the chapter on intellectual factors related to the indispensability of leadership ([1925] 1957, pp. 74–86). This is the only place where Michels attempts a reasonably complete response, even adopting some of Weber's own language (e.g., "The most objective leader would be the most indispensable," p. 86). However, no new analysis of domination appears in these revisions. Michels merely restates his earlier views, summed up in the assertion, "*competence is domination*" (p. 82).

reification of "iron laws" while at the same time replacing Michels's older idealist vocabulary of popular sovereignty with the language of legal and traditional authority, purposive and value rationality, and charisma. Indeed, Michels's perspective was redefined *within* Weber's more complete schema in the concept of "direct democracy free from domination" (*herrschaftsfreie unmittelbare Demokratie*), a rare subtype that could then be analyzed in terms of the sociological conditions requisite for its practice (1964, pp. 214–17; 1968, pp. 289–92). In addition, Weber succeeded in confronting the problem of domination not only at the sociological but also at the political/institutional level, in the latter case eventually proposing a constitutional scheme for "plebiscitary leader democracy." The interpretation of these ideas belongs to a later time and place, however, for this aspect of Weber's thought came to fruition after the collaboration with Michels had ended.

Thus two contrasting scientific "problematics" underlie the divergences in Weber's and Michels's interpretations of the bureaucratic phenomenon. On this score, because of its sociological rigor and freedom from atavistic moralizing, Weber's understanding of the actual contribution of *Political Parties* can appear superior to Michels's and more productive theoretically. When Weber's own categories are used, it is equally instructive to note how the contrasting judgments suggest a subtle disagreement concerning "value relevance" (*Wertbeziehung*), in which different "evaluative ideas" (*Wertideen*) are employed not merely to select a subject matter but—more important—to define the relevance of the analysis of organization to certain cognitive goals and cultural values. Once, when affirming the practicability of "political democratization," Weber remarked, "I can't keep you [Michels] from believing in more, nor can I force myself to do so" (AMW 50). But in the final analysis Michels *wanted* to believe in more. He aimed to uncover the factors obstructing ideal democracy; Weber, the conditions associated with domination; ultimately one wanted a knowledge of "utopian possibilities," the other knowledge "without illusions."

CONCLUSION

Michels actually did respond to Weber's later analysis of domination, but after Weber's death. He occasionally cited the *Economy and Society* discussion of charisma and plebiscitary rule in the context of his own consensus theory, which was designed to reconcile democratic with aristocratic principles and thus to serve as an apology for Mussolini's "democratic dictatorship" ([1922] 1925; 1927*a*, pp. 90, 111, 122–23; 1927*b*; 1927*d*, pp. 753–57). Of course, Weber's concept of charismatic domination was not entirely immune to such uses, as critics have repeatedly complained (Friedrich 1961; Röhrich 1972, pp. 151–67; Mommsen 1974*a*, chap. 4),

and one can only speculate about how Weber might have replied to Michels's interpretation of it. One can be certain, nevertheless, that, caught up in the ideological politics of the literati (one of Weber's favorite targets), Michels consistently ignored or repudiated the other parts of Weber's thought concerned with a comprehensive sociology of legitimate domination and with legislative politics and institutional controls. True to the scientific analysis of 1911, Michels's understanding of "domination" and "authority" remained spare and unenlightening (see 1927*b*; 1930), and true to his political convictions of 1906, he reverted in the 1920s to repudiation of parliamentarism and representative institutions.

Despite Weber's perceptive commentary, Michels could never quite escape an *idée fixe*, a changing vision of moral and political perfection—first socialism and syndicalism, then Italian nationalism and fascism—that led toward those "miserable monstrosities" of "academic prophecy" that Weber abhorred ([1919*b*] 1946*b*, p. 155). It was characteristic at the end of their association to find Weber defending Michels from scurrilous press attacks, yet remonstrating that Michels's honest defense of his political activities on grounds of "pure idealism" was simply an admission of "immaturity," an innocent display of "*Kitsch* from the political nursery" (NW 30/14, 13–14, June 20, 1915; NS 157, 1–4, Weber to Schmoller, Jan. 10, 1916). Borrowing from Weber's terminology, one might say that Michels's experimental moral existence embodied a kind of ontological "ideal type," a living instance of the "one-sided accentuation of reality" so characteristic of those romantic *Gesinnungsethiker* who could readily switch back and forth between revolutionary and reactionary beliefs in the name of "pure idealism" (see Weber 1906*a*, p. 322). In this case, cathectic means simply replaced cognitive ends. Surely Weber was correct to foresee that the most probable outcome of such experimentation with a "romanticism of the intellectually interesting" ([1919*a*] 1946*a*, p. 115) was inner collapse, disillusionment, escape from scientific freedom, and a search for authoritarian political solutions.

These scientific and political divergences were reproduced in the interpersonal dynamics of this unique and stormy partnership. Weber's frank appraisal, "You know you are something of a 'ruthless personality' [*Gewaltmensch*] and somewhat touchy when you run into opposition" (AMW 58)—written to Michels concerning his request to join the *Archiv* as co-editor—could have applied equally well to himself. In the end the clash of demonic personalities left too little room for continued cooperation: Michels chose to be a "déclassé intellectual" and "renegade," Weber a "bourgeois critic" and "outsider." Inserting a final evaluative idea, one might add that by insisting on enchantment, on a unity of "vocation" and "life," Michels proved himself only a typical representative of the literati; whereas by attempting to demonstrate the practice of scientific and political

vocations in a world without enchantment, Weber achieved a standing *sui generis*.

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The Vesting of Interests and the Determinants of Political Power: Size, Network Structure, and Mobilization of American Industries, 1886–1905¹

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The issue addressed is, What does a social group need to gain political power? Empirical, historical analysis is utilized to explore the relative saliency of three determinants of political power to explain variation in power as defined conceptually and operationally independent from its determinants. The determinants are magnitude, the extent to which the group is tied into economic, political, and social networks, and the degree of mobilization. The aspect of power investigated is the vesting of economic interests, that is, the extent to which the interests of an actor are taken into account by the government in its routine operation. Regression analysis is used to assess the relative explanatory power of the magnitude, network relations, and degree of mobilization of 12 industries on the vesting of economic interests in the U.S. Department of State in the period 1886–1905. Results show that the different components have different explanatory value under varying conditions, depending on the type of interest that is vested. When the State Department was discussing export-related matters the value of exports was the best predictor of vesting, with mobilization having a secondary impact. However, the effect of network relations was notably weak, contrary to predictions of the power-elite perspective. The conclusion stresses the contingent nature of the determinants of political power.

Our admitted preponderance in the world's markets gives enormous power which has its obviously great responsibilities, its lurking perils. . . . Is it not true that the highest industrial efficiency provides the broadest, most enduring base for general progress and enlightenment? Admitting this, may we not conclude that, in the end, the spread of American trade, of American ideas, of American enterprise and ingenuity may be made beneficent not merely to ourselves, but to all mankind? [FRED-

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ERIC EMORY, Chief of the Bureau of Foreign Commerce, Department of State (Emory 1901)]

One of our greatest helpers has been the State Department in Washington. Our ambassadors and ministers and consuls have aided to push our way into new markets to the utmost corners of the world. [JOHN D. ROCKEFELLER (cited in Collier and Horowitz 1976)]

The benefits achieved in the pursuit of the national interest do not accrue equally to all members of a population. Some benefit more than others. Regardless of the sincerity of Emory's concern for "all mankind," Rockefeller was a chief beneficiary of the responsibilities assumed by the U.S. State Department in the late 19th century. The special place of the Standard Oil Company in the "national interest" at that time is a question of political power.

One of the fundamental theoretical questions about power concerns the determinants of power: What does a group need to gain power in a polity like the modern national state? How do politically powerful groups differ from powerless ones? For example, is a large resource-rich but dispersed group more strategically situated for gaining power than a smaller but tightly cohesive group with a specific type of resource? Are there some circumstances in which the large dispersed group is better situated than the small group, but other situations in which the small group has the advantage? With few exceptions (e.g., Moore 1958; Stinchcombe 1968), the issue of the organizational characteristics that differentiate politically powerful groups from powerless ones has been overshadowed by the debate over the distribution of power, the pluralist-elitist debate. Nonetheless, it is an issue central to any comprehensive theory of power.

If one reads the basic sociological literature with an eye toward answering the question, What does a group need to gain political power? one finds few explicit answers. One has to read between the lines. This paper considers three possible answers: (1) *Magnitude*: a group needs to be large. For example, some accounts argue that big business is powerful simply because it is big. (2) *Networks*: power is a function of the roles filled by actors and actors' relationships with each other. (3) *Resource mobilization*: the collective use of material and symbolic resources is a precondition of political power. The magnitude perspective is interested in how *large* a group is, the network perspective looks at *who* one knows, while the mobilization perspective concentrates on *what* a group can put to practical use. These perspectives are not contradictory; the complete picture of power includes them all. The goal of this study is to examine how magnitude, network position, and the mobilization of resources are empirically associated with the political power of several American industries in the late 19th and early 20th centuries.

Power and the Vesting of Interests

Power has never been definitively conceptualized, much less adequately measured. This study is no exception. Researchers have generally isolated some component of power and measured how groups differ on that. Some have looked at the influence that groups have over decisions of other groups (Dahl 1957, 1967). Others have identified power by reputation (Hunter 1953, 1959). Still others have looked at characteristics of role incumbents and the overlapping of roles (Domhoff 1967; Mills 1956; Dye 1976).

The dependent variable in this study is labeled the "vesting of interests."² Vesting means that the interests of a group are routinely taken into account by public authorities in the handling of state affairs. It differs from the notion of "objective interests" in that it includes a characteristic of state officials as well as the situation of the group whose interests are being vested.

Tilly defines power as the net return gained by the collective application of resources. In his view, power "is a ratio describing what happens over some defined period of time when group A applies resources to the influence of group B and group B responds by yielding other resources to group A" (Tilly 1970, p. 16; see also Lukes 1974). One type of return gained by the application of resources is being taken into account by the government in administration and decision making. The interests of powerful groups, whether or not they always prevail, are taken into account on decisions which affect them. The interests become the responsibility of the government agent, who perceives them as under his jurisdiction. The interests are vested. Vesting then connotes both a relationship between actor and government and a political status of an actor. There are two relevant dimensions of the concept: (1) the weight that the interests carry when

² The particular phrase "vesting of interests" was suggested to me by William Gamson. It was also used by Veblen (1920), who argued that the vested interests dominated the era of corporate capitalism. His usage is somewhat different from mine, however. According to Veblen, "a vested interest is a legitimate right to get something for nothing, usually a prescriptive right to an income which is secured by controlling traffic at one point or another" (pp. 161-62). It is income derived not from productive labor but from "an established order of ownership and privilege" (p. 162). His distinction between the vested interests and the common man meant nonproductive vs. productive labor. Since the vested interests would lose their wealth in a free market, their interests are "vested" in the status quo. Veblen's prototypical vested interests were monopolists and speculators. My concept is similar in that vested interests are gained through structures of ownership and privilege. However, I conceive of interests as "vested" in relationship to specific social structures, in this case the state. Moreover, I do not see that vesting necessarily involves nonproductive wealth. Both productive and nonproductive wealth depend on privilege and ownership. Coleman (1974) has used the term in discussing the legal evolution of the corporate body. He points out the significance of the legal sanction of individuals' vesting their resources—and hence their interests—in corporate bodies. Eventually, corporate bodies were able to act on behalf of both their interest and the vested interests of members and investors.

compared with conflicting interests (the heavier the opposition withstood, the more power), and (2) the scope of issues and decisions for which the interests are considered (the greater the scope, the greater the power). This study deals with the latter dimension.

Magnitude

The magnitude perspective assumes that the government would apply some universalistic criteria to all members of a class of actors regardless of inclusion in an elite or political mobilization. The government itself is seen as acting autonomously to apply "objective" criteria. If the government is seeking to bolster the economy, policy would be oriented toward the largest industries. If it is facilitating exports, large exporters would be taken into account. Thus government action is seen as a "public good" that benefits the members of a class proportionate to "objective" rather than "political" criteria (Olsen 1971). The magnitude perspective would predict that the interests of industries large in aggregate size would be more likely than those of small industries to be taken into account (vested) by the government. But there is little theoretical development of the magnitude concept in the literature on power. It is invoked most typically in ad hoc accounts of particular historical events.

Networks

The network perspective treats power as a consequence of one's position in the social structure. A small, fairly homogeneous, interacting set of actors who occupy certain strategic roles wields power. Cohesiveness is seen as one of the primary sources of their power.

Those social scientists identified as "power elitists" have argued that there is a group of people distinguishable from the general population who control the central social institutions (Domhoff 1967, 1971, 1974; Dye 1976; Hunter 1953, 1959; Mills 1956). Because of the institutionally strategic position of the elite individuals (Dye 1976; Mills 1956; Stanworth and Giddens 1974), and because of their common orientation and world view (Domhoff 1967; Hunter 1953, 1959; Mills 1956), they are able to control the instruments of government effectively. Because the elite control the instruments of government, the government operates in their interest. Thus, in order to have the government act consistently in one's interests, one would have to become affiliated with the elite network.

Others not associated with the power-elite camp have also treated power in a network fashion (Laumann and Pappi 1976). Deutsch (1966) describes government as a communications net, and the political process is seen as information flow. Bauer, Pool, and Dexter (1972) explicitly eschew

the elitist as well as the pluralist camp, constructing a middle ground based on communication from businessmen to government decision makers.

The network perspective would predict that industries whose leaders hold commanding positions in the economy, polity, and society would be more likely than other industries to have their interests vested in government agencies.

Mobilization of Resources

Mobilization refers to the process by which a group changes from a passive social grouping to an active participant in social life (Tilly 1978). It embodies a component of organization and a component of activity. Tight organization facilitates the mobilization of resources, which are necessary for political contention. Organization differentiates mobilization from magnitude by delivering resources for the pursuit of collective goals. Resources are useless until they are organized and applied in action. In this perspective there is no power without political action and no action without the mobilization of resources.

Two families of literature fit the mobilization perspective on the question of what a group must possess in order to gain political power: the pluralist approach, including interest-group studies, and the resource-management perspective on collective action. Pluralists maintain that different decisions are decided upon by different sets of decision makers who control different resources (Riesman, Glazer, and Denney 1950; Rose 1967; Dahl 1957, 1967; Truman 1971). Proponents of the resource-management perspective have concentrated on the mobilization of material resources for social movements and political contention (Tilly 1973, 1978; Gamson 1973; Oberschall 1973). Differential access to resources implies differential political power. The mobilization perspective would predict that groups gain political power through the mobilization of resources. Industries whose interests are successfully vested should show some evidence of using lobbies, organizing campaigns to influence politicians, making campaign contributions, or organizational capacity for mobilization.

Thus we have two determinants of power that are well established in the literature on power—network relationships and the mobilization of resources—and one determinant that is commonly found in historical treatments of particular historical groups—magnitude. The question addressed by the empirical analysis of this paper is the extent to which each determinant explains variation in one aspect of political power among a sample of essentially similar social groups. In particular, I examine the variation in the extent of vesting of interests in the conduct and administration of U.S. foreign policy among a sample of 12 industries. It is hypothesized that those industries whose political interests are vested should (*a*) be larger,

(*b*) be more tightly connected into an elite network, and (*c*) show greater capacity and more sustained application of resources for political activity than those industries whose interests are not vested. The paper will examine each component of power separately and then all the components together using basic linear regression analysis.

METHODS

Data for the period 1886–1905 were collected for vesting and the three determinants of power: magnitude, network position, and mobilization activity. Twelve industries were randomly sampled from the Industrial Classification of the National Bureau of Economic Research (NBER) (Evans 1943, pp. 50–51).³ The operational definitions for the industries were adapted for the purposes of this study from the *Standard Industrial Classification Manual* (U.S. Technical Committee on Industrial Classification 1942).

The dependent variable was coded from content analysis of correspondence between the State Department in Washington and American embassies and legations. Vesting of interests was operationalized as the relative frequency with which an industry was mentioned in these documents. All letters and telegrams sent by the State Department in Washington along with all letters and telegrams coming in from the embassies were included. Five countries were randomly drawn from countries which had complete sets of documents for the years 1886–1905 (U.S. National Archives 1963). Great Britain was included as a class by itself, since the Court of St. James was the center of international diplomatic activity, making a total of six countries. About 8,360 records were read.

Coders read each sampled record to see if there was any reference to an industry, defined as a profit-making enterprise carried on by a private corporation, company, or trade association.⁴ Typically, industries were mentioned by reference to their product, although reference to a company or a set of companies was common. There were some references to companies as employers. In order to check coder reliability, 11 volumes of Despatches from the Mexican embassy, covering three and one-half years, were read and coded by three coders. Inter-coder reliability proved satisfactory.

There are several reasons that the number of mentions of an industry in

³ The "triple-letter" categories were used as the sampling unit. See Roy (1977) for elaboration and limitations of the sampling procedure. The final sample was agricultural machinery, book publishing, boots and shoes, coal mining, distilled liquor, lumber milling, meat packing, paper and paper products, petroleum refining, railroads, sugar refining, and telegraph.

⁴ Excluded from coding were mentions of an industry that involved the routine purchase of supplies or use of services such as sending a telegram or a coal purchase.

department documents should be indicative of vesting:⁵ (1) Vesting is treated as the degree to which the interests of an industry are taken into account in the routine administration of affairs. In the discussion of particular issues, nonpowerful actors are often capable of having apparent impact and even short-range success. Over a period of decades, however, only highly powerful actors can consistently be taken into account in the minor affairs of state as well as the major ones. While grand policy discussions can effectively mask the economic interests being served, routine administration often requires explicit mention of particular products, companies, or businessmen. (2) These documents record the complete interaction between the offices. There were no telephones, and personal contact was rare. Frequent discussions of a candid nature were found in both confidential and nonconfidential letters and telegrams. (3) There was little effort to mask references to economic interests. The State Department legitimately represented American economic interests abroad. Although there was some public resentment against big business within the United States, its international activities were nationalistically approved. (4) Industries mentioned included those that had no "objective" interest in foreign trade, indicating that counting mentions taps political considerations. The industry that was mentioned more than any other coded industry was the railroad, which had no imports or exports.⁶

The independent variables constructed for the industries fall into three major groups: magnitude variables, network variables, and mobilization variables.

Magnitude variables.—Several variables that measure the size and international trade of industries have been taken from published sources (which draw primarily from government statistics). The magnitude variables included in this study are: (1) The value of capital engaged in producing the product, from the U.S. Census of Manufacturers (U.S. Bureau of the Census 1880, 1890, 1900, 1905).⁷ This is not book capital, but the value of physical capital actually engaged in production.⁸ (2) Data for imports and

⁵ Although particular circumstances may distort measuring degree of vesting by number of mentions at any particular time, it is felt that such disturbances will even out over several years and several countries.

⁶ Railroad companies were involved in international investments, but international investments was the subject matter mentioned least of all economic topics.

⁷ Creamer, Dobrovolsky, and Borenstein (1960), of the National Bureau of Economic Research, while acknowledging that some economists had been skeptical of these figures, compared 1919 figures with independent data and concluded that the U.S. Census of Manufacturers data were of acceptable accuracy.

⁸ The total capital stock and funded debt of the railroads were used in place of value capital, since they were the only statistics available. The figures were included in the 1907 *Statistical Abstract of the United States*, as compiled by the editor of *Poor's Manual of Railroads*. The figures included a substantial portion of "water" and are to be considered inflated.

exports were drawn primarily from the *Statistical Abstract of the United States* (various issues) and Lipsey (1963). (3) A capital intensity index, though not strictly a measure of magnitude, was included. It is the value of capital (as described above) divided by the value of annual production, as reported in the U.S. Census of Manufacturers (U.S. Bureau of the Census 1880, 1890, 1900, 1905).

Network variables.—Analyzing the network relations of the industries involved collecting data on three levels: the company, the names of individual officers and directors, and biographical data on the individuals identified.

To be included, companies were required to have \$1 million capitalization, which the NBER has used as the criterion of a large corporation (Evans 1943), or if unincorporated, \$1 million in assets. However, railroads were given a bottom cutoff point of \$25 million, since there were so many highly capitalized companies. About 150 primary and secondary sources were used to identify the qualifying companies and the names of their major officers (president, vice-president, secretary, and treasurer) and directors (or partners of unincorporated companies) for each year, 1886–1905. The sources identified over 25,000 company-director years for over 4,200 individuals in 333 companies. Many represent the same director in the same company in successive years. While incomplete for some companies, this is, as far as I know, the longest continuous set of annual directorship data on record.

Once the individuals holding leadership positions in the companies were identified, their biographies were coded from the *National Cyclopaedia of American Biography*.⁹ The coding operated under the principle that the individual leaders of companies who held positions in other organizations linked the company with the other organizations and indirectly with the members of the other organizations. This coding was designed to indicate interaction within a network structure. The units coded were therefore *affiliations*, that is, memberships or formal ties of officers and directors with other economic, political, or social organizations, linking the company and other organizations. Thus, a mention that the person “wrote extensively on tariff reform” would not be coded, while the fact that he “was active in the tariff reform movement” would be included in the data set. The former was not an organizational affiliation, while the latter was.

The analysis included the coded biographies of 780 individuals, for whom 10,149 affiliations were coded. Since some individuals were officials in more

⁹ The *National Cyclopaedia of American Biography* is one of two major biographical dictionaries for American notables of this period, the other being the *Dictionary of American Biography* (the well-known *Who's Who* is a more recent publication). An annual, it was begun in 1890, explicitly to lionize the lives of men who made notable contributions to American economic life.

than one sampled industry, the data, when analyzed for variation among industries, included 13,097 links.

The types of affiliations (organizational relationships) were typed in three groups: economic, political, and social. Within each group, affiliations were also coded according to the type of relationship to the organization (e.g., corporate director, political incumbent, social member), as well as according to a more detailed breakdown on the type of organization (the industry and size of corporations, the branch and level of government, and the particular area of social life).

Mobilization variables.—The strategy for the mobilization data in this study was to identify a few concrete phenomena that indicate ability to pool and apply resources on behalf of an industry. For each general phenomenon, a protocol of questions (primarily closed-end items) was used to "survey" all the material on the sampled industries. Mobilization variables included are:

1. Capital concentration: Capital concentration is a surrogate for the ability to mobilize and apply resources. An industry that is dominated by a few firms can mobilize and apply resources more easily than one that is decentralized into many firms. Mobilization implies not only aggregate availability, but also control of resources. If all the resources of an industry are under a single control, resources can be better coordinated for the achievement of goals than if many different independent companies must agree not only on goals, but also on the relative share of resources contributed to achieve the goals.¹⁰

The concentration ratio, that is, the percentage of total capital of an industry controlled by the four largest firms, is used as the measure of concentration.¹¹ Concentration ratios for 1890, 1900, and 1905 were calculated by dividing the authorized capital stock of the four largest firms in each industry by the value of capital of the industry as reported in the Census of Manufacturers.

2. Protocol of activities: Actual mobilization was examined by enumerating reported attempts to intervene in the political process. The protocol itemized several political actions that might be undertaken by industries, including: (1) lobbying; (2) attempts by corporate officials to influence

¹⁰ Two other advantages accrue to monopolistic industries. First, a monopoly is an example of Olsen's "privileged groups," that is, actors who constitute a large enough proportion of a collectivity so that "public goods" gained for the collectivity will cover the costs of mobilization (Olsen 1971). Second, monopolistic firms are, by definition, free of market constraints on pricing (Scherer 1971; Shepherd 1970) and thereby have more net profits for conversion into organizational resources.

¹¹ Economists (Scherer 1971; Shepherd 1970; Universities National Research Bureau Committee for Economic Research 1955) agree that this measure is generally the easiest to obtain from readily available information and is correlated on the order of .8 and .9 with other, more sophisticated measures of concentration, such as the Gini index.

political events, such as visiting politicians, appearing before committee hearings, etc.; (3) nonpolitical (i.e., business or social) contact with political figures; (4) political party involvement of industry officials; (5) campaign contributions; and (6) bribes, special favors, payoffs, or other illegitimate activities.¹²

About 170 sources were consulted for mobilization activities, including standard economic histories, industry and company histories, government reports, and trade journals.

RESULTS

The relationship between vesting of interests and magnitude variables, network variables, and mobilization variables is examined using elementary correlation and regression. Two caveats must be made. (1) The unit of analysis for these computations is the industry time period. The period from 1886 to 1905 was divided into four time periods in order to incorporate variation across time as well as variation across industries. To insure that the correlations and regression coefficients were not inflated by autocorrelation over time, the computations were repeated using analysis of covariance, holding time constant (treated as a nominal variable). The results were consistent with those obtained when time was not controlled, but the estimates were too unstable to be treated as reliable for their own sake. Thus it was concluded that time was not a confounding factor for using the industry time period as the unit of analysis.¹³ (2) The railroad industry was found to be an outlier in the computations since its level on the variables was extremely high. When it was included, the covariance on nearly all variable pairs was due largely to the effect of the railroad industry. The railroad industry was the highest on level of vesting, the magni-

¹² The nature of the evidence for the data was also coded. This was especially important for secondary sources, which sometimes make rather bald assertions with only the slightest support from facts. The evidence was coded according to whether it was (1) an allegation; (2) a formal conclusion of a public or private body, such as a court decision or a congressional hearing; (3) a matter of contemporary consensus; (4) a matter of scholarly consensus; (5) an unsupported statement of a secondary source; (6) an assertion acknowledged by an authoritative official of the industry; (7) direct, tangible evidence, such as documents; or (8) other kinds of evidence. I then attempted to locate the original source of the information if given in the source being coded. Analysis using different levels of "proof" showed essentially similar results. The results reported include all information except allegation. Since the study of mobilization is concerned with the extent to which certain actions took place, selective coverage by information sources is a serious problem. If industry A is found to have undertaken more mobilization actions than industry B, we need some sort of assurance that the finding is not merely a function of more complete coverage of A than of B. Some compensation for unequal coverage was gained by digging deeper for the less thoroughly documented industries.

¹³ A similar strategy was adopted by Hibbs (1973).

tude variables, the network relations, and the mobilization variables. The methodologically conservative approach chosen was to eliminate the railroad industry from correlations and regressions and to discuss it separately. Thus, the reported coefficients underestimate the positive relationship among these variables in all 12 sampled industries.

Magnitude

Value of capital.—Between 1889 and 1904 the book value of total capital in the economy increased by 103%, from \$5,697 million to \$11,588 million (Creamer et al. 1960). Thus, over those 16 years, the value of capital in manufacturing more than doubled. What is of interest here is the extent to which the vesting of economic interests was a function of magnitude. The correlation between the value of capital and the degree of vesting for the industries other than railroads was $-.19$. Thus, there was a weak negative relationship between capitalization and vesting. Although weak, this relationship is in the opposite direction from what would be expected. A large value of capitalization was not associated with vesting by the State Department.

Exports.—The lack of a positive relationship between vesting and aggregate size of industry is not especially surprising. Some large industries were primarily domestic, and some small industries may have conducted a large proportion of their trade abroad. Perhaps vesting is not related to aggregate size but to the value of exports and imports.

The value of exports grew rapidly over this period. In the 20 years from 1886 to 1905, American exports increased from \$666 million to \$1,492 million, an increase of 124% (Lipsey 1963). By 1893, the value of American exports exceeded that of every country in the world except Britain (LaFeber 1963).

LaFeber has argued that the government was strongly committed to encouraging American exports as part of "a general consensus reached by the American business community and policy makers in the mid-1890's that additional foreign markets would solve the economic, social, and political problems created by the industrial revolution" (1963, p. 412). He emphasizes the role of agricultural exports in adjusting the balance of trade, the disruption of deeply rooted economic relationships by the depression of 1893, and the hope that increased exports would ease the glut of overproduction by American manufacturers. Although exports never accounted for a large proportion of the GNP, LaFeber argues that they were the cornerstone of economic foreign policy in the Gilded Age.

The results indicate that the government was, indeed, concerned with the issue of exports. The State Department discussed exports far more

than any other economic topic, accounting for 27.4% of all documents coded.

Moreover, the interests of industries with a high volume of exports were more likely to be vested than industries with a low volume of exports. The zero-order correlation between the value of exports and the degree of vesting in the State Department records was .14.

Further examination, however, shows that the relationship between vesting and value of exports held only for those documents that concerned American exports. If one defines export-related vesting as the frequency with which an industry is mentioned in documents whose topic is exports and general vesting as the frequency in all other documents, an interesting divergence emerges, suggesting two document-producing processes. The zero-order correlation between general vesting and value of exports was $-.22$, a negative relationship. However, the zero-order correlation between export-related vesting and value of exports was a strong $.49$. High exporting industries had their interests vested in export-related matters, but such vesting was not generalized to other matters.¹⁴

The single variable most related to vesting in general documents was the capital intensity index, with a zero-order correlation of $.44$. The relationship between general vesting and all other magnitude variables was negative and small. On the other hand, the correlation between capital intensity and vesting in export-related documents was $-.16$. Thus export-related and general vesting followed dramatically divergent patterns.

If export-related and general vesting are both standardized and regressed against value of exports and capital intensity index, this dual process is starker. Table 1 shows the decomposition of variance for these regressions.

TABLE 1
REGRESSION OF VALUE OF EXPORTS AND CAPITAL INTENSITY ON
EXPORT-RELATED VESTING AND GENERAL VESTING

DEPENDENT VARIABLE	INDEPENDENT VARIABLES	
	Value of Exports	Capital Intensity
Export-related vesting ($N = 30$) ($R^2 = .281$):		
b691	.002
SE.....	.223	.238
β530	.002
General vesting ($N = 30$) ($R^2 = .233$):		
b	-.388	.820
SE.....	.352	.376
β	-.195	.386

¹⁴ Separate analysis indicates that it is the absolute value of exports that is associated with vesting, not the proportion of a product that is exported. Thus it is the magnitude of exports, not the industry's dependence on them that matters.

In the regression computation with export-related vesting as the dependent variable, the standardized regression coefficient for value of exports was .53, while the coefficient for capital intensity index was .00. The variance explained is over a quarter, 28%. Thus, the value of exports was strongly related to export-related vesting, independent of capital intensity.

The vesting of interests by high-exporting industries was specific to discussions of export-related matters. When general vesting was regressed against value of exports and the capital intensity index, the standardized regression coefficient of the capital intensity index was .39 while that of the value of exports was $-.20$. The total variance explained is nearly a quarter, 23%.

Thus, there were two informal document-generating processes at work—one generating documents that discussed exports, and one generating documents that discussed general subjects. This divergence between export-related and general documents is important because it indicates that vesting of interests can be a specialized relationship even within a single agency. Exporters' interests concerning exports were vested. But in general matters they had no special claim on the State Department's attention. Vesting can be a general or specific relationship between political actors and a state agency.

The Network of Corporate Elites

The institutional network of the sampled industries was analyzed through the links created by formal affiliations held by officers and directors of corporations. Techniques of collective biography showed that each individual had an average of 13.0 links, of which 8.3 were economic, 1.5 were political, and 3.2 were social.

Thus, as one might expect with a group of officers and directors of large corporations, most of their affiliations were with other economic organizations. And insofar as personal affiliations represent interorganizational relationships, the companies were more tightly tied to other economic organizations than to political or social organizations.

The number of total affiliations/person increased over the period from 10.61 affiliations/person to 12.84 affiliations/person, indicating that corporate officers were broadening their organizational life. The number of economic and social affiliations/person increased, while the number of political affiliations/person declined slightly over the period.¹⁵

The network perspective suggests that industry vesting is related to the extent to which industries are tied into the network of the ruling elite. According to this perspective, such "objective" factors as the value of ex-

¹⁵ See Roy (1981) for a development of implications of declining political affiliations.

ports or capital intensity of industries should be related to vesting only as mediated through network relations.

Since vesting for export-related and general matters exhibited divergent patterns in the analysis of the magnitude variables, that distinction is maintained. The correlation between export-related vesting and the number of affiliations/person was .15, while the correlation between general vesting and the number of affiliations/person was .20. Thus vesting had a weak to moderate relationship to inclusion in the network of elites, when no other variables were held constant. Economic, political, and social affiliations all showed a similar pattern in their relationship to export-related and general vesting.

However, when magnitude variables were held constant the relationship between export-related vesting and network relationships disappeared. Regressing export-related vesting on value of exports and number of affiliations/person (table 2) showed that inclusion in the elite network did not explain vesting: the standardized regression coefficient was trivial. But the coefficient for value of exports was quite substantial (.44). The State Department focused its attention on industries that had a large amount of exports, not on those tied into elite networks. Having export-related interests vested in the State Department was a specialized status held by large exporters.

Integration into the elite network was very weakly related to the vesting of general interests in the State Department. With capital intensity held constant, the standardized regression coefficient of total affiliations/person on general vesting was .07. There was some divergence among types of affiliations—being tied into the economic network does not necessarily mean that an industry was tied into the political or social network. The social network was somewhat isolated from the economic and political net-

TABLE 2
REGRESSION OF NUMBER OF AFFILIATIONS/PERSON, VALUE OF
EXPORTS, AND CAPITAL INTENSITY ON EXPORT-RELATED
VESTING AND GENERAL VESTING

DEPENDENT VARIABLE	INDEPENDENT VARIABLES		
	Affiliations/ Person	Value of Exports	Capital Intensity
Export-related vesting ($N = 44$) ($R^2 = .207$):			
b041	.602	...
SE.....	.107	.195	...
β054	.440	...
General vesting ($N = 30$) ($R^2 = .202$):			
b073897
SE.....	.193386
β070422

works. The zero-order correlation among economic, political, and social affiliations/person ranges from .27 for economic and social affiliations/person to .59 for economic and political affiliations/person.

Economic and political affiliations had similar weak effects on general vesting. With capital intensity held constant, the standardized regression coefficient of economic affiliations/person was .13, while the coefficient of political affiliations/person was .18 (table 3). Social affiliations/person had no effect whatsoever; the standardized regression coefficient was virtually zero. Although not reported here, results of affiliations with specifically upper-class organizations were comparable. Thus those industries tied into the nation's elite network had little advantage over other industries for general vesting in the State Department. Even those whose officials were politically active in public office had only a minor edge. Industries whose officials were members of social elite organizations fared no better than those run by uninvolved, purely business officers.

Thus the network perspective finds little support. During this period, one of intense economic and political change, it was a very weak determinant of vesting. The results here indicate that inclusion in the elite certainly did not explain political power.

This is not to negate the existence or importance of an elite network in other historical eras, including late 20th-century America. The results do not rule out the possibility that the elite network has become an increasingly strategic component of political power. Rather, they suggest that

TABLE 3
REGRESSION OF ECONOMIC AFFILIATIONS/PERSON, POLITICAL AFFILIATIONS/
PERSON, AND SOCIAL AFFILIATIONS/PERSON ON GENERAL VESTING,
HOLDING CAPITAL INTENSITY CONSTANT

DEPENDENT VARIABLE	INDEPENDENT VARIABLES			
	Capital Intensity	Economic Affiliations/ Person	Political Affiliations/ Person	Social Affiliations/ Person
General vesting ($N = 30$) ($R^2 = .211$):				
b832	.209
SE.....	.400	.310
β392	.127
General vesting ($N = 30$) ($R^2 = .229$):				
b	1.014	...	1.756	...
SE.....	.365	...	1.685	...
β477179	...
General vesting ($N = 30$) ($R^2 = .198$):				
b944008
SE.....	.373562
β444003

during the era in which the corporate sector was emerging and gaining political power, factors other than elite cohesiveness were decisive.

Political Mobilization

The measure of mobilization used was the capital concentration ratio, as described in an earlier section. The concentration ratio was positively related to vesting for both export-related and general documents. As seen in table 4, the standardized regression coefficient for the concentration ratio with export-related vesting was .44 (value of exports held constant), while for general vesting, the coefficient was .38 (capital intensity held constant).¹⁶ Thus, while highly concentrated industries were more likely to be taken into account by the State Department than nonconcentrated industries on all types of subjects, there was a slightly weaker relationship between concentration and vesting for general matters than for export-related matters.

The degree of political activity was measured by the number of instances in which the industry intervened in the political arena, as described above. The isolated effect of political activity on the vesting of interests was only moderate. The total number of political activities of the industry was a

TABLE 4
REGRESSION OF CONCENTRATION RATIO, VALUE OF EXPORTS,
AND CAPITAL INTENSITY ON EXPORT-RELATED VESTING
AND GENERAL VESTING

DEPENDENT VARIABLE	INDEPENDENT VARIABLES		
	Concentration Ratio	Value of Exports	Capital Intensity
Export-related vesting ($N = 44$) ($R^2 = .391$):			
b021	.719	...
SE.....	.006	.162	...
β438	.526	...
General vesting ($N = 30$) ($R^2 = .319$):			
b028614
SE.....	.013376
β381289

¹⁶ The relationship between the concentration ratio and general vesting is slightly clouded by moderate multicollinearity in the independent variables. Highly concentrated industries tended to be capital intensive industries. The zero-order correlation between the concentration ratio and the capital intensity index is .41. This means that the relative estimates of the regression coefficients are not as precise as they would be for noncorrelated independent variables. The two independent variables are both correlated with the general vesting in zero-order correlation (.47 for the capital intensity index and .45 for the concentration ratio). Together, they explain 32% of the variance of general vesting. For our purpose, however, the precise allocation of effect between the independent variables is not as important as the general relationship.

determinant of vesting for both export-related and general vesting (table 5). Regardless of subject matter, those industries with overall extensive political activity were more likely to be taken into account by the State Department than those with low political activity.

Industries that are conventionally considered powerful, such as petroleum refining or telegraph, showed up here as powerful. They were also the industries that were organized monopolistically and that were active politically.

Magnitude, Network, Mobilization, and the Vesting of Interests

A general goal of this study has been to examine the extent to which the vesting of interests by industries in a government organization was a function of the industry's magnitude, its network relations, and its ability to mobilize resources for political contention. Each concept has been discussed separately. This section will examine their relative effects when all are considered together. A regression equation with a representative variable from each of the three concepts will be used, but we must be aware of two limitations. In the first place, the model is not specified precisely: the regression takes an additive form even though there are certainly interactive relations among the variables. For example, mobilization was probably much more effective among industries that were well integrated into the elite structure, and network relationships were probably increased through mobilization. Second, in those equations that include both the concentration ratio and the capital intensity index, two of the independent variables share a common term—the value of capital. However, the common term is not considered problematic, since (1) the two variables have substantive meanings of their own, and (2) the common term is not highly correlated with the dependent variable (Fuguitt and Lieberman 1974).

TABLE 5
REGRESSION OF POLITICAL ACTIVITY, VALUE OF EXPORTS, AND
CAPITAL INTENSITY ON EXPORT-RELATED VESTING
AND GENERAL VESTING

DEPENDENT VARIABLE	INDEPENDENT VARIABLES		
	Political Activity	Value of Exports	Capital Intensity
Export-related vesting ($N = 44$) ($R^2 = .271$):			
b215	.599	...
SE.....	.111	.183	...
β258	.438	...
General vesting ($N = 30$) ($R^2 = .228$):			
b222908
SE.....	.850361
β175426

Table 6 presents regression equations that include one central variable from each of the determinants of power, with export-related and general vesting as dependent variables. The magnitude concept for export-related vesting included capital intensity. In both equations, the number of economic affiliations/person represented the network concept, while the concentration ratio was used as a surrogate for the ability to mobilize resources.

For export-related vesting the value of exports had the strongest impact, with a standardized regression coefficient of .54. The concentration ratio had a slightly weaker, but substantial, relationship with vesting, with a standardized regression coefficient of .46. Economic affiliations/person had a low, negative coefficient, $-.09$. This suggests, as stated earlier, that the State Department seemed to be taking the initiative in actions which benefited exporters. The total variance explained was an impressive 40%.

General vesting presented a different picture. The variables representing the three determinants were all correlated with each other. Capital intensity, economic affiliations/person, and the concentration ratio each correlated with one of the others at least .4.¹⁷ Such multicollinearity rendered the estimation of the separate effects of the independent variables imprecise. But it also meant that the separate effects of the variables were not as important as their common association. Capital intensive industries tended

TABLE 6
REGRESSION OF ECONOMIC AFFILIATIONS/PERSON, CONCENTRATION
RATIO, VALUE OF EXPORTS, AND CAPITAL INTENSITY ON
EXPORT-RELATED VESTING AND GENERAL VESTING

DEPENDENT VARIABLE	INDEPENDENT VARIABLES			
	Economic Affiliations/ Person	Concen- tration Ratio	Value of Exports	Capital Intensity
Export-related vesting ($N = 44$) ($R^2 = .398$):				
b	-.104	.022	.745	...
SE.....	.148	.006	.174	...
β	-.091	.463	.544	...
General vesting ($N = 30$) ($R^2 = .321$):				
b080	.028579
SE.....	.300	.014398
β048	.370273

¹⁷ On the other hand, the correlations among the independent variables in the equation for export-related vesting is:

	Value of Exports	Concentration Ratio
Concentration ratio45	...
Economic affiliations/person16	.24

to be highly concentrated and to be affiliated with other parts of the economy. The three variables together explained 32% of the variance of general vesting. Thus, the industry that had the best chance of vesting general interests in the State Department was highly concentrated, capital intensive, and well connected in the business elite. The railroad industry, which was not included in the computations, had these characteristics and also had the greatest vesting of all industries.

Since the railroads were not included in the statistical analysis a special word should be said about them. As American development progressed in the mid-19th century, nationally organized transportation and communication became strategic necessities. National, state, and local governments all willingly gave the railroads financial and legal backing. But their strategic position was enhanced by mobilization. Chandler (1965) has pointed out the many pioneering activities of the railroads, including modern lobbying, public relations, and even illicit means of political influence. By the mid-1880s they were more politically mobilized than any other economic interest group (Kolko 1963, 1965). The railroad magnates were also the first nationally organized capitalist elite. Other industries, as they became nationally organized, had to deal with the economic, social, and political structures developed by the railroad. The elite that the new industrialists aspired to join, the stock market they eventually became listed on, and the lobbying and public relationship techniques they adopted were all institutionalized by the railroad industry.

During the period under study, the railroad embodied all three determinants of power. Its magnitude dwarfed the rest of the economy, with average capitalization at \$5,712.27 million (investment book capital, U.S. Bureau of the Census 1975) compared with the other sampled industries' average of \$184.64 million (book value). The data on affiliations demonstrated the extent to which the railroad subsumed the national elite. It averaged 1,701 total affiliations over the period, while each of the other 11 industries averaged only 155 affiliations. Attempts at direct political influence by the railroads totaled 33 for the period, compared with an average of 10.4 for the other industries. The concentration ratio was quite low, only 12.5% compared with 45.2% for the other industries: it was not the case that a small number of railroad operating companies dominated the industry. There were, however, a handful of "communities of interest" that controlled the industry through financial control and interlocking directorates (U.S. Interstate Commerce Commission 1908; Campbell 1938). In terms of the vesting of interests in the State Department, the railroad industry was mentioned a total of 103 times over the period, compared with an average of 11.73 for the other industries.

These data support the notion that the railroad was an entrenched power rather than an ascendant one. Mobilization is a particular characteristic

of groups whose power is contingent (Tilly 1978). The degree of railroad mobilization activities exceeded that of other industries less than its degree of magnitude or network relations exceeded the others'. While railroad network relations consistently intensified over the first three quinquennia of the period, its mobilization activity actually declined.

CONCLUSION

This study set out with a very general question in mind: What characteristics of politically powerful actors differentiate them from less powerful actors? The social science literature on political power has been concerned primarily with the identification of who has power and how power is distributed across social groups. Especially in the corpus of literature contributing to the pluralist-elitist debate, theoretical questions of power have been clouded by ideological concerns of partisans on both sides. Thus recent political sociology has shifted emphasis away from pluralist-elitist debate toward issues of a more precise definition of political power, the nature of the state, and the consequences of state action on society.

Three determinants of political power were identified as characteristics that differentiate the powerful and the powerless—magnitude, network relations, and mobilization of resources. The aspect of power examined was the vesting of economic interests in the administration of American foreign policy.

The findings of the study have corroborated the analytical distinction among these three determinants of power. The determinants statistically explain a substantial proportion of the variation in vesting. By specifying the circumstances under which they are explanatory independent variables, their relationship to political power has been revealed. Perhaps the most important finding of the study is that magnitude, network relations, and degree of political mobilization had different political consequences for different activities of the same government agency. In particular, the factors explaining the vesting of interests in general matters differed distinctly from the factors explaining the vesting of economic interests in matters concerning American exports—a central topical issue of the era. For general matters, the vesting of economic interests was generally associated with mobilization, as suggested by political sociology of the mobilization perspective (Tilly 1978; Gamson 1973). However, mobilization was overshadowed in the explanation of export-related vesting by magnitude variables. Mobilization capability positively affected vesting, but not as strongly as did the value of exports. The industry's network relations had no effect on vesting.

Thus, the determinants of power had different effects under different conditions. Specifically, magnitude characteristics and inclusion in the elite

network did not lead to the vesting of all interests equally. Among magnitude variables, the value of exports was sufficient for the vesting of export-related interests, while capital intensity was the strongest determinant of general vesting. Mobilization had a slightly stronger effect on export vesting, while network relationships had little effect on either type of vesting. Historically these results are consistent with the findings of such historians as LaFeber (1963), who paint a picture of general consensus among political and economic leaders over the need for exports, in response to which exporters mobilized.

The lack of effect of network affiliations, while presenting problems for historians who depict the robber barons as a monolithic class, does not warrant rejection of network relations as a determinant of power. But it does open up the issue. The question is less whether inclusion in the elite is necessary to power than under which historical conditions it is critical, and under which conditions other factors become decisive. The history of the railroad industry suggests that during times of intense economic and political change, network relations may be less decisive than mobilization. The network perspective in general and power elitism in particular are oriented toward explaining stability rather than change. They seek to explain the processes by which a power elite can reproduce itself over time to the exclusion of other contenders. The period under examination was a time of intense economic and political change during which the elite underwent substantial flux in its own organization (Roy 1981).

Mobilization, alone among the three determinants of power treated here, had similar effects on export-related and general vesting of interests. This suggests that mobilization may bring a generalized payoff from government. The variable measured here was not mobilization oriented toward the State Department, but capacity for mobilization and general mobilization activity. Yet there was payoff from the State Department on all issues. Although the State Department made an implicit distinction between export-related matters and other matters vis-à-vis magnitude characteristics and inclusion in the elite, it made no such distinction vis-à-vis mobilization. Mobilized industries gained general as well as specific payoffs.

These results lead to the conclusion that it is too simplistic to say that magnitude, or elite membership, or mobilization determines political power. One must specify the conditions under which each pays off. To understand how historical circumstances underlie the effectiveness of various determinants requires a theory of the state. This is where conventional theories of power fall short. While adding useful information about the mechanics of political influence, power elitists, pluralists, and resource-management and interest-group researchers rarely specify the conditions under which the mechanics are ineffective or the different types of payoffs that can be gained by different types of mobilization. Discussions of magnitude, net-

work relations, and mobilization have been vague about the circumstances under which the government can be influenced and the circumstances under which the government might act autonomously.

The results here do not negate the importance of the concentration of authority roles within a small group of individuals or of the tight network structure within that group, as argued by the elitists. But they do raise questions about the circumstances under which those network relations achieve results for members of the elite. They do not preclude the broad distribution of decision making argued by the pluralists nor do they contradict the emphasis on political contention in the resource-management perspective. But they indicate that mobilization has less effect on the vesting of interests when the government takes the initiative on an issue.

The contingent nature of the effects of magnitude, network, and mobilization raises questions about the structure of the state itself. The dynamics of power are contingent upon bureaucratization, structures of authority and accountability, and the nature of state penetration into the social and economic realms. These issues must become a part of any comprehensive theory of political power. For example, some evidence suggests that bureaucratization has the effect of reducing the impact of network linkages (Roy 1981). It is clear from the analysis presented here that government initiative in stimulating American exports diminished the effect of network and mobilization while increasing the importance of magnitude.

The study was conceived in the context of prevailing theories of power—elitism, which relies most heavily on network analysis, and pluralism and the resource-management perspective, which emphasize the mobilization of resources. Magnitude characteristics were originally included because of their use in ad hoc explanations in historical analysis. The attempt was not so much to construct a synthesis of what were seen as ultimately irreconcilable theoretical frameworks as to test variables suggested by each framework by looking at these variables in relation to a measure of power that was conceptually and operationally distinct from the factor purported to explain power. The goal was to identify not the wielders of power but the differences between powerful and powerless actors with similar functional roles—in other words, to identify the determinants of political power. The clearest answer to the question of what characteristics differentiate politically powerful actors was "It depends." The context of power becomes paramount.

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Structural Blockage: A Cross-national Study of Economic Dependency, State Efficacy, and Underdevelopment¹

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This cross-national study of the effects of dependency and state efficacy on development supports the "structural blockage" argument of dependency theory. According to this argument, international economic dependency impedes development in the more advanced countries of the periphery of the world capitalist system. In support of this argument, we show that two forms of participation in the world economy, the export of a small number of commodities and the export of primary products, obstruct development in advanced peripheral countries. The argument that these forms of participation in trade pose internal, structural obstacles to development is supported by evidence showing that state-sponsored institutional transformations outweigh the development-stunting influence of these forms of participation. On the basis of these findings, we argue against the divergent-development thesis implied in some dependency arguments.

The writings of Marx on British imperialism seem contradictory from a neo-Marxist dependency perspective (Foster-Carter 1974). On the one hand, Marx argues that British imperialism stunted and distorted the economic development of Ireland; on the other hand, he argues that this same imperialism revolutionized Indian society and set it on a course convergent with Western societies (see Selsam, Goldway, and Martel 1970, pp. 135-45). Dependency theorists would argue that the effects of British imperialism were similar in both India and Ireland: to stunt and to distort development. Therefore, they would argue, Marx was simply wrong about India. The present economic condition of India seems to support this assessment of Marx, for India remains one of the poorest countries in the world.

The point of this paper is that Marx's writings on imperialism are not

¹ The authors are listed alphabetically to indicate equal contribution. We would like to thank an anonymous referee for the considerable energy expended in evaluating an earlier version of this paper and for the many valuable comments supplied.

contradictory. Indeed, the contrast between Ireland and India presented in his writings is entirely consistent with the strain of dependency theory supported by the statistical analyses we present below. We call this strain of dependency theory the "structural blockage argument." The primary thesis of this argument is that participation in the world economy impedes economic development only in the *advanced* periphery of the world economy. Participation in world trade is thought to facilitate development in poor peripheral countries by stimulating infrastructural improvements and by removing precapitalist obstacles to development. While initially beneficial, participation often induces forms of export specialization (e.g., the export of a few agricultural commodities) inimical to further development. Such specializations stunt the process of structural differentiation. Dependency-induced stagnation, according to this reasoning, should be most evident in advanced peripheral countries, not in countries that may still suffer from primeval underdevelopment. The contrast between Ireland and India in Marx's discussion of British imperialism is consistent with this perspective. Ireland suffered even in Marx's day from the structural blockages induced by trade with the British, while India had only begun to experience the "annihilation" of its "old Asiatic society" (Selsam et al. 1970, p. 139).

We would like to emphasize the fact that the structural blockage argument we support is one of several dependency arguments contained in many versions of dependency theory. The other major dependency arguments include class-centered, unequal exchange, and world market vulnerability arguments.² Though different authors attach different weights to these different mechanisms, dependency theorists typically weave the various arguments together in their writings on development. We show in this paper that there is considerable empirical support for the structural blockage argument. The pattern of findings we obtain suggests that structural blockage is perhaps the most fundamental mechanism of dependent underdevelopment.

Before we present our statistical analyses of dependency and development, we spell out the structural blockage argument and contrast it with the other dependency arguments. We argue that the structural blockage argument is the only dependency argument that (1) clearly specifies the relationship between level of development and the counterdevelopmental effects of dependency and (2) suggests that through political initiative underdeveloped countries can overcome trade-induced obstacles to development.

² At the risk of oversimplification, we have distilled these arguments from the dependency literature in order to further the empirical assessment of dependency theory. We welcome attempts to improve on our analysis of this literature or on our efforts to draw out the different empirical implications of the various dependency arguments.

THE STRUCTURAL BLOCKAGE ARGUMENT

Proponents of the structural blockage argument (Singer 1950; Hirschman 1958; Galtung 1971; Bettelheim 1972; Amin 1974; Ragin and Delacroix 1979) emphasize the influence of participation in world trade on the evolution of the internal social structures of poor countries. Some scholars who use this argument (e.g., Galtung) do not distinguish between class dynamics and other features of social structures problematically related to class phenomena. Thus, what we call the structural blockage argument needs to be clarified. Marx's formulation of the distinction between class structure and the larger societal framework is a good starting point for clarification.

In *Capital* Marx (1967, p. 359) credits the unprecedented development of the forces of production in Europe to the specifically capitalist innovation of the factory system. According to Marx, the "division of labor in the workshop," which constitutes the core of the factory system (see also Rueschemeyer 1977, pp. 3-4), accelerated the generation of surplus value and created conditions conducive to greater surplus extraction by capitalists. Capital accumulation and concentration, in turn, stimulated further industrial development and a diffusion of capitalist relations of production.

The unprecedented development of the forces of production in Europe was made possible, in part, by the prior existence of uniquely favorable societal structures. Marx (1967) contrasts European societies with "Asiatic societies" and argues that capitalist development in the former was facilitated by a greater development of the "societal division of labor." An important distinction between the societal division of labor and the workshop division of labor is germane to the structural blockage argument. The *workshop* division of labor involves the sequential division of production in one work site under one authority. The steps in the workshop division of labor are work stations (Marx 1967, pp. 342-45). The *societal* division of labor involves the movement of commodities (i.e., merchandise that changes hands commercially) from one workshop to another (e.g., from the farm to the factory). The movement of commodities in the societal division of labor entails ownership, authority, and spatial discontinuities. In contrast to the workshop division of labor, which is a specific contribution of capitalism, the societal division of labor is "common to all economic formations" (Marx 1967, p. 359).

A high societal division of labor promotes development through various mechanisms. (1) Each increase in the societal division of labor reacts back on itself, creating conditions for further expansion (Marx 1967, p. 383). The separation of the tannery from the cattle farm paves the way for the separation of the shoe factory from the tannery. (2) Each increase in the societal division of labor expands intermediary social roles, especially such

occupations as clerk and merchant (see Galtung 1971, pp. 85-90). (3) Increases in the societal division of labor stimulate the growth of such infrastructures as communication and transportation networks (Galtung 1971). (4) Increases in the societal division of labor create new discontinuities in ownership that indirectly enlarge governmental activities by providing taxation opportunities and by expanding the adjudicatory functions of the state. To the extent that state intervention in the economy is conducive to growth, increases in the societal division of labor accelerate economic development by strengthening and enlarging the state.

Thus, increases in the societal division of labor tend to be self-multiplying, and they stimulate an expansion of the general "internal structural differentiation of society" (Rueschemeyer 1977) outside the sphere of production proper. Any obstacle to the expansion of the societal division of labor, therefore, poses an obstacle to development because it may stunt the process of structural differentiation. Furthermore, obstacles to the expansion of the societal division of labor may coexist with a high workshop division of labor and with the establishment of genuine capitalist class relations.

What, then, is a structural blockage? A structural blockage exists when the expansion of the societal division of labor is impeded. Certain forms of participation in the world economy create such impediments in underdeveloped countries. These forms of specialization cut these countries off from the developmental benefits listed above because the expansion of the societal division of labor is blocked. In general, countries that specialize in the export of a small number of commodities or in the export of primary commodities have relatively bifurcated economies, with few organic bonds between export and nonexport sectors. This bifurcation in itself poses obstacles to the expansion of the societal division of labor, for it represents a relative curtailment of internal linkages. Also relevant to the structural blockage argument is the fact that in such countries there are relatively few ownership discontinuities in the export sector. The relative absence of such discontinuities, again, indicates that potential internal linkages have not been or cannot be realized. Even though the intrusion of world market forces into these countries stimulated the development of the workshop division of labor, export specializations have created conditions inimical to the expansion of the societal division of labor and thereby stunted the process of structural differentiation.

According to the structural blockage argument, underdeveloped countries participate in the world economy in order to escape their original poverty. They tend to participate in rough conformity with the principle of comparative advantage (see Ragin and Delacroix 1979). This participation improves their overall productivity and leads to modest developmental progress. Foreign investments further stimulate the development of the

forces of production (i.e., the workshop division of labor) and facilitate increased capital accumulation. Yet participation in the world economy limits the progress of peripheral countries by imposing on them forms of participation that interfere with the expansion of the societal division of labor. These forms of participation, represented by a specialization in the production for export of few, primary products, may block structural differentiation. Hence, instigation of capitalist development in the periphery by the core may be followed by a period of developmental stagnation of indefinite duration. Thus, even though peripheral countries are working parts of the world economy, capitalism does not bestow on them the extraordinary developmental advantages it conferred on Euro-America (Marx and Engels 1969) and Japan (Halliday 1975).

OTHER DEPENDENCY ARGUMENTS

The loose body of writing known as "dependency theory" is characterized by its discursive complexity. (For a brief but comprehensive summary see Von Eschen [1975]; see McGowan and Smith [1978] for an incisive critical review; the most succinct presentation by an empirically inclined scholar remains Galtung [1971, 1976].) Nevertheless, it is possible to discern within this literature three types of arguments, in addition to the structural blockage argument, that describe mechanisms that causally link participation in the world economy to underdevelopment. These are (1) world market vulnerability arguments, (2) class-centered arguments, and (3) unequal exchange arguments. We have distilled these arguments from the dependency literature. They are not strictly identified with particular authors. As noted above, dependency scholars tend to weave the various dependency arguments together, sometimes emphasizing one mechanism of dependent underdevelopment, sometimes another. For purposes of empirical inquiry, however, it is necessary, at the risk of oversimplification, to distinguish among the various mechanisms specific to each argument and to assess them separately.

The market vulnerability argument contends that (1) the world demand for the primary products of peripheral countries must undergo a relative decline, and (2) this downward trend is aggravated by international market fluctuations in the prices of primary products (see Galeano 1971; Furtado 1965; Gomez 1966; Maizel 1958; Magdoff 1969). The vagaries of the international market are thought to destabilize the foreign exchange earnings of peripheral countries and to impair their capacity for public and private planning. The weight of the empirical evidence now available, however, supports neither the price-fluctuation argument (Coppock 1962; MacBean 1966; Erb and Schiavo-Campo 1969; Naya 1973; Askari and

Weil 1974) nor the idea that there is a downward trend in the demand for primary products.

There are two main versions of the class-centered argument. In one version, proponents argue that core capitalists are able to appropriate the surplus generated through the exploitation of the labor and the extraction of natural resources of peripheral countries because of the complicity (Bourricaud 1966; Petras 1970, pp. 23–32; Galtung 1971; Mandel 1975, p. 56; Amin 1974, 1976) or ineptitude (Frank 1972; Berman 1974; Mamdani 1976) of the elites of these countries. Thus complicity or ineptitude of elites is held to explain the failure of normal (i.e., Western) capitalist development.³

In the other version of the class-centered argument, Wallerstein (1974*a*, pp. 346–57) arrives at a similar destination via a slightly different route. He explains the failure of capitalist development in the periphery by arguing that different class structures have an unequal potential for generating creative (i.e., “progressive” in the Marxist sense) class tensions. He traces different contemporary class structures back to the different modes of labor control associated with functional roles in a historically determined world division of labor. According to this scheme, the early assignment of some regions of the world to the production of primary export commodities led to the local domination of these regions by nonentrepreneurial, parasitic elites. Underdevelopment in this perspective is a historically induced, structural condition of the world capitalist economy.⁴

The two main proponents of the unequal exchange argument are Mandel (1975) and Emmanuel (1972, 1974).⁵ The two mechanisms emphasized by these authors are largely economic and structural; one concerns the exchange of commodities between the periphery and the core; the other, the export of capital from the core to the periphery. Mandel (1975, p. 53) argues, “The exchange of commodities produced in conditions of a higher

³ Frank (1972, p. 136), e.g., argues, “The special interests created by the dependence of the Latin American bourgeoisie on the metropolis have obliged the sector of the bourgeoisie that once favored bourgeoisie nationalism to . . . join an alliance for the progress of imperialism . . . and by this alliance to deepen still further dependence, dependent development and underdevelopment.” Petras (1970, p. 24) states, “Given their preponderant position in the new urban industrial complex, it is highly unlikely that the economic elites would seek to break down barriers to economic development. The new industrial elites are not autonomous but rather dependent in varying degrees on external economic forces.”

⁴ Wallerstein (1974*b*, pp. 391–92) argues, in agreement with Frank, that contemporary Latin American countries are underdeveloped because of the early assignment of these areas to raw material production for the capitalist world economy. This particular mode of participation led to the local domination of this region by nonfeudal, capitalist, but antidevelopment elites.

⁵ Below we emphasize the writings of Mandel because he attempts to integrate the unequal exchange argument into a more encompassing dependency perspective.

productivity of labour against commodities produced in conditions of a lower productivity of labour [is] an unequal one; it is an exchange of less against more labour, which inevitably led to a drain, an outward flow of value and capital from . . . [peripheral] countries to the advantage of Western Europe." In this perspective, trade between the core and the periphery is a direct mechanism of underdevelopment since it involves the exchange of commodities containing unequal quantities of labor. According to Mandel, the export of surplus capital from the core to the periphery intensifies the drain of surplus value. This is because the export of capital distorts the development of peripheral countries toward complementarity with core economies. "This meant especially that they had to concentrate on the production of raw materials" (Mandel 1975, p. 57). This further intensifies unequal exchange because raw materials are often labor intensive.

The unequal exchange argument, because it is based on the labor theory of value, seems to indicate that the counterdevelopmental effects of trade should be greatest in the least developed countries. Drain should be greatest, according to this reasoning, where value added per worker is least (i.e., in the most underdeveloped countries). However, unequal exchange arguments, particularly those of Mandel, are buttressed with a "combined and uneven development" perspective (as formulated, e.g., by Amin [1976]; see Trimberger [1979]). According to this argument, precapitalist relations of production are subordinated by the world economy and are replaced by various forms of partially capitalist or semicapitalist relations of production. This process, and the process of unequal exchange in general, is stimulated and intensified by core investments in the periphery. The unequal exchange argument, in all its subtleties, therefore, affirms that the more extensive a peripheral country's participation in the world economy, the greater its de-development. Nevertheless, the argument is compatible with both a periphery-wide de-development (following the labor theory of value) and stagnation specific to the advanced periphery (following combined- and uneven-development arguments).

COMPARISON OF THE DEPENDENCY ARGUMENTS

Because of the complexity and interconnectedness of the various dependency arguments discussed above, it is difficult to specify precisely the various patterns of empirical findings that would support each argument. Nevertheless, it is important to delineate such differences because of the vastly different political and policy implications of each argument. Below, we contrast three of the four dependency arguments with respect to their predictions concerning the effects of investment dependence, trade dependence, and form of participation in the world economy on economic development. (We do not discuss the market vulnerability argument because

the available empirical evidence does not support it.) We pay special attention to the differences among these arguments that are relevant to the task of specifying the variable effects of dependency on economic development for countries at different levels of development (especially for poor and advanced peripheral countries). We also contrast the different implications of these arguments concerning the possibility of successful prodevelopmental state action in peripheral countries.

Of the three dependency arguments discussed above, only the structural blockage argument explicitly posits a negative effect, specific to the advanced periphery, of form of participation in the world economy on development. According to this argument, trade and foreign investment promote development in the most underdeveloped countries. At the same time these forces also promote forms of export specialization that become obstacles to development once the benefits from the introduction of the workshop division of labor have been realized. Thus, measures of form of participation such as primary product specialization and commodity concentration should have a negative effect on development for advanced peripheral countries.

The class-centered argument suggests that investment dependence, trade dependence, and form of participation in the world economy (which, according to this argument, indicates position in the international division of labor) should have periphery-wide negative effects on development because each of these different manifestations of dependency indicates the probable existence of class structures inimical to development. Such class structures, according to this argument, are necessarily associated with dependency; there is no reason to expect them to appear only in advanced peripheral countries. Indeed, a pattern specific to the advanced periphery would contradict the long-term historical perspective that informs class arguments.⁶ According to both Frank and Wallerstein, world systems remain in gestation for several centuries. The entire non-European world has been incorporated into the present world economy at least since the turn of the century (Chirot 1977). The class structures that now exist in the periphery cannot be pristine; they are integral features of the contemporary world system. This is not to say that class-centered arguments are necessarily *incompatible* with an advanced-periphery-specific effect of dependency on development. We argue simply that there is nothing inherent in class-centered arguments that would suggest such a pattern.⁷

⁶ If anything, the class-centered argument would predict a *poor*-periphery-specific effect of dependency on development. Wallerstein (1974b, p. 405), e.g., argues that semi-peripheral countries (roughly comparable to our advanced peripheral group) are both exploiters and exploited.

⁷ One could argue, e.g., that inappropriate class structures become most inappropriate when industrialization becomes crucial for further development. In general, however,

Unequal exchange arguments are somewhat compatible with an advanced-periphery-specific effect of dependency on development. However, these arguments posit that *trade dependence* (as indicated, e.g., by the intensity of countries' involvement in trade) is the principal mechanism of dependent underdevelopment.⁸ If a drain to the detriment of the periphery results from the differential quantities of labor embodied in the commodities it trades with the core, the magnitude of the drain should be proportional to the magnitude of trade. According to the labor theory of value, this trade-based drain should operate to the detriment of all peripheral countries. In Mandel's version of the unequal exchange argument, however, core investments are thought to intensify trade-based drains, in part because such investments stimulate primary product specialization and commodity concentration. To the extent that the core invests preferentially in the advanced periphery, the harmful effects of trade dependence may be greatest in advanced peripheral countries. Additionally, because investment dependence and certain forms of participation in the world economy are associated with this intensified drain of value, they, too, may be associated with inferior economic development in the advanced periphery according to the unequal exchange argument. Note, however, the primacy of trade dependence relative to other types of dependency. The unequal exchange argument is supported only if a periphery-wide or advanced-periphery-specific negative effect of trade dependence on development can be demonstrated. If this effect can be demonstrated, other possible dependency effects can be interpreted in the light of the unequal exchange mechanism.

Another important point of disagreement among these three dependency arguments concerns the efficacy of deliberate, prodevelopmental political action in peripheral countries. An important theoretical and policy implication of the structural blockage argument is that dependency-induced underdevelopment must be responsive to state-sponsored corrective measures, especially if those measures are directed at expanding the societal division of labor. If blocks to development are largely internal (regardless of their ultimate origins), they may be removed or compensated for by internal means. Hence, the structural blockage argument views the stagnation of much of the periphery as more amenable to indigenous state-

class-centered arguments emphasize the general antidevelopment orientation of periphery elites regardless of the presence or absence of industry or the presence or absence of the opportunity to industrialize.

⁸ Note that Mandel (1975, p. 66) argues that unequal exchange has "nothing to do with the material nature of the commodities which these nations produce—whether they be raw materials or finished goods. . . ." Differences in the amount of labor embodied in exported products do not correspond, in the present capitalist world economy, to the nature of commodities. Note also that Mandel argues that the process of unequal exchange through trade has intensified in late capitalism (see also Emmanuel 1972, pp. 264–65).

instigated intervention than do the other dependency arguments. The class-centered argument offers no remedy for the dependency-created feebleness (Mandel 1975; Mamdani 1976) or ineptness (Frank 1972) of elites in the periphery. Nor does it indicate how ineffective state structures or otherwise inappropriate polities owed to dependency might be improved or replaced (Wallerstein 1974*b*).⁹ Some class-centered arguments suggest that nothing short of the wholesale dissolution of the world capitalist economy will bring relief to the periphery (Wallerstein 1974*b*). Finally, the policy prescriptions of the unequal exchange argument seem to rest with the improbable adoption of an autarkic posture by peripheral countries.¹⁰

CONCEPTUALIZATION AND MEASUREMENT: DEVELOPMENT,
DEPENDENCY, AND STATE EFFICACY

Development

The comparability of studies inspired by dependency theory is hampered by both conceptual and measurement confusion regarding development. On the conceptual side one may distinguish two positions. (1) Development may be viewed in strictly economic terms. This conceptualization warrants the use of indicators of development based on national products. (2) Development may also be viewed as a more diffuse concept indicated by a variety of social and political, or *relational*, measures (Seers 1972). Telephones per 1,000 population is a good example of a relational indicator. Telephone networks approximate a material manifestation of the Durkheimian concept of "moral density" (Durkheim 1964, pp. 256-82). (See Finsterbusch [1973] on the relative substitutability of economic and relational measures.)

On the measurement side of the matter, one finds a large number of difficulties pertaining to data quality. One central difficulty is the lack of con-

⁹ Wallerstein (1974*b*, p. 403) states, "One cannot reasonably explain the strength of various state machineries at specific moments of the history of the modern world-system . . . [except] . . . in terms of the structural role a country plays in the world-economy at that moment in time. To be sure, the initial eligibility for a particular role is often decided by an accidental edge. . . . But once this relatively minor accident is given, it is the operations of the world market forces which accentuate the differences, institutionalize them, and make them impossible to surmount in the short run."

¹⁰ Mandel (1975, p. 376) argues that socialist revolutions in the peripheral countries will provide the proper context for "liberation from the capitalist world market" and the eventual construction of a socialist world economy. Emmanuel (1972, p. 269) admits that "a sudden smashing of the existing structure" of international exchange would entail "losses for the world as a whole." He recommends that poor countries violate the classical and neoclassical economics principle of comparative advantage and produce domestically the more capital intensive goods they obtain at present through international trade. He also argues that a system of international redistribution of value should be established (Emmanuel 1972, p. 270).

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fidence in the most available and most used indicators of development, measures based on national products. Critics point out that the gathering of national-product data in poor countries is subject to a great deal of enumerative error. A variety of critiques suggest that national-product-based measures such as GNP per capita overestimate the development gap (Dommen and Monaldi 1976). The use of those measures, however, is favored by the World Bank (see Chenery and Syrquin 1975) and defended by classical scholars (Kuznets 1964, 1968), neo-Marxist scholars (Emmanuel 1974), and scholars in sympathy with the classical Marxist tradition (Bairoch 1976).

One critic of national-product-based measures, Barbera (1973), argues that telephones per 1,000 population is less biased than GNP per capita. He argues that telephone installations are easily and carefully counted by business concerns with a vested interest in accuracy. In order to palliate some of the enumerative difficulties associated with national-product-based measures, we duplicate every equation in which GNP per capita is the dependent variable with one that substitutes telephones per 1,000 population for GNP per capita.¹¹ Telephones per 1,000 population appears to possess desirable characteristics from the standpoint of conceptual and metric critiques of GNP per capita; it also has relational content and a higher degree of enumerative accuracy.

† In the following statistical analyses of dependency and development, we use a lagged dependent variable design that allows an (implicit) examination of change in level of development. Specifically, we regress each indicator of development measured in 1975, GNP per capita (GNPC75) and telephones per 1,000 population (PHON75), on its 1960 values (GNPC60 and PHON60) to control statistically for the stability of these indicators over time.

Dependency

The specification of dependency measures in existing studies is constrained by few empirical or theoretical considerations. On the one hand, factor analyses of dependency measures fail to reveal a unidimensional dependency construct (McGowan and Smith 1978, p. 215). On the other hand, however, these studies are not guided by theoretical concerns: most fail to lay out the bare logic of the various dependency arguments.

¹¹ This measure is especially relevant to the structural blockage argument because it is an infrastructural measure. If structural blockages do occur, infrastructures should show the effects of such blockages. Generally, in our analyses of development we interpret only those effects that are duplicated in equations with GNP per capita and telephones per 1,000 population. This conservative strategy increases our confidence in our results.

The most frequently used measures of dependency are measures of foreign investment. (For a serious review of cross-national studies utilizing such measures see Bornschier, Chase-Dunn, and Rubinson [1978].) These appear to be favored because they capture simultaneously the notions of external control and market dependence which can often be detected side by side in dependency theory. Trade-based measures are used nearly as frequently as investment measures with even less theoretical or empirical justification. Even a reduced set of measures based strictly on trade forms only a mediocre unidimensional structure in factor analysis (Kaufman, Chernotsky, and Geller 1975; McGowan and Smith 1978, p. 215). (See McGowan and Smith [1978] for a good review of empirical studies using trade measures.) Some political scientists (e.g., Vengroff 1975; see also Snyder and Kick 1979) construe dependency in political terms and operationalize it accordingly. This approach, however, seems to betray the logic of economic primacy inherent in dependency theory (Terry 1972, p. 97). In fact, political dependency measures have low correlations with investment- and trade-based measures (see Snyder and Kick 1978).

For our purposes it is useful to distinguish two aspects of dependency conceived as mode of participation in the world economy: the *magnitude* and the *form* of participation.

Measures of the magnitude of participation, such as the ratio of a country's foreign trade to its GNP (trade dependence) and the ratio of a country's stock of foreign investment to its GNP (investment dependence), indicate the volume of a country's involvement in the world economy relative to the size of its economy. A high score on either of these measures often indicates the consequences of some form of imperialism, especially as conceived by neo-Marxist dependency theorists (and by Lenin [1917]). At a more abstract level, these measures indicate the degree of control that external actors and events may exert on a country's economic fate. In this study we use both measures of magnitude of participation in the world economy—stock of direct private foreign investment in 1967 (by Development Assistance Council countries) divided by GNP (INVS67) and value of foreign trade in 1960 divided by GNP (TRAD60)—in our statistical analysis of the relationship between dependency and development.¹²

Measures of form of participation, such as degree of specialization in the

¹² Bornschier et al. (1978) argue that an appropriate measure of core capital penetration is the stock of direct, private foreign investment divided by the square root of the product of energy consumption and population. They also argue, however, that the measure we use—the stock of direct, private foreign investment divided by GNP—can be substituted for their more complex measure without significantly altering the results of analyses of the effects of this kind of dependency on development. While we do not necessarily concur with this assessment of the substitutability of the two measures, we prefer the measure we have used because it seems less arbitrary and more interpretable than their more elaborate measure.

export of primary products and degree of concentration of export commodities, indicate the structural positions of countries in the international division of labor. Primary product specialization and commodity concentration are two forms of participation which have been shown to have negative effects on development (see, e.g., Ragin and Delacroix 1979). We use both of these forms in the statistical analysis below. Our measures of form of participation are the ratio of the value of primary product exports to total exports (PRIM60), measured circa 1960, and the ratio of the value of the five most important export commodities to total exports (commodity concentration, COMC60), also measured circa 1960.¹³

State Efficacy

Few studies consider the possible mediating effects of political initiative and state characteristics on the relationship between dependency and development (Tugwell 1974; Delacroix 1977). Delacroix and Ragin (1978) show that some economically dependent states have the ability to reduce the counterdevelopmental effects of core cultural influence (a superstructural form of dependency). Robinson (1977), in an attempt to specify empirically one of the mechanisms of dependent underdevelopment, shows that dependency reduces government revenue, which, in turn, is positively related to development. To the extent that government revenue indicates state strength, this argument suggests that dependency weakens the capacity of states to pursue or implement policies conducive to development.¹⁴

The scant attention paid to the state by students of development is surprising given the influence it is accorded by theoretically oriented students of past (Anderson 1974; Supple 1976; Hartwell 1976, p. 379; Wallerstein 1974a) and contemporary development (Huntington 1968; Myrdal 1970; Horowitz 1972; Chirot 1977) and by the openly statist ideologies of many Third World leaders (see, e.g., Isaacman 1978, p. 6). One possible explanation of the relative neglect of the state in empirical, cross-national studies of underdevelopment is that it is difficult to conceptualize and operationalize "state efficacy." It is especially difficult to operationalize the concept in a manner that is not ideologically tainted.

¹³ We would like to underscore the fact that measures of form of participation in the world economy are also measures of position in the international division of labor, as conceived, e.g., by Wallerstein (1974a). To weight these measures by magnitude of participation (e.g., as in value of primary product exports/GNP) would be to measure something quite different from position in the international division of labor. In any event, for a significant subset of periphery countries the value of primary product exports/GNP is virtually perfectly correlated with trade/GNP.

¹⁴ We argue below that direct taxation is much more relevant than aggregate government revenues to questions concerning the effects of dependency and state action on development.

We begin the difficult task of conceptualization by distinguishing two aspects of state efficacy, viability and activism. We argue that the effective state must possess both of these characteristics. States that are only weakly established over the territories they control (i.e., states that are not viable) are unlikely to see the fruits of their activism, for they will have difficulty obtaining the necessary cooperation of their subject populations. Conversely, a viable state must pursue policies that are conducive to development (i.e., it must be active); viability is not by itself conducive to development, for viability is sometimes achieved through passivity (i.e., allowing internal, dependency-induced obstacles to development to persist).

An examination of the history of state intervention in the development of Western Europe suggests several kinds of state activism that are likely to further development.¹⁵ Supple (1976, pp. 325-40) argues, with others, that state economic leadership finds its most useful employment in (1) policies designed to substitute public for private resources whenever the expected benefits of an investment are "external economies" from the standpoint of the private capitalist or otherwise unavailable to him (most such investments are in infrastructure rather than in direct production; see Supple [1976], pp. 303-4), and (2) the eradication of precapitalist economic and social institutions that place constraints on overall productivity and their replacement by modern institutions (Supple 1976, pp. 302-3).

The construction of railroad networks in 19th-century Europe is a good example of the first kind of fruitful state activism. The initial outlays required appeared prohibitive to investors and the payoff seemed distant (Supple 1976, pp. 326-27). Yet the benefits that railroads brought to developing European countries were substantial (Weber 1961, pp. 219-21; Lenin 1917, p. 97; Mandel 1975, p. 51; Supple 1976, pp. 328-29; Kuznets 1966, pp. 24-25). The creation of Belgium's rail network, for example, undertaken by the young Belgian state and completed within a very short time, contributed greatly to the impressive economic success of this small, moderately endowed, and newly independent country (Supple 1976, p. 327).

The second kind of fruitful state activism mentioned above involves institutional transformation. Institutional transformation includes the uprooting of traditional structural restraints on economic activity (e.g., guilds and castes), the attenuation of customary redistributive mechanisms, the reform of suboptimal systems of land tenure, sometimes the elimination of internal tariffs (Supple 1976, pp. 308-9, 318), and often the eradication

¹⁵ The example of neo-European countries such as the United States, Canada, Australia, and New Zealand is of little significance because one could argue that these countries had enormous natural advantages (e.g., a low population/resources ratio) not duplicated in most of the periphery.

of nepotism and other bureaucratically nonrational forms of administration (Myrdal 1970, pp. 237–40). Institutional transformation also requires the creation of new institutions with economic relevance. This typically involves the standardization of law and the legislation of standards (e.g., with respect to weights and measures but also taxes) and the effective enforcement of both.

The success of this transformation seems to depend to a large extent on the replacement of old institutional complexes with new networks more subject to direct state manipulation. To implement this replacement, it is essential to rechannel individual loyalties away from the old communities of kin, locality, and ethnicity and toward the nation-state. The most parsimonious strategy to this effect is to create a new, universal social role superseding all others and supported by resources directly at the command of the state. We hypothesize, with Bendix (1977) and others (e.g., Huntington 1968; Swanson 1971), that the concept of "citizenship" forms the foundation of this role and undergirds the modern institutional complex (see also Eisenstadt and Rokkan 1973).

It is difficult, of course, to tap diverse kinds of state activism with a single indicator. In the analyses below we use a composite index of state activism based on three highly correlated indicators of state-sponsored infrastructural and institutional transformation: value of direct taxation per capita, the ratio of secondary school enrollment to the secondary school age population, and value of public investment per capita. The value of public investment per capita is directly relevant to the matter of infrastructural improvement; a large proportion of all public investment in most countries is specifically for this purpose. Most other public investments are at least indirectly relevant to infrastructural development. The secondary school enrollment ratio is relevant to institutional transformation. Socialization into the citizenship role takes place in the school (Meyer and Robinson 1975, pp. 134–62; Meyer and Rowan 1978; Ramirez and Robinson 1979). A high ratio of students in schools, relative to the appropriate age group, indicates that the state assumes an activist stance toward the creation of this role. Value of direct taxation per capita indicates (1) the ability of a state to direct and redirect national resources and to control economic behavior indirectly through devices such as tax incentives and disincentives, and (2) the success of the state in penetrating social and economic life, in general.

The correlations between these three indicators of state activism for peripheral countries are .687 (public investments per capita with secondary school enrollment ratio), .755 (secondary enrollment ratio with public investment per capita), and .866 (direct taxation per capita with public investment per capita). The composite scores of countries on the index formed by these three indicators were computed by first standardizing the

three indicators and then summing for each country the three *z*-scores. In our analysis we label the state activism index ACTV65. The data used to construct this index were collected for the years 1960–67.

The viability of a state is much more difficult to assess than its activism. States that are only weakly established and regimes that must fight for survival are in a poor position to implement activist policies or to translate such policies into actual developmental progress. Yet it is difficult to determine when, in fact, a state is firmly established. (The Shah's government in Iran, for example, appeared firmly established to U.S. State Department officials only a few months prior to its fall.) Rather than attempt a formal delineation of the characteristics of viable states, we approach this problem from the perspective of apparent or *de facto* viability. Specifically, we argue that viable states exist in countries that (1) experienced no more than one coup d'état over the period of observation (1960–75) and (2) suffered neither significant native-based guerilla activity nor amputation of territory over this same period.¹⁶ Viable states are indicated in Appendix A. In our statistical analyses we code viable states (VIALE) 1 and others 0.

Peripheral states most capable of overcoming the counterdevelopmental effects of dependency should show both a high degree of viability and an activist stance—that is, they should be effective states. Conceptually, this means that an activist posture will probably bear little fruit if not implemented by a viable state, while an inactive but viable state will fail to counter dependency-induced obstacles to development. Both viability and activism must be present.

DESIGN

The design of this study is similar in form to recent quantitative cross-national analyses (e.g., Bornschier et al. 1978). We use a panel design in the analyses of the effects of dependency on development because good longitudinal data are available. These analyses are based on OLS regression estimates. We compared these estimates with WLS estimates using a technique outlined by Glejser (1969) and found no important differences. Contrary to past practice (Ragin and Delacroix 1978), in some of our analyses we estimate the effect of each measure of dependency on each indicator of development in separate equations.¹⁷ In Appendix B we report

¹⁶ Obviously, these criteria incorporate a large authoritarian component. However, authoritarianism is not enough to place a state in this category. E.g., Duvalier's Haiti and Amin's Uganda are excluded.

¹⁷ We use this strategy to allow each of the measures of dependency a maximum opportunity to explain variation in development. Also, because (1) the dependency measures have different patterns of missing data and (2) we use list-wise deletion of missing data, this method preserves degrees of freedom.

data sources and measurement procedures for all measures of dependency, development, and state efficacy included in our statistical analyses.

The analyses reported below are based on data on 72 peripheral countries considered over the 1960–75 period. We define as belonging to the periphery any country whose GNP per capita was less than \$1,000 in 1960. This set of 72 countries includes virtually all peripheral countries in existence in 1960.¹⁸ Nevertheless, countries with less than 1 million population in 1960 have been excluded because of the extreme values they register on variables of theoretical interest. This exclusion conforms with the practice of Chenery and Syrquin (1975, p. 102) and Bornschier et al. (1978). We have also excluded two oil-exporting countries, Saudi Arabia and Libya,¹⁹ whose recent history is in dramatic contradiction with the predictions of dependency theory. The sample is subdivided into an advanced peripheral group with GNP per capita between \$250 and \$1,000 in 1960 and a poor peripheral group with GNP per capita of less than \$250 in 1960 (see Appendix A).²⁰

The division of our sample into advanced and poor peripheral countries facilitates our examination of competing models of mechanisms connecting dependency and underdevelopment. As noted above, the class-centered argument supports periphery-wide negative effects of all forms of dependency on development; the unequal exchange argument supports both periphery-wide and advanced-periphery-specific effects of, especially, trade dependence on development; while the structural blockage argument supports poor-periphery-specific, positive effects of investment and trade dependence on development and advanced-periphery-specific negative effects of form of participation in the world economy (or position in the international division of labor) on development. To test these different conceptualizations of the mechanisms that connect dependency and underdevelopment, we use a technique for testing metric slope differences outlined by Draper and Smith (1966) and by Hanushek and Jackson (1977, pp. 127–29). This technique is in some ways statistically similar to performing

¹⁸ In the analyses of the effects of state efficacy on development and in the analyses of the effects of dependency on the components of state efficacy, countries with centrally planned economies are not included because they lack data on one or more of our measures.

¹⁹ The exclusion of these two countries is not theoretically justified. However, the task of explaining the substantially different results we obtain when these two cases are included could not be undertaken within the scope of this paper.

²⁰ These cutoff points were selected on the basis of an examination of the relationship between GNP per capita in 1960 and change in GNP per capita over the 1960–75 period. Countries with less than \$250 GNP per capita in 1960 generally experienced, at best, modest increments over this period, while countries between \$250 and \$1,000 GNP per capita in 1960 experienced increments of varying size, from small to very large. Countries with more than \$1,000 GNP per capita in 1960 experienced uniformly large increments in GNP per capita.

separate analyses for groups of countries (i.e., splitting the sample) as practiced by Chenery and Syrquin (1975, pp. 65, 107-8) and Ragin and Delacroix (1978). In addition, it permits the direct examination and comparison of metric slope estimates pertaining to different groups.²¹

To test the hypothesis that the effect of dependency (D) on development (G_2 for GNP per capita at time 2) varies by subpopulation (here poor and advanced peripheral countries), net of the effect of some other independent variable (G_1 for GNP per capita at time 1), the original equation (B 's indicate unstandardized regression coefficients),

$$G_2 = B_0 + B_1G_1 + B_2D + e, \quad (1)$$

must be augmented by a dichotomous variable (A for advanced periphery countries) and a variable indicating what Hanushek and Jackson (1977, p. 129) call the "dummy slope," A multiplied by D (or AD). The dichotomous variable A is coded 1 if a country is a member of the advanced peripheral group and 0 if it is a member of the poor peripheral group. The resulting equation is

$$G_2 = B_0 + B_1G_1 + B_2D + B_3A + B_4AD + e. \quad (2)$$

In this equation the estimate of the effect of dependency on development for poor peripheral countries is B_2 ; for advanced peripheral countries it is $(B_2 + B_4)$. The estimate of the intercept for poor peripheral countries is B_0 ; for advanced peripheral countries it is $(B_0 + B_3)$. If the variance in G_2 explained by equation (2) is significantly greater than that explained by equation (1), the argument that there is a uniform periphery-wide effect of dependency on development must be rejected. The proper estimates of the effects of dependency on development for poor and advanced peripheral countries are provided instead by equation (2).

This technique is also used in the analysis of the effects of state efficacy on development. We argue that state actions aimed at promoting development and counteracting internal, dependency-induced obstacles to development will be successful only if carried out by viable states. This means that in the regression of development on the components of state efficacy, viability (VIALE) and activism (ACTV65), it is necessary to include a third, multiplicative term, $VIALE \times ACTV65$, indicating the copresence of viability and activism. (In the analysis we label this term EFCACY for state efficacy.) Because VIALE is a dichotomously coded variable, the interactive term EFCACY actually indicates a dummy slope of the state activism variable. In other words, the variable EFCACY tests the argument that the magnitude of the effect of ACTV65 on development is greater for

²¹ Testing for slope differences is superior to splitting the sample because it allows the researcher to specify which relationships vary by subsample. To split the sample would be to allow *all* relationships to vary by subsample.

countries with viable states than it is for countries lacking viable states. It is important to keep in mind that only the structural blockage perspective explicitly argues that political initiative and state characteristics counteract dependency-induced underdevelopment.

ANALYSIS OF DEPENDENCY AND DEVELOPMENT

We proceed directly to a test of the group specificity of the effects of our measures of dependency—trade dependence (TRAD60), investment dependence (INVS67), primary product specialization (PRIM60), and commodity concentration (COMC60)—on our two measures of development, GNP per capita (GNPC75) and telephones per 1,000 population (PHON75).²² To adjudicate between the different arguments linking dependency and underdevelopment discussed above, it is necessary to test for the differential effects of the several measures of dependency on development for peripheral countries at different levels of development. We operationalize levels of development within the periphery by distinguishing between poor and advanced peripheral countries as defined above. This procedure, also used by Chenery and Syrquin (1975, pp. 65, 107–8), is advocated by McGowan and Smith (1978, p. 233).

In the statistical analyses below we compute the dependency-development equations in the stepwise manner described in the design section. First we compute a periphery-wide equation, and then we test for group-specific slopes. In table 1, which summarizes the results of these analyses, we report the equation modeling group-specific effects only if the increment to the explained variance is statistically significant; otherwise, we report in the column for poor countries the results of the equation which describes the common regression estimates for the two peripheral groups.

The estimates of the effects of trade dependence (TRAD60) reported in table 1 are uniformly weak and nonsignificant. No slope differences between poor and advanced groups are revealed except for an unexpected

²² In an earlier version of this paper we reported the results of an analysis of the differences in the effects of dependency on development for core countries and all peripheral countries. In that analysis we tested for the differences between core and peripheral countries with respect to the magnitude of their slope differences in equations similar to those reported in table 1. This examination was motivated by an interest in the thesis, shared by virtually all dependency arguments, that the effects of magnitude and form of participation in world trade on development are not the same for core and peripheral countries. Our analysis confirmed this thesis. We found that (1) core countries do not suffer developmentally from specialization in the export of a few commodities or from specialization in the export of primary commodities and do benefit from their overall intensity of participation in trade, (2) peripheral countries suffer from such specializations even though they are not harmed by the intensity of their participation in trade, and (3) both core and peripheral countries suffer developmentally if they are highly penetrated by foreign capital.

TABLE 1

PANEL REGRESSION OF MEASURES OF DEVELOPMENT ON MEASURES OF DEPENDENCY: POOR AND ADVANCED PERIPHERAL COUNTRIES

DEPENDENT VARIABLES	INDEPENDENT VARIABLES										R ²	N
	Constant	GNPC-60	PHON-60	Ad- vanced TRAD-60	Poor TRAD60	Advanced PRIM60	Poor PRIM60	Advanced COMC60	Poor COMC60	Ad- vanced INVS-67	Poor INVS67	Advanced Dummy
GNPC75:												
b.....	901	N.R.	N.S.	N.R.
B.....	-96.372	4.437	...	N.R.	(-148.079)	N.R.
GNPC75:												
b.....	712	-730	N.S.783
B.....	54.333	3.494	-1965.140	(-78.591)	1661.236
GNPC75:												
b.....	728	-851	N.S.905
B.....	74.295	3.573	-2373.571	(-126.157)	1920.243
GNPC75:												
b.....	903	N.R.	-123	N.R.
B.....	-45.621	4.473	N.R.	-656.036	N.R.
PHON75:												
b.....	744	N.S.	N.S.
B.....	4.261	2.743	85.542	...	(-11.669)	(-21.870)
PHON75:												
b.....	589	-804	N.S.952
B.....	-2.081	2.214	-173.059	(-121)	161.573
PHON75:												
b.....	592	-837	N.S.	1.007
B.....	-3.897	2.225	-186.930	(7.071)	170.996
PHON75:												
b.....	804	N.R.	N.S.	N.R.
B.....	6.135	2.973	N.R.	(-23.446)	N.R.

Note.—Significance level = .10; N.S. means not significant; N.R. means not relevant; nonsignificant coefficients are reported in parentheses; slopes reported for the advanced peripheral countries show arithmetic difference between poor and advanced peripheral countries; b = standardized, B = unstandardized coefficients; GNPC75 = GNP per capita in 1975; PHON75 = phones per 1,000 population in 1975; GNPC60 = GNP per capita in 1960; PHON60 = phones per 1,000 population in 1960; TRAD60 = trade/GNP in 1960; PRIM60 = primary product specialization in 1960; COMC60 = commodity concentration in 1960; INVS67 = stock of foreign capital/GNP in 1967. Columns headed "poor" present the main effect; those headed "advanced" present the interaction effect. The coefficients showing the effects of dependency on development should be interpreted as follows. The coefficient for Poor-PRIM60 shows the estimate of the slope for the relationship between primary product specialization and development for poor peripheral countries. This coefficient in the GNP per capita equation is -78.591. The advanced peripheral group estimate of this same relationship can be obtained by adding the coefficient reported for the poor group (-78.591) to that reported in the Advanced-PRIM60 column (-1965.140). The slope for the advanced periphery, thus, is -2043.731.

positive advanced-periphery-specific effect of TRAD60 on PHON75. This positive effect of trade dependence on one of our measures of development contradicts all three of the dependency arguments discussed above. The fact that the pattern is not duplicated in the GNPC75, however, suggests that this finding should not be taken too seriously. Generally, we interpret only those findings that are significant in both GNPC75 and PHON75 equations. The failure of trade dependence to have any negative effect on either measure of development disallows even a qualified acceptance of the unequal exchange argument.

Investment dependence (INVS67) has a periphery-wide, significant negative effect on GNPC75 and a *nonsignificant* negative effect on PHON75.²³ The inconsistency between the findings for the two measures of development, again, dictates that we interpret this finding cautiously. The periphery-wide negative effect of investment dependence on GNPC75 is most consistent with the class-centered argument. This argument emphasizes the complicity of elites in the periphery relative to core appropriation of surplus value produced in the periphery. The finding is not consistent with the structural blockage argument. According to the structural blockage argument, core investments should promote development in the poor periphery.

The findings relative to primary product specialization (PRIM60) and commodity concentration (COMC60), however, support the structural blockage argument. For the poor peripheral group the estimates of the effects of PRIM60 and COMC60 on GNPC75 and PHON75 are approximately zero and nonsignificant. For the advanced group, however, these two measures have very strong, negative, significant effects on both indicators of development. Note that the negative effects of these variables on PHON75 constitute additional indirect evidence in support of the structural blockage argument since telephone networks are viewed by some scholars (e.g., Barbera 1973) as a fundamental infrastructure of highly differentiated societies. Overall these findings reinforce the structural blockage argument. The fact that primary product specialization and commodity concentration do not interfere with the development of poor peripheral countries confirms the argument that dependency blocks only the development of advanced peripheral countries.

One possibility not addressed in table 1 is that, net of the effects of pri-

²³ Bornschier et al. (1978) report an advanced-periphery-specific negative effect of investment dependence (foreign capital penetration) on development. The difference between our finding and theirs could be due to (1) the fact that we use slightly different forms of the same measure (see n. 12 above), (2) the fact that we use different cutoff points, or (3) the fact that we use a panel design while they use a cross-sectional design with a growth rate for their dependent variable. In any event, they do not attempt to address the relevance of their advanced-periphery-specific finding to the matter of adjudicating among the different dependency mechanisms.

mary product specialization and commodity concentration, core capital penetration may have no effect on GNP per capita. We examine this speculation in table 2. The equations reported in this table show that either measure of form of participation in the world economy annuls the negative effect of INVS67 on GNPC75 observed in table 1. This provides further support for the structural blockage model of development, for it demonstrates that the finding that supports directly one of the other dependency arguments is spurious. The measure INVS67 has a negative effect on GNPC75 only because of its association with COMC60 and PRIM60.²⁴

ANALYSIS OF DEPENDENCY, DEVELOPMENT, AND STATE EFFICACY

Because it places the structural mechanism of underdevelopment within peripheral countries, the structural blockage argument implies a greater scope for autonomous, ontogenetic corrective measures. A better specified model of development, according to this argument, would include variables that gauge the effectiveness of the state in combating dependency-induced underdevelopment. Before we present the analyses of the effects of state efficacy on development, we must first consider the effects of dependency on state efficacy. If state efficacy is possible only when dependency is absent, it is useless to argue that dependency-induced underdevelopment can be remedied through state efficacy. We examine the relationship between dependency and the components of state efficacy in tables 3 and 4.

In the first four equations in table 3 we show the separate effects of each dependency measure on our dichotomous measure of viability (VIALE). We control for development as indicated by GNP per capita in 1960. Two types of dependency have significant negative effects on viability, PRIM60 and COMC60. However, the betas estimating these effects are of modest size ($-.23$ and $-.26$, respectively). Because the highest explained variance associated with these equations is only $.10$, it is clear that most of the variation in state viability results from phenomena other than dependency or level of development. (These equations differ little when GNP per capita is removed from each.) The real sources of variation in viability are, of course, outside the scope of this paper. This conclusion is supported when the dependency measures are allowed a maximum opportunity to explain variation in viability. This is achieved by entering all dependency mea-

²⁴ The findings reported in table 2 suggest that the conclusions of Bornschier et al. (1978) are incomplete. If they had included measures of primary product specialization and commodity concentration in their analysis, they probably would have found results similar to ours. In any event, our findings suggest that investment dependence is counterdevelopmental only because it is associated with primary product specialization and commodity concentration. This finding is consistent with the structural blockage argument. According to this argument, investment dependence promotes export specializations inimical to development.

TABLE 2
PANEL REGRESSION OF GNP PER CAPITA ON INVESTMENT DEPENDENCE, PRIMARY PRODUCT SPECIALIZATION, AND
COMMODITY CONCENTRATION: POOR AND ADVANCED PERIPHERAL COUNTRIES

DEPENDENT VARIABLE	INDEPENDENT VARIABLES							R ²	N
	Constant	GNPC60	INVS67	Advanced PRIM60	Poor PRIM60	Advanced COMC60	Poor COMC60		
GNPC75:									
b.....759	N.S.	-.509	N.S.532	65
B.....	201.937	3.714	(-266.777)	-1368.786	(-233.223)	1102.588	
GNPC75:									
b.....778	N.S.	-.600	N.S.	.636	65
B.....	46.015	3.808	(-249.276)	-1680.431	(-92.686)	1318.452	

NOTE.—See note to table 1.

TABLE 3
REGRESSION OF STATE VIABILITY ON GNP PER CAPITA AND
MEASURES OF DEPENDENCY: PERIPHERAL COUNTRIES

DEPENDENT VARIABLE*	INDEPENDENT VARIABLES						R ²	N
	Constant	GNPC60	TRAD60	PRIM60	COMC60	INVS67		
VIABLE:								
b.....235	N.S.057	73
B.....	.295	.0005	(.081)		
VIABLE:								
b.....	...	N.S.	...	-.235084	71
B.....	.931	(.0002)	...	-.589		
VIABLE:								
b.....	...	N.S.	-.262101	71
B.....	1.043	(.0002)	-.794	...		
VIABLE:								
b.....284	N.S.	.087	72
B.....	.348	.0007	(-.189)		
VIABLE:								
b.....	...	N.S.	N.S.	N.S.	N.S.	N.S.	.142	71
B.....	1.020	(.00002)	(.184)	(-.444)	(-.387)	(-.129)		

NOTE.—See note to table 1.

* VIABLE = dichotomously coded measure of state viability.

TABLE 4
REGRESSION OF STATE ACTIVISM ON GNP PER CAPITA AND MEASURES
OF DEPENDENCY: PERIPHERAL COUNTRIES

DEPENDENT VARIABLE*	INDEPENDENT VARIABLES						
	Constant	GNPC60	TRAD60	PRIM60	COMC60	INVS67	R ² N
ACTV65:							
b.....863	N.S.	
B.....	-2.440	.010	(-.962)751 44
ACTV65:							
b.....687	...	-.309	
B.....	.542	.008	...	-3.375811 42
ACTV65:							
b.....801	-.143	...	
B.....	-1.115	.009	-2.010763 43
ACTV65:							
b.....858	N.S.	
B.....	-2.613	.010	(-1.192)	.757 46
ACTV65:							
b.....674	N.S.	-.302	N.S.	N.S.	
B.....	1.215	.008	(-.588)	-3.292	(-.434)	(-.797)	.823 42

NOTE.—See note to table 1.

* ACTV65 = index of state activism based on secondary school enrollment ratio, public investment per capita, and direct taxation per capita.

tures simultaneously in the equation. With this procedure the relevant proportion of explained variance is .142.

The analyses of the effects of dependency on our index of state activism (ACTV65) show a somewhat similar pattern (see table 4). Again, PRIM60 and COMC60 have modest, significant negative effects. However, in these equations the proportions of explained variance are high, and the effect of GNPC60 is strong and significant in each equation. Thus, even though these equations show that state activism is only modestly constrained by dependency, they show also that state activism is seriously constrained by level of development.

This pattern of results is, in part, a consequence of the cross-sectional design used in table 4. With such a design it is possible to address only questions concerning the kinds of countries that have active states. A panel design would be more appropriate, making it possible to address questions concerning the *expansion* of state activism. Unfortunately, longitudinal data on two of the three components in our state activism index are not available. Good longitudinal data exist only for secondary school enrollment ratio.

We analyze these longitudinal data with a panel design in table 5. We regress secondary school enrollment ratio in 1970 (SECR70) on the same ratio in 1955 (SECR55), on GNP per capita in 1960, and on the dependency measures examined in tables 3 and 4. The equations in table 5 show that none of the dependency measures has a significant negative effect on the expansion of secondary school enrollments and that GNPC60 has only a modest effect. Thus, secondary enrollment expansion is unconstrained by dependency and only slightly constrained by level of development. This one component of state activism, of course, correlates very highly with the other two components and with the index formed by the three indicators. To the extent that longitudinal variations in the other two components of state activism, public investment per capita and direct taxation per capita, correlate with longitudinal variations in secondary enrollment ratio, the results presented in table 5 for secondary enrollment ratio are generalizable to the state activism index. We turn now to an examination of the effects of dependency and state efficacy on development.

We argue that according to the structural blockage argument the beneficial effects of state efficacy on development may outweigh the harmful effects of dependency since both mechanisms influence the process of structural differentiation via the societal division of labor. Strong support for the structural blockage argument can be obtained by demonstrating that state efficacy cancels the negative effects of dependency shown above in tables 1 and 2. Table 6 shows the results of this test. We regress the two indicators of development on commodity concentration and primary product specialization (preserving the group-specific structure demonstrated in

TABLE 5
PANEL REGRESSION OF SECONDARY SCHOOL ENROLLMENT RATIO ON GNP PER CAPITA
AND MEASURES OF DEPENDENCY: PERIPHERAL COUNTRIES

DEPENDENT VARIABLE*	INDEPENDENT VARIABLES							R ²	N
	Constant	SECR55†	GNPC60	TRAD60	PRIM60	COMC60	INVS67		
SECR70:									
b.....686	.277	N.S.789	75
B.....	7.155	1.009	.027	(-1.511)		
SECR70:									
b.....703	.239	...	N.S.827	67
B.....	11.572	1.102	.024	...	(-5.755)		
SECR70:									
b.....691	.238	N.S.831	67
B.....	15.928	1.084	.024	(-11.736)	...		
SECR70:									
b.....725	.258	N.S.	.823	75
B.....	6.835	1.127	.025	(-4.557)		
SECR70:									
b.....670	.222	N.S.	N.S.	N.S.	N.S.	.831	63
B.....	21.792	1.042	.022	(-2.401)	(-5.160)	(-10.504)	(-1.649)		

NOTE.—See note to table 1.

* SECR70 = secondary school enrollment ratio in 1970.

† SECR55 = secondary school enrollment ratio in 1955.

TABLE 6
PANEL REGRESSION OF MEASURES OF DEVELOPMENT ON MEASURES OF DEPENDENCY AND STATE EFFICACY:
POOR AND ADVANCED PERIPHERAL COUNTRIES

DEPENDENT VARIABLES	INDEPENDENT VARIABLES										R ²	N
	Constant	GNPC60	PHON60	Advanced PRIM60	Poor PRIM60	Advanced COMC60	Poor COMC60	VIABLE	ACTV65	EFCACY*	Advanced Dummy	
GNPC75:												
b.....394	...	N.S.	N.S.183	N.S.	.391	N.S.	40
B.....	-73.691	2.063	...	(-884.903)	(254.866)	415.320	(15.719)	195.605	(826.172)	
GNPC75:												
b.....352	N.S.	N.S.	.168	N.S.	.374	N.S.	40
B.....	-139.354	1.842	(-1220.754)	(468.165)	381.498	(44.149)	186.981	(1060.756)	
PHON75:												
b.....585	N.S.	N.S.189	N.S.	.412	.690	40
B.....	-31.311	...	2.222	(-130.731)	(14.126)	35.730	(-13.303)	17.077	130.098	
PHON75:												
b.....542	N.S.	N.S.	.203	N.S.	.472	N.S.	40
B.....	-36.788	...	2.061	(-56.337)	(32.081)	38.238	(-8.723)	19.598	(65.192)	

NOTE.—See notes to tables 1, 3, and 4.

* EFCACY = multiplicative state efficacy measure formed from VIABLE and ACTV65.

table 1), on the two component terms of state efficacy, viability (VIALE) and activism (ACTV65), and on the state efficacy interaction term (EFCACY).²⁵

In each of these four equations the state variables reduce to nonsignificance the previously observed negative effects of dependency on aggregate development.²⁶ State efficacy (EFCACY) has a strong positive and significant effect on both indicators of development, GNP per capita and telephones per 1,000 population. We argue that this pattern obtains because the process of structural differentiation is more powerfully affected by state actions than by dependency. The strong positive effect of state efficacy (EFCACY) supports our argument that states combining viability and activism are most capable of overcoming dependency-induced obstacles to underdevelopment (i.e., they are most capable of promoting internal structural differentiation). The nonsignificant coefficients attached to ACTV65 show that unless activism is combined with viability, developmental progress will not result.

We should note that our measure of state efficacy is correlated with the measures of form of participation in trade ($-.64$ with primary product specialization and $-.53$ with commodity concentration) and that, as shown above, these two dependency measures have modest, negative *cross-sectional* effects on the two components of the state efficacy measure. We do not deny that the more dependent countries, to a limited extent, tend to be governed by less effective states (see tables 3 and 4). However, the one piece of longitudinal evidence available to us (reported in table 5) suggests that dependency poses no obstacle to the *expansion* of state activism, one of the essential components of state efficacy. Clearly, more research needs to be done on the sources of longitudinal variation in state activism and state viability. Only then will we be able to specify the precise nature of the relationship between dependency (especially, form of participation in the world economy) and state efficacy. For the present, we can conclude simply that there is a weak cross-sectional correspondence between these two but no evidence of a longitudinal relationship.

Whatever the precise nature of the longitudinal relationship, our finding that, net of the effect of state efficacy, dependency has no effect on development stands. According to the structural blockage argument, this pat-

²⁵ In the analyses below of the effects of dependency and state efficacy on development, we exclude consideration of the two measures of magnitude of participation, TRAD60 and INVS67. The former is excluded because it has been shown here to have no negative effect on development, the latter because its negative effect on GNP per capita (shown in table 1) is spurious, as shown in table 2.

²⁶ In the analyses below of the effects of dependency and state efficacy on development, state activism index. However, the limited number of cases included in these equations reproduce, almost perfectly, the coefficients reported in table 1 when the state relevant variables are removed from the equations reported in table 6.

tern obtains because the effect of state efficacy on the process of structural differentiation (via the societal division of labor) is much stronger than the effect of dependency.

DISCUSSION

We introduced this paper with a brief discussion of Marx's apparently inconsistent analysis of British imperialism and then argued that his analysis is consistent with the structural blockage argument of contemporary dependency theory. We then elaborated the structural blockage mechanism, using *Capital* to buttress contemporary discussions, and contrasted the structural blockage argument with other dependency arguments. By separating and simplifying the different dependency arguments, we were able to set up a rough adjudication between them. This adjudication involved an examination of the effects of different kinds of dependency for countries at different levels of development (poor and advanced countries) and an examination of the effects of state efficacy on development, net of dependency effects. Overall, our findings support the structural blockage argument over class-centered and unequal exchange arguments.

It is important to emphasize the fact that our results provide only incomplete support for the structural blockage argument. According to this argument, trade and foreign investment should promote development in the poor periphery. Our analysis shows, however, that trade dependence (trade/GNP) and investment dependence (stock of foreign investment/GNP) have no significant effect, positive or negative, on development for poor peripheral countries.²⁷ There are several possible sources of our failure to find the predicted pattern. (1) To the extent that measurement error has biased our measures (Long 1980), mechanical problems associated with the use of ratio variables may have obscured a positive relationship between investment and trade dependence and development for poor peripheral countries. Note that the numerator in one of our measures of development (GNP per capita) appears in the denominator of both measures of magnitude of participation in the world economy (trade/GNP and stock of foreign investments/GNP). If serious measurement error does exist, a possible positive correlation for poor countries may have been biased toward zero. (2) A related technical problem concerns the fact that our measures of development, GNP per capita and phones per 1,000 population, show the least relative variation over time in the poor periphery. These measures may be too crude to capture important differences among poor peripheral countries. (3) It is possible that the sorts of structural blockages that exist

²⁷ This assessment is based on the results reported in table 2, where we showed that, net of primary product specialization and commodity concentration, investment dependence has no significant effect on development.

in advanced peripheral countries already have formed in poor peripheral countries involved in the export of primary commodities or in the export of a small number of commodities. If this is the case, the positive effects of trade and investment dependence may be canceled by the negative effects of primary product specialization and commodity concentration to the extent that these different kinds of dependency coincide in the poor periphery. (4) Our model does not control for the capricious distribution of mineral and other valuable natural resources over the surface of the earth. McGowan and Smith (1978) have shown that with a crude control for economic potential (a resource index), the net effects of investment and trade dependence on development are positive. Their results are based on a sample of tropical African countries (countries that are members of our poor-periphery category).

Nevertheless, we do show that *negative* effects of dependency on development are discernible only in the advanced periphery and that form of participation in trade is more important than magnitude of participation. The implied temporal placement of the counterdevelopmental consequences of dependency (i.e., their advanced-periphery specificity) argues against the divergent development thesis implied in some dependency arguments. According to the structural blockage argument, development along a single path is blocked by certain forms of specialization in the world economy. State action directed specifically at counteracting these structural blockages is the key to development. Neither autarky nor a wholesale dissolution of the world economy is a prerequisite for development of the periphery.

The structural blockage argument has undeniable political and policy implications. It is consistent with the experience of many peripheral countries. An Algerian minister of industry and energy has recently declared, for example, that "the real solution to the development problem lies in the capacity of each of the developing countries to mobilize its own resources and energies" (quoted in Barraclough 1978, p. 58). This assessment mirrors the conclusion reached by some students of development: "There is now widespread agreement that the central development problems of most developing countries are largely internal" (Diaz-Alejandro et al. 1978, p. 156). This does not mean that the problems of peripheral countries were not caused by participation in the world economy. The structural blockage argument simply points to certain broad solutions whose historical precedents are often ignored.

Contemporary core countries, with the exception of Great Britain and possibly the Netherlands, endured periods of high commodity concentration and primary product specialization. (E.g., as late as 1932 raw cotton accounted for 22% of the value of all U.S. exports.) In the European experience, according to many economic historians, the later a country began industrialization (which involved a shift away from the export of a small

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APPENDIX B

MEASURES AND SOURCES

Code Name	Definition	Sources
GNPC75.....	Gross national product per capita in 1975	World Bank 1970-76
GNPC60.....	Gross national product per capita in 1960	International Bank for Reconstruction and Development 1971
PHON75.....	Telephones per 1,000 population in 1975	American Telephone and Telegraph 1976
PHON60.....	Telephones per 1,000 population in 1960	Taylor and Hudson 1971
TRAD60.....	Value of imports plus value of exports divided by gross national product	World Bank 1976
PRIM60.....	Value of exports in Standard International Trade Categories 0-4 (or equivalent) divided by value of total exports, 1955-60	United Nations 1953-69
COMC60.....	Value of five most important exports divided by value of total exports	United Nations 1953-69
INVS67.....	Stock of private foreign direct investment by DAC countries divided by gross national product, 1967	Ballmer-Cao, Thanh-Huyen, and Scheidegger 1979
SECR55.....	Secondary school enrollment as a percentage of secondary school age population in 1955	Taylor and Hudson 1971
SECR70.....	Secondary school enrollment as a percentage of secondary school age population in 1970	Taylor and Hudson 1971
VIALE.....	Dichotomously coded variable indicating countries with "viable" governments over the 1960-75 period	Elliot 1974 and Kitchen 1976
ACTV65.....	State-activism index composed of (1) secondary school enrollment ratio in 1960; (2) direct taxation per capita, ca. 1960-67; and (3) public investment per capita, ca. 1960-67	(1) World Bank 1970-76, (2) Ballmer-Cao et al. 1979, (3) Ballmer-Cao et al. 1979

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Racial Insurgency, the State, and Welfare Expansion: Local and National Level Evidence from the Postwar United States¹

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This paper addresses systematically the possible nexus between insurgent political action and the state apparatus, concentrating specifically on the relationship between urban riots and welfare or relief-giving activity in the postwar United States. The theoretical warrant for the analysis has its genesis in Piven and Cloward's influential thesis relating insurgency and relief giving in capitalist society. This perspective is juxtaposed with the orthodox developmental perspective of welfare institutions, and the causal processes and underlying images of the state are compared. A critical review of the empirical work on the riot-welfare relationship suggests several deficiencies and questions which we attempt to redress and address, respectively, in a cross-level empirical analysis of (a) changes in welfare expenditures from 1960 to 1970 in a panel of U.S. cities and (b) annual changes in national aggregate relief-program categories for the postwar United States (1947-76). The results of the city-level analysis, parallel to several similar studies, provide extremely weak to no support for the hypothesized riot-welfare relationship. However, the postwar time-series analysis provides consistently strong evidence that the urban riots played an important role nationally in the short-term expansion of the extent of relief giving across several major program categories. The conclusion considers the implications of the findings for theories of (a) welfare institutions in late capitalism, (b) the state in late capitalism, and (c) collective action and insurgency.

¹ This article is a revised and expanded version of a paper we presented at the 74th annual American Sociological Association meetings in Boston, August 1979. It also reports portions of the first author's unpublished Ph.D. dissertation (Department of Sociology, Indiana University, 1979) and portions of the second author's unpublished M.A. thesis (Department of Sociology, Indiana University, 1977). For comments (some of which we were unable or unwilling to incorporate) on these and previous versions of this paper, we wish to thank Paul Burstein, Joe Feagin, Larry Griffin, David Knoke, Anthony Orum, Charles Ragin, David Snyder, George Stolnitz, Alan Wolfe, participants in the Florida State University Political Economy Reading Group (especially Ted Chiricos, Jim Fendrich, Kathy Kemp, Larry Polivka), and the anonymous referees. Special thanks to David Snyder for directing both theses from which this paper stems. We are also grateful to Tom Dye and the Policy Sciences Program (Florida

Black insurgency² in the United States was certainly one of the most significant insurgent movements of the postwar era—perhaps the most significant. Beginning with the early activities of the Civil Rights Movement in the 1950s, racial insurgency (marches, sit-ins, boycotts, protest demonstrations, riots, etc.) escalated increasingly during the 1960s and early 1970s, as blacks became the initiators instead of the targets of collective violence (Janowitz 1969). These disturbances, experienced directly by many American cities and indirectly by others, eventually elicited substantial concern on the part of both local and federal governments (see National Advisory Commission on Civil Disorders 1968; Governor's Select Commission on Civil Disorder 1968; Governor's Commission on the Los Angeles Riots 1965). Not surprisingly, however, nearly all the attention of policymakers was directed toward explanations of why the riots occurred and how they might have been prevented (Masotti, n.d.; Feagin and Hahn 1973; Lipsky and Olson 1977).

Only in the recent aftermath of the turbulent sixties have some researchers begun to redirect attention to other equally important questions concerning the forces producing the "wind-down" of the movement (e.g., Oberschall 1978) and the consequences of urban racial insurgency in particular. Much of this recent research has addressed the issue of governmental responses to racial riots specifically in the form of public income support expenditures or expansion in relief rolls (e.g., Piven and Cloward 1971, 1977; Betz 1974; Welch 1975; Kelly 1977; Button 1978; Albritton 1979; Isaac 1979; Jennings 1979).

With few exceptions, this accumulating body of literature has failed to explicate clearly the theoretical perspectives and assumptions which underlie most analyses. This is unfortunate, for findings are typically left suspended without much theoretical foundation or immediately clear implications. Our position is that the empirical study of the consequences of collective violence and insurgency should, at a minimum, be motivated and informed by general theoretical arguments pertaining to the particular outcome of interest (e.g., welfare expansion, income distribution, etc.) and also by theory of collective violence. Findings should have implications relevant to both bodies of literature.

Moreover, most previous studies contain serious methodological flaws.

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² By insurgency we mean attempts, organized in varying degrees, to affect structural change by thrusting new interests into the political arena (see also Jenkins and Perrow 1977). But more than this, insurgency is protest politics (or social protest) which attempts to organize and represent interests outside institutional mechanisms (e.g., electoral or interest-group politics).

In general, problems derive from inadequate measures of insurgency (e.g., exclusive reliance on one form or dimension of activity), the particular choice of analytical unit typically employed (i.e., cities), the neglect of time or the dynamics of such processes, and failure to take into consideration and control for other variables which may influence the state expenditure process. The latter flaw is probably the result of neglecting alternative theoretical arguments on "public" expenditures in general and welfare programs in particular.

In this paper, we attempt to redress these problems and go beyond previous research on the relationship between racial violence and welfare. First, we argue that, despite variation in specific hypotheses, two major perspectives purport to explain welfare-institution behavior in advanced capitalist societies. Usually unrecognized or left implicit, they are referred to here as the orthodox or developmental/modernization perspective and the political conflict/struggle perspective (Isaac 1979). Put simply, these arguments consider welfare or relief expansion as, respectively, (1) a response by a class-neutral state to the needs and dislocations inherent in an expanding industrial society, or (2) a response by a capitalist (class-dominated) state to a threat produced by insurgency from below, whether massive political protest or disorder. Second, since some of the previous analyses bear on our design, we also subsequently provide an overview and critique of the several relevant empirical studies which focus specifically on the riot-welfare nexus. Third, a major part of the paper reports two separate but related investigations bearing on the developmental and conflict perspectives of welfare institutions. In brief, these empirical analyses revolve around the causal role of central determinants of each theoretical perspective in producing (a) changes in welfare expenditures from 1960 to 1970 in a panel of U.S. cities and (b) year-to-year changes in national aggregate welfare (relief) program categories for the postwar United States. We direct our attention and our interpretation of these results to theoretical issues which (1) relate directly to the causal arguments embedded in the developmental/modernization and political conflict/struggle perspectives of welfare expansion, (2) bear indirectly on the underlying assumptions concerning the nature of the state in these theoretical perspectives, and (3) add to the recently emerging body of empirical work linking forms of insurgency and collective violence to objective outcomes or consequences for groups employing these strategies or for social systems more generally.

THEORIES OF WELFARE EXPANSION

Numerous specific arguments or hypotheses exist concerning the growth in welfare spending (variously assessed as social service, public expenditures,

etc.).³ Some of these hypotheses have been reviewed and conceptually categorized in a variety of ways by others (e.g., cf. Tarschys 1975; Hage and Hanneman 1977; Klein 1977; Cameron 1978). Although the major causal variables and processes specifically hypothesized may vary considerably, when the implicit models of the state are exposed, it is plausible to argue that there are two basic approaches—a developmental/modernization and a political conflict/struggle perspective⁴ (Isaac 1979).

The Developmental/Modernization Model

Causal processes.—The orthodox explanation for the growth in state or public expenditures focuses primarily on aspects of socioeconomic development (e.g., output or income per head, urbanization and industrialization). Such developmental/modernization factors are prominent in cross-national analyses of expenditures (e.g., Jackman 1975; Wilensky 1975), in national time-series analyses (e.g., Hage and Hanneman 1977; Heisler and Peters 1978; Peters and Klingman 1978), and in analyses of more specific types of expenditures within subnational units (e.g., states: see Dye 1966; Winegarden 1973; Grønbjerg 1977).

Probably the earliest version of this perspective was presented by Wagner ([1883] 1958) as a general "law of expanding state activity." Simply put, Wagner's "law" states that public expenditures will tend to rise more than proportionately to national income or product. This expectation implies, as elaborated by some recent writers (e.g., Musgrave 1969), that the societal demand for state services (and hence willingness to accept increased tax burdens) is income elastic, and therefore will increase with economic development and general affluence.⁵

The more recent vintages of the argument generalize similar notions in

³ Many of these hypotheses have been tested empirically on a variety of analytical units (e.g., cities, states, countries) both cross-sectionally and over time. While much of the writing concerns the theory of public expenditures generally (e.g., Musgrave 1969; Tarschys 1975; Klein 1977; Peters and Klingman 1978), other studies have examined more specific expenditure categories—for example, cross-national social insurance program experience (Cutright 1965; Jackman 1975) and Aid to Families with Dependent Children (AFDC) in U.S. states (Piven and Cloward 1971; Winegarden 1973; Grønbjerg 1977). In general, many of the specific hypotheses have been applied as explanations across a variety of state expenditure categories.

⁴ Some specific hypotheses are difficult to place unambiguously within either of these two general perspectives. For example, Peacock and Wiseman (1961) propose what could be termed a "displacement thesis" which emphasizes the impact of large-scale crises (such as war) in inducing infrequent but typically large changes in the general level of taxation (and hence spending) tolerated by the public; taxes and spending are raised drastically during a crisis, lowered somewhat after it, but never returned to the precrisis level.

⁵ For further discussion of Wagner's "law," see Bird (1970, 1971), Gupta (1968), Musgrave (1969), Tarschys (1975), and Cameron (1978).

terms of equilibrium concepts of supply and demand. In equilibrium terms, state expenditures represent a "direct system response" to newly emerging needs of industrial society (demand)—frictional unemployment, urbanization, an aging population, etc.—which are, in turn, recognized and responded to as national income or product (supply) increases owing to economic development (e.g., see Hage and Hanneman 1977). The growth of discretionary resources, which may either "trickle down" or be "consciously directed" on the basis of perceived needs accompanying industrialization (e.g., the decline in kinship-based support or security in old age), is the main underlying theme. A variety of cross-national studies (e.g., Cutright 1965; Galenson 1968; Pryor 1968; Jackman 1975; Wilensky 1975) have argued and elaborated on one or another version of this view and presented varying degrees of empirical support.⁶

A prime example of this line of argument is Wilensky's (1975) recent study of the effect of economic development level on social security expenditure effort. He posits that two major intervening variables "explain" this effect: one is demographic, the age composition of the population; the other is essentially bureaucratic (specifically, the age of the social security system). Wilensky (p. 47) summarizes his argument in the following words:

Over the long pull, economic level is the root cause of welfare-state development, but its effects are felt chiefly through demographic changes of the past century and the momentum of the programs themselves, once established. With modernization, birth rates declined, and the proportion of aged was thereby increased. This increased importance of the aged, coupled with the declining economic value of children, in turn exerted pressure for welfare spending. Once the programs were established they matured, everywhere moving toward wider coverage and higher benefits. Social security growth begins as a natural accompaniment of economic growth and its demographic outcomes; it is hastened by the interplay of political elite perceptions, mass pressures, and welfare bureaucracies.

Wilensky goes on to argue that this process is independent of differences among industrialized countries in political ideology or political structure. In other words, common to advanced industrial societies are pressures which produce levels of development regarding social security (and other public expenditures) which make them appear to converge on this dimension despite other political or economic distinctions.

There are, of course, other variations of this line of thought. Focusing on Aid to Families with Dependent Children (AFDC) expenditures across the United States, Grønbjerg (1977) argues that there are two primary com-

⁶ However, others (Bird 1971; Musgrave 1969; Gupta 1968) have presented evidence that such a positive economic development effect does not exist for more homogeneous wealthy nations. See also Wildavsky (1975) for a theoretical argument predicting that the rate of economic growth should be inversely related to public expenditure changes.

peting explanations: (1) the stratification thesis, which argues that welfare growth is predominantly a function of poverty and dislocation (i.e., need or demand), and (2) the mass society thesis (Grønbjerg's explanation), which emphasizes sociopolitical change or the extension of citizenship and social rights under parliamentary democracy. From our point of view, these are not fundamentally distinct arguments. Instead, they are specific hypotheses within the same general orthodox perspective. The first represents an economic development hypothesis (consistent with the presentation above, e.g., Wilensky [1975]), while the second is a political development thesis. The latter can be viewed as one mechanism intervening between economic modernization and the expansion of public expenditures. Both are, however, development/modernization arguments which make very similar tacit assumptions about the political economy—particularly the nature and role of the state.

The image of the state.—A central but largely ignored component of any theory of state expenditures is the model or view of the state which it assumes (Isaac 1979). The model of the state embedded in developmental explanations of the state-expenditure process is predominantly implicit.

The developmental/modernization perspective of state expenditures embodies what has been variously referred to as a liberal (e.g., see Gordon's [1977] comparative statements) or "class-mediation" view of the state (see Sweezy 1970). This approach views the state in modern democratic capitalism as a near perfect reflection of aggregated individual preferences as expressed through interest representation in the polity. As such, the state is typically seen as responding rather automatically to societal needs and dislocations. In general, most elements of the pluralist model of the polity and social structure are implied (Isaac 1979). The state, in other words, is justified in acting because it is sensitive to preferences of societal members in general and because it seeks to promote the general interest of all—"the public interest." While class and other conflicts admittedly arise, the state is seen as the logical and necessary institution (or set of institutions) to resolve such conflicts; it is, in this sense, seen as a class-neutral arbiter (see Sweezy [1970] for a critique and comparison with a Marxist view). Developing primarily in the post-Great Depression, Keynesian world, this view sees the activist state implementing the functions of redistribution, stabilization, and allocation in accordance with societal preferences (e.g., cf. the optimistic arguments in Heller [1977] and Okun [1977]).

In summary, the process of economic development leads to other social structural changes (e.g., demographic, bureaucratic, economic) which can produce new needs among various adversely affected segments of the population (e.g., the poor, the aged, the disabled, the unemployed). The state, in turn, recognizes these as problems and responds for two important reasons: (1) societal capacity to respond has increased through increased eco-

nomic growth, and (2) there is a more or less built-in propensity for the state to further the public interest more generally.

This perspective, based on a pluralistic image of the state and polity, sees the central political process revolving around participation in electoral politics. It is through this mechanism that the state comes to aggregate and reflect individual preferences adequately. Therefore, the assumptions of a class-neutral state, a pluralist (open) polity, and the inextricably linked assumption (or characterization) of insurgents as irrational, emotional, and non-goal-oriented lead to the exclusion of insurgency or collective violence as an important form of political action (Isaac 1979).⁷ The orthodox perspective, then, serves as a baseline against which to consider the primary focus of this paper—the impact of racial insurgency on welfare institutions in the postwar United States. The second major theoretical perspective—the political conflict model due largely to Piven and Cloward (1971)—emphasizes this latter process.

The Political Conflict/Struggle Model

Causal processes.—While a large number of studies beginning with the orthodox (developmental) theory of state expenditures have focused on the role of such theoretically important factors as economic development, economic and political system characteristics, and urbanization, more recent efforts by some sociologists and radical economists have looked elsewhere for the primary forces. These explanations locate the basic processes producing state expenditures (in general, and welfare expansion in particular) in the class and institutional structures of advanced capitalist societies. There are several major variants to this line of thought. However, integral to all such arguments are relations between classes, class conflict, the various sectors of the economy (e.g., monopoly, competitive, and state sectors; see O'Connor [1973]), and the role the state plays in the process.

One version of the approach⁸ (the one we examine explicitly below)

⁷ Indeed, insurgency, if anything more than innocuous, is devalued as likely to have negative individual, group, or societal consequences. For other recent statements pointing to this assumption in pluralist theory, either generally or more specifically within classical theories of collective violence (relative deprivation/disorganization perspectives), compare Gamson (1975), Jenkins and Perrow (1977), and Snyder and Kelly (1979).

⁸ Other forms of the political conflict argument exist. However, these explanations typically locate the main forces affecting relief and other forms of social service expenditures in more stable aspects of class political organization. For example, Hicks, Friedland, and Johnson (1978) focus on the organizational density of unions and large corporations in the determination of distributive outcomes of state fiscal policies. Similarly, Hage and Hanneman (1977), in a longitudinal analysis of the growth of the welfare state in Britain, France, Germany, and Italy, develop measures of right and left political power in an effort to test a political conflict hypothesis of welfare-state growth

argues that the forces which initiate new welfare programs or trigger increased expenditures and rolls often derive from insurgent movements, particularly those that are especially disruptive. This line of argument is probably best developed by Piven and Cloward (1971) in their influential *Regulating the Poor* (see also George 1973). They argue that there are both qualitative and quantitative cyclical components to state welfare spending, and in their words (p. xiii), "the key to understanding of relief-giving is in the functions it serves for the larger economic and political order, for relief is a secondary and supportive institution. Historical evidence suggests that relief arrangements are initiated or expanded during the occasional outbreaks of civil disorder produced by mass unemployment, and are abolished or contracted when political stability is restored . . . expansive relief policies are designed to mute civil disorder, and restrictive ones to reinforce work norms." For example, in the 20th-century United States the labor movements of the 1930s (i.e., Unemployed Workers' and Industrial Workers' Movements; see Piven and Cloward [1977]) were followed three decades later by the Black Movement (i.e., Civil Rights and Welfare Rights Movements; see Piven and Cloward [1977]). During both major upheavals, concessions in the form of relief and social welfare programs were extracted from the state (Piven and Cloward 1971, 1977).

The Piven and Cloward (1971, 1977) version of this argument does not ignore the processes that the developmental argument suggests as important—for example, economic growth ("modernization") or need (unemployment, poverty levels, etc.). However, these forces alone are not considered the major *direct* causes of short-term welfare expansion. Instead, forces endemic to capitalist societies such as rapid modernization and depression (when extreme enough) periodically contribute to the development of "political disorder."⁹ Disorder or insurgent movements in turn are the direct forces which, if extensive or severe enough, have produced the expansion of relief rolls and purse. This conflict or political struggle perspective generally points to the fact that it is indeed political disorder which leads such expansion and not the reverse. This is a consistent theme in Piven and Cloward (1971, 1977) and has been argued by others as well. For example, Sawers and Wachtel (1975, p. 115) observe: "It is interest-

in each of these four countries. For present purposes and brevity there are good reasons for considering these two approaches (organizational and insurgency) as related but distinct arguments (see Isaac 1979). They are not, however, necessarily incompatible, as O'Connor's influential *The Fiscal Crisis of the State* demonstrates.

⁹ This aspect of the Piven and Cloward argument is a version of the classical perspective on collective protest and violence, variously referred to as the rapid-change or relative-deprivation perspective. Recent empirical work has cast serious doubt on the various versions of this perspective (e.g., Snyder and Tilly 1972; Lodhi and Tilly 1973; Jenkins and Perrow 1977; see Snyder [1978] for a useful overview of recent theory and research).

ing to note that both in the 1930s and 1960s social welfare programs did not really get underway until political uprisings posed a sufficient threat to the dominant class to force them into concessions." The fundamental distinction between the orthodox developmental perspective and this version of the political conflict/struggle perspective rests with the differing models of the state upon which these respective theories are erected.

The image of the state.—Whereas the orthodox (developmental) perspective assumes a class-neutral state responding in relatively automatic fashion to societal needs and dislocations, the political conflict/struggle perspective assumes that the nature and role of the state (and hence of state-controlled institutions) is substantially different. This alternative view of the state is not explicitly presented by Piven and Cloward. However, the broad framework can be outlined as we have done for the orthodox perspective.

In brief, the state is assumed to be inextricably linked to the capitalist social order (primarily the class and institutional structure). In a word, the state is embedded in capitalist society.¹⁰ Because of this, institutionalized politics (in general, electoral and interest-group mechanisms) are typically viewed as predominantly ineffective means for relatively powerless groups (i.e., lower- or working-class groups—in this case black Americans) to achieve basic socioeconomic change in their interest. Change which does result from time to time owing to institutionalized political efforts is argued to be inherently co-optive or reformist (see Piven and Cloward 1977). Rather, this perspective sees basic state response, perhaps leading to structural change, as resulting (however infrequently or insignificantly) from noninstitutionalized politics—that is, from social protest or collective violence (in short, from insurgency). It is not typically argued that insurgency always results in advantageous consequences for those who employ it. But advantageous outcomes, if they do occur, generally derive from pressure (threat) placed on the state and on dominant class interests.

The image of state-controlled welfare institutions is not one of class neutrality responding to fluctuations in discretionary resources and general

¹⁰ Theoretical developments in the class perspectives on the state have surged recently, and the literature is much too extensive to present in any detail here. It is worth noting, however, that the image of the state implicit in Piven and Cloward's work appears to have shifted somewhat. In *Regulating the Poor* (1971), the image seems primarily instrumentalist but is never explicitly stated as such. In their more recent *Poor Peoples' Movements* (1977), the underlying model is stated explicitly to be structuralist (see p. 30, n. 31). Jenkins (1979) has commented somewhat differently on this apparent shift in Piven and Cloward's model of the state. Our purpose is not to develop a theory of the state, but only to sketch broadly the basic assumptions which differ between the orthodox and conflict models of state expenditures. For a detailed presentation of a Marxist view of various forms of state spending, see Foley (1978). For excellent overviews and recent developments of the various versions and debates within the general class perspective on the state, see Gold, Lo, and Wright (1975), Esping-Anderson, Friedland, and Wright (1976), Block (1977), and Wright (1978).

levels of societal need; instead, it is one in which the state employs welfare institutions (or at least some aspects thereof) as social control mechanisms. Expansion on a major scale takes place only when it must—only when threat is sufficient. Implicit in this reasoning is the idea that civil disorder or insurgency (in this case racial violence) serves as a constraint resource (in Gamson's [1968] terms; see also Piven and Cloward [1977, p. 24]). Such a constraint may motivate particular actors (governments) to respond to some demands.

The general impression given by Piven and Cloward's argument, though not stated precisely in these words, is that welfare institutions are often employed to serve one of the major functions of the capitalist state—that of legitimation.¹¹ Welfare institutions expand substantially only when they must do so to undercut mounting threat to the state and socioeconomic order. In this sense, the long-term interests of the capitalist class in general may be preserved (i.e., the ability to continue to accumulate profitably), although some capitalist interests may be forgone or harmed in the short run.¹² Thus, for Piven and Cloward (1971) and some other political conflict/struggle theorists of welfare institutions (e.g., George 1973), the fundamental premise on which the insurgency-welfare expansion relationship is predicated consists of these basic features of the nature of the state.

The insurgency-welfare relationship: summary of previous empirical research.—In addition to their own attempt to present data supportive of their thesis, several other related empirical studies bear on the Piven and Cloward hypothesis. These studies attempt to address an important theoretical question, the insurgency-welfare relationship, which has too often been either (a) critically dismissed by many orthodox theorists (e.g., Grønberg 1977) without empirical refutation or (b) uncritically accepted by some radical theorists (e.g., Sawers and Wachtel 1975; Szymanski 1978) without convincing empirical support. However, each of the putatively supportive (i.e., Piven and Cloward 1971; Betz 1974; Welch 1975; Button 1978; Jennings 1979) and disconfirming empirical studies (e.g.,

¹¹ We follow O'Connor's (1973) meaning of the concepts of accumulation and legitimation (his term is legitimation). In his words, the former (p. 6) "... means that the state must try to maintain or create the conditions in which profitable capital accumulation is possible ..." and the latter means, "... the state also must try to maintain or create the conditions for social harmony." For additional discussion of the multiple functions of the capitalist state and the mechanisms through which they are implemented, see Szymanski (1978).

¹² For an argument which questions the classical logic underlying such presumed short-term loss, see Hirschhorn (1978). It is worth mentioning at this point that the Piven and Cloward version of this argument contains three important theories: (1) a theory of political disorder (which we previously suggested lacks empirical support; see n. 9), (2) a theory of welfare or relief expansion, and (3) a theory of the functions or consequences of relief-giving and welfare institutions. It is primarily the last two features which have not been adequately addressed empirically. Our analysis focuses exclusively on the second theory.

Durman 1973; Albritton 1979) is plagued by one or another (in some cases several) methodological shortcoming(s) which render the evidence suspect to varying degrees.

The picture that emerges from these empirical studies is somewhat mixed in findings and in design characteristics. One local level (Betz 1974) and one state-level (Jennings 1979) study report findings variously supportive of the riot-welfare hypothesis. In addition, Welch (1975) contains some consistent but ignored evidence on riots and governmental revenue inflows to cities, and Button (1978) cites supportive elite interview data. In contrast, two local level studies (Durman 1973; Albritton 1979) argue for an interpretation of the "welfare explosion" which tends to center predominantly on legislative amendments to the Social Security Act of 1935.

In general, limitations of methods and design characterize each of these studies to varying degrees. We suspect that the following features are most problematic: (a) not directly relating riots to welfare changes (e.g., Piven and Cloward 1971; Durman 1973; Betz 1974; Albritton 1979), (b) exclusively employing riot frequency and ignoring severity (e.g., Welch 1975; Button 1978; Jennings 1979), (c) failure to control simultaneously for other important determinants of relief fluctuations (e.g., Piven and Cloward 1971; Durman 1973; Betz 1974; Albritton 1979), (d) predominant focus on cities and local level processes (e.g., Piven and Cloward 1971; Durman 1973; Betz 1974; Welch 1975; Albritton 1979), and (e) failure to consider time or the dynamics of the riot-welfare relationship (e.g., Durman 1973; Betz 1974; Button 1978; Albritton 1979).

THE PRESENT ANALYSIS

The present analysis addresses these problematic limitations. Moreover, we think there are several important reasons for addressing the following question: if there were, in fact, governmental responses to racial insurgency in the form of relief expansion, at what levels of government and through which programs were such responses implemented? First, along these lines, Piven and Cloward point to the existence of a differential government response to black insurgency. They observe (1971, p. 245): "At first glance these data might suggest a simple and direct relationship between growing black turbulence . . . and greater responsiveness by local government. . . . However, local government was also under pressure to conciliate blacks from quite another source during the 1960's; namely, the federal government. Even as many urban political leaders continued to fight a rearguard action against blacks on behalf of their constituents, national political leaders were developing programs that represented concessions to blacks." This argument suggests no relief response, or perhaps a small one, on the part of local governments and substantially greater response by the federal govern-

ment. Indeed, a major thrust of Piven and Cloward's argument centers on electoral instability—in particular, the fiscal and political strategies of the faltering Democratic party as the 1960s approached. In their words (p. 261), “. . . the hallmark of the Great Society programs was the direct relationship between the national government and the ghettos, a relationship in which both state and local governments were undercut.”

Second, although variously flawed, several previous studies present evidence consistent with this divergent local-national pattern. While no study employing city-level data reported a net relationship between riots and local welfare expansion (e.g., Welch 1975; Kelly 1977),¹³ local riots appeared to have a positive net effect on the inflow of state and federal funding categories (Welch 1975; Button 1978).

Third, most attention concerning the “welfare explosion” has been focused on AFDC expenditures and rolls, which, while primarily disbursed at the state and local levels, originate with the federal government (President's Commission on Income Maintenance Programs 1969). Therefore, the hypothesis should be examined across different units of analysis and different programs, reflecting various components of the state welfare apparatus.

Fourth, the racial disturbances of the 1960s grew to be primarily a national rather than a local phenomenon. Although certain cities (in particular, a number of large, northern, industrial cities) experienced greater riot frequency and/or severity, the threat which such collective protest and violence produced was readily disseminated to the entire nation on a day-to-day basis by means of the national news media (see Janowitz 1969; Oberschall 1978). In other words, we think it quite reasonable to hypothesize that local political officials were as concerned with averting the onset of rioting in cities where it had not yet occurred as other officials were with quelling any further outbreaks in cities where substantial rioting had already taken place. In addition, the federal government became increasingly the target institution for the black movement (Button 1978). Moreover, the federal government's spending strategies did not appear to be severely hampered by negative public opinion during most of the 1960s. Indeed, despite the wake of the black violence, public opinion surveys during this period indicated majority white approval for large-scale government spending directed at the reduction of racial inequalities (see Blumenthal et al. 1972; Campbell and Schuman 1968).

Finally, as we argued above, all explanations of state expenditures of any type make implicit or explicit assumptions concerning the nature of the state. Such underlying models of how the state is assumed to respond to

¹³ Betz (1974) is a partial exception. However, he demonstrated a simple relationship, not a net effect, and his welfare category was not purely local but a composite of state and local expenditures.

internal processes (e.g., unemployment and business cycles, poverty levels, demographic shifts, political conflict and disorder) further suggest the importance of examining these patterns at the national level. Since the federal government controls the greatest share of the state fiscal apparatus (including the welfare purse), the relationships involving these various models of the state might be most clearly revealed at the national level.

Basic Expectations and Hypotheses

Based on the foregoing presentation of broad perspectives, our analysis revolves around the general evaluation of some of the more central aspects of the developmental and political conflict models of relief institutions. The dominant concern, in this regard, is the role which rather violent racial insurgency (urban riots) may have played in expanding dimensions of major relief programs in recent U.S. history. Our argument, then, is that highly violent or disruptive insurgencies are central to the political conflict/struggle argument, but assumed to be innocuous or even detrimental to insurgent interests according to the orthodox viewpoint.¹⁴

Furthermore, on the basis of previous arguments, we expect that (1) racial disorder severity will have a greater impact on relief-program expansion than sheer disorder frequency, and (2) across program categories, those that are primarily controlled at the central (federal) level will respond more dramatically to these insurgencies than those at the state or local level.

DATA AND METHODS

Local Level

Data.—Our analysis of the local response to racial insurgency employs all cities in the continental United States with at least 25,000 inhabitants in 1960 ($N = 670$). This approach avoids the problem of only using small samples of large cities. Additional logic recommending cities of this size, rather than some alternative areal unit (e.g., SMSAs), to assess the local response is more fully elaborated elsewhere (see Kelly and Snyder 1980). Moreover, since we have information on racial violence frequency and se-

¹⁴ We recognize that some versions of orthodox or developmental argument (with pluralist state premises) do refer to or acknowledge the role of protest or mass political pressure (e.g., Marshall 1950). However, the form of such collective political action is crucial in making the theoretical distinction which we want to make. Typically, orthodox arguments center on the possibly beneficial aspects of peaceful protest or pressure, but argue that more violent or threatening forms of social protest tend to be detrimental in that, among other things, they can lead to the loss of important third-party support (e.g., Schumaker 1975). Our thanks to David Snyder, David Knoke, and Paul Burstein for raising questions concerning this issue.

verity for these cities, we avoid the problems associated with dividing the sample into riot and nonriot cities. We are thus able to make use of a greater amount of information (i.e., variation in the characteristics of racial violence) than previous studies.

Equations.—In order to test the local welfare response to racial insurgency, we estimate (by ordinary least squares [OLS]) two lagged dependent variable panel regression equations¹⁵—one containing frequency of racial disturbances and a counterpart containing the severity of disturbances.¹⁶ These equations take the following form:

Welfare\$/capita₁₉₇₀ =

$$\begin{aligned} & b_0 + b_1 \text{ Welfare}\$/\text{capita}_{1960} + \\ & b_2 \text{ Total Public}\$/\text{capita}_{1960} + b_3 \text{ Poverty}_{1960} \\ & + b_4 \text{ City Government}_{1960} + b_5 \text{ Number Nonwhite}_{1960} \\ & + b_6 \text{ South} + b_7 \text{ Racial Violence}_{(1961-68)} + \varepsilon_{1970}. \end{aligned} \quad (1)$$

Variables.—We employ two indicators of racial violence: simple frequency and a multiple-indicator composite index of severity. Frequency is measured by summing the total number of riots occurring in a given city over the interim period covered by equation (1), that is, 1961–68.¹⁷ The severity index¹⁸ consists of four standardized (z-scored) components: number killed, number injured, number of participants, and duration of riots in days.

It is important to control for variables which may be related to the incidence of racial violence and likewise to control for determinants of changes in welfare spending (see Snyder and Kelly [1979] on this point). The only variables which have been consistently found to be important determinants of frequency and severity of racial violence are number nonwhite and regional location of a city (i.e., South/non-South; see Spilerman [1971, 1976]). Both of these variables are included in equation (1).

Several determinants other than racial violence appear in equation (1).

¹⁵ This procedure results in the analysis of change because the lagged dependent variable residualizes the dependent variable in terms of its time -1 (1960) level. The residualized values represent the amount of change over the period specified (see Bohrnstedt 1969; Heise 1970; Land and Felson 1976).

¹⁶ The estimates of frequency and severity, here and throughout our analysis, are arrived at through separate equations since they are highly collinear with each other.

¹⁷ We do not include 1969 disorders in the city panel equations since the dependent variable is actually observed for the 1969–70 period.

¹⁸ The estimated internal consistency reliability (Cronbach's Alpha) of the severity composite is .901. Although other strategies have been used to measure severity, they typically involve rank ordering of events based on a priori cutoffs or definitions of a severe disorder (see Wanderer 1968; Spilerman 1976). Rather than rank-order events, we prefer the strategy of allowing the components to vary without such restrictions.

First, the 1960 per capita level of local government welfare expenditures appears as a lagged dependent variable (Welfare\$/capita, 1960). Two other factors are included—one as an indicator of government capacity to increase welfare spending and the other as an indicator of need in the local population. Capacity is measured by total per capita local government expenditures less welfare expenditures (Total Public\$/capita, 1960); cities with larger total per capita expenditure budgets (primarily a reflection of previous-period revenues) are likely to be in a better position to increase welfare resources (Greenwood and Sweetland 1972). Citizen need is measured by the percentage of families with less than \$3,000 annual income in 1960 (Poverty, 1960). Finally, arguments in the theory of urban politics (e.g., Lineberry and Fowler 1967; Eisinger 1973; Friedland 1976) suggest that nonreform city administrations should be more responsive to minority needs than reform administrations. Therefore, equation (1) also includes a dummy variable for City Government Form (1 = nonreform or major-council commission form of city administration; 0 = reform or city manager government). Further information on variable construction and data sources is contained in the Appendix.

National Level

Data.—In an attempt to pursue further the insurgency-welfare expansion hypothesis and to test our arguments concerning differential program response, we also employ annual aggregate time-series observations for the United States over most of the postwar period (i.e., 1947–76). (Data sources and additional detail on variable construction are reported in the Appendix.)

Dependent variables.—The multivariate analyses of national level year-to-year changes in relief focus on several aggregate aspects of various major program categories. The specific dependent series can be categorized as follows: (1) aggregate (real) expenditures, (2) aggregate rolls (or number of receiving units), and (3) average (real) benefits per receiving unit.

The various expenditure series are total public assistance expenditures originating from (a) federal, (b) state, and (c) local sources. While the public assistance category itself is a composite of all expenditures for four basic programs (Old-Age Assistance [OAA], Aid to the Blind [AB], Aid to the Permanently and Totally Disabled [APTD], and Aid to Families with Dependent Children [AFDC]), it allows an assessment of at least one important question, that is, the differential response of the overall public assistance program as it is funded under federal, state, and local auspices. It should also be borne in mind that any riot effects on these series will be a joint result of the movements in all four program series. Nevertheless, these annual fluctuations should provide an indication of the responsive-

ness of the general public assistance program to developmental and conflict processes.

We also analyze two other important and more specific expenditure series—AFDC and General Assistance (GA) expenditures. The former is sponsored and heavily funded by the federal government, though administered locally (President's Commission on Income Maintenance Programs 1969). The General Assistance program, however, is financed by state and local revenues only (Stevens 1970, p. ix; U.S. Bureau of the Census 1975, p. 381). General Assistance is intended to provide aid to those who are poor, unemployed, or unable to gain support from some other major assistance program (e.g., AFDC or Supplementary Security Income after 1973; see Schiller [1976]).

The second general category of relief-program characteristics consists of program rolls or the number of receiving units. (Recall, for example, that AFDC rolls and not expenditures provided the primary focus for Piven and Cloward [1971].) Here we examine annual changes in both AFDC rolls (number of receiving families) and GA rolls (number of receiving individuals).

A third general characteristic of these programs is also of interest: annual changes in benefit levels per receiving unit. Changes in benefits per unit in the aggregate will be a function of changes in aggregate expenditure levels and changes in the size of program rolls. Specifically, we employ the average real monthly benefits per AFDC family and GA recipient.

Aggregate annual plots (not presented here) of the major relief program characteristics and racial insurgency activity over the postwar period indicate that (1) the AFDC program has been larger than the GA program on all three dimensions (aggregate real expenditures, rolls, and average real benefit levels) over the period; (2) AFDC characteristics were rather stable until the rapid expansion in the mid- to late 1960s, but GA characteristics show much more year-to-year volatility; and (3) each of the six program characteristics variously reveals upward swings which tend to be most steep around the mid- to late 1960s—precisely the period of most frequent and severe riot activity.¹⁹ Such temporal covariation is suggestive but, of course, not very convincing evidence for the political conflict/struggle argument. We withhold any inferences until more complete specifications are presented and estimated below.

Racial violence measures.—We continue to employ the same two measures of racial violence described in the preceding description of our city-level data. The only change is that now the city-level observations have been aggregated to provide national annual observations for the frequency and severity of riots. Our racial violence series have been obtained from

¹⁹ Because of space limitations, figures displaying these welfare series are not presented. However, they are available from us on request.

several sources (see the Appendix) common to other studies using similar types of event data (e.g., Snyder and Tilly 1972; Lodhi and Tilly 1973; Jenkins and Perrow 1977; Burstein 1979; Kelly and Snyder 1980).

Furthermore, evidence on news reports of certain event data has accumulated rather consistently, suggesting that such data are quite valid under most conditions—especially for events (e.g., riots) which tend to be rather severe (see Snyder and Kelly 1977; Jenkins and Perrow 1977; Monti 1979). Since our series are all concerned with features of riots involving at least 30 participants, some property damage, and/or injury to persons, we feel that the series are as reliable and valid as may be obtained on such events. Our severity composite, which displays extremely high internal consistency, removes most doubt concerning high levels of random measurement error.

Other predetermined variables and equation specification.—Our previous examination of the research focusing on welfare and relief programs suggests several other variables which are thought to cause changes in welfare spending and rolls. Particularly important in this regard are variables central to the developmental perspective. Therefore, in all expenditure equations, in addition to the racial violence measures we include as exogenous determinants: (a) gross national product per capita (GNP/C), in constant dollars, which is a standard indicator of national economic well-being or development; (b) percentage of the labor force unemployed (% Unem), as an indicator of fluctuations in labor market conditions and therefore societal demand for various forms of relief; (c) party of the executive in office (Party), since there is evidence to suggest differential party proclivities for welfare spending and objectives surrounding the classic Phillips curve trade-off between inflation and unemployment (see Hibbs 1977) (this variable is constructed as a standard zero/one dummy assigned to each year: Democrat = 1, Republican = 0); and (d) military expenditures (Military\$), in millions of constant dollars, since there is evidence suggesting that an inverse substitution effect exists between forms of spending (e.g., Russett 1971). The general form of our aggregate program expenditure equations, therefore, is:

$$W_t^* = f(W_{t-1}^*, \text{GNP/C}_{t-1}, \% \text{ Unem}_{t-1}, \text{Party}_{t-1}, \text{Military}_t^*, \text{RV}_{t-1}), \quad (2)$$

where W^* is a general term representing the current period value of any one of the specific welfare spending categories previously introduced, and RV_{t-1} represents one of the two measures of racial violence—frequency or severity.²⁰ Throughout the analysis the natural logarithm of riot frequency

²⁰ Preliminary estimation employing additional variables led us to eliminate two other potential determinants of expenditure changes: (a) Fuchs's relative poverty index (see Fuchs 1969) and (b) a measure of urbanization. Both can be viewed as developmental "demand" indicators.

is employed instead of the raw-event frequency, since it is theoretically reasonable to expect disturbance frequency to demonstrate decreasing marginal increments in an outcome response (Button 1978; Eisinger 1974).²¹

For the welfare-roll equations, Party and % Unem are included, following the same logic as above. We also specify the following as exogenous determinants of welfare-roll changes: (a) War is a dummy variable for war and nonwar years (i.e., war years = 1950–53, 1965–73 = 1, other years = 0), since wars are likely to draw off (or absorb) portions of the “surplus population” that might end up on welfare rolls if it were not for higher conscription rates (see Baran and Sweezy 1966; O'Connor 1973); and (b) average monthly benefit level per unit (Benefit/Unit), in constant dollars, is included since it has been argued that benefit levels may induce higher motivation to seek assistance (e.g., Winegarden 1973). The general specification of our welfare-roll equations is as follows:

$$N_t = f(N_{t-1}, \text{Benefits/Unit}_{t-1}, \text{Party}_{t-1}, \% \text{ Unem}_{t-1}, \text{War}_t, \text{RV}_{t-1}), \quad (3)$$

where N is a general term representing one of the roll measures and all other variables are as before.²²

For the benefit-per-unit equations, GNP/C, Party, Military³, % Unem, War are all employed (in addition to the racial violence measures), following the same logic as in the expenditure and roll equations. The general specification of our benefit equations is:

$$\text{Benefits/Unit}_t = f(\text{Benefits/Unit}_{t-1}, \text{GNP/C}_{t-1}, \text{Party}_{t-1}, \text{Military}_t^3, \text{War}_t, \% \text{ Unem}_{t-1}, \text{RV}_{t-1}). \quad (4)$$

Our equations take the form of partial adjustment models (see Johnston 1972) in which current-period relief components are taken as partially a function of previous-period levels and partially adjusting to other exogenous conditions in the interim. All exogenous variables except war years and military spending are lagged one period to allow for a reasonable period of institutional adjustment by state managers. We also examined current-period exogenous effects and point out below cases in which the two tended to differ substantially.

Estimation.—We estimated welfare-expenditure, roll, and benefit equations of the form presented in equations (2)–(4) by ordinary least-squares (OLS) regression. It is well known that lagged dependent variable models

²¹ Furthermore, the semilogarithmic specifications best fit the frequency data in this study and the cross-national frequency data over time in another study (Isaac 1979).

²² Other variables examined as determinants of changes in rolls but eliminated because of their consistent empirical unimportance were: (a) Fuchs's relative poverty index, (b) urbanization, (c) GNP/C, (d) population level, and (e) the number of female-headed poverty families with children under 18 (see Platky [1978] for arguments and data).

of this form produce biased and inconsistent estimates under conditions of nontrivial serial correlation (e.g., see Johnston 1972). Therefore, the results of four tests for serial correlation (Durbin-Watson d , Geary's τ , Durbin's h , and Durbin's second method) were examined.²³ The preponderance of the evidence in all but three of the 22 equations indicated that the null hypothesis of no significant serial correlation should not be rejected. The three problematic equations (with ρ in the range of $-.4$) were reestimated via pseudo-generalized least squares (GLS) (see Hibbs 1973). The results of the GLS estimations altered neither the salient features of the parameter estimates nor the central inferences we draw below.

RESULTS

The Local Level

Table 1 presents the parameter estimates of the basic two-wave (1960–70) panel model for total per capita welfare-expenditure changes across all U.S. cities with populations of at least 25,000 in 1960.²⁴ Owing to the highly collinear racial violence measures, separate equations containing frequency and severity are estimated here and throughout.²⁵

Each equation accounts for slightly more than half the variation in changes in per capita welfare expenditures over the decade. The bulk of the explained variance, however, is due to the autoregressive effect, not to the variables of central theoretical interest. Beyond the lagged value of the dependent variable, only total municipal expenditures (less welfare) per capita shows a consistently significant and sizable standardized effect.²⁶ Although riot severity is statistically significant, in substantive terms the effect is trivial. The major developmental perspective variable representing "demand"—size of the poverty population in 1960—appears to play a

²³ We rely on multiple tests for serial correlation since the Durbin-Watson d is biased in the direction of the null hypothesis and others are essentially large sample tests. For a discussion of the Geary test, see Geary (1970). For discussion of the others, see Johnston (1972).

²⁴ Because of space limitations, variable means and standard deviations are not presented here. They are, however, available from us (for the city- and national-level data) on request.

²⁵ We have also estimated our equations employing an interactive measure of frequency and severity of riots by summing the national logarithms of the two components (for other examples of this procedure, see Hibbs [1976]). The results of this specification, not presented here, produced violence effects which were consistently situated between the separate frequency and severity effects presented in this paper.

²⁶ We report significance levels throughout. However, such tests do not lend themselves to strict traditional interpretation since we are not employing sample data. We rely on such tests primarily for information concerning the variability of the estimates. We indicate, as meaningful, estimates significant at the 5% (or less) level for the city-level data and at the 10% (or less) level for the national time-series data.

Racial Insurgency and Welfare Expansion

TABLE 1

OLS ESTIMATES FOR LAGGED DEPENDENT VARIABLE EQUATIONS FOR DETERMINANTS OF DECADE CHANGES IN LOCAL PER CAPITA WELFARE EXPENDITURES, UNITED STATES CITIES (with Population of 25,000+ in 1960), 1960-70 ($N = 670$)

Predetermined Variables	Frequency Equation	Severity Equation
Welfare expenditures/capita, 1960.....	.941**	.936**
	.536	.534
Total government expenditures/capita, 1960 ^a061**	.068**
	.315	.400
Poverty population, 1960 ^b431	.459
	.021	.021
City government form, 1960.....	-.002	-.001
	-.031	-.036
Nonwhite population, 1960.....	.00004	.00002
	.007	.011
South.....	.002	.001
	.012	.007
Frequency, 1961-68.....	.012	...
	.056	
Severity, 1961-68.....001*
		.061
Constant.....	-.001	-.001
R^2531	.532

NOTE.—Coefficient below the metric is the standardized slope.

^a Multiplied by 1 million.

^b Multiplied by 1,000.

* Significant at $P \leq .05$.

** Significant at $P \leq .01$.

trivial role in inducing greater overall welfare expenditures across U.S. cities.

We had earlier hypothesized that sole reliance on local level data (cities) may inappropriately mask fundamental state welfare response to riots. The meager effects of both racial violence frequency and severity are consistent at this point with such an argument. On the basis of these results, we maintain that, as we suggested above, the theoretical arguments should be evaluated on alternative units of analysis and for various programs rather than prematurely rejecting the political conflict/struggle argument.

National Level

Aggregate relief expenditure responses.—We begin the presentation of the national aggregate time-series results with the public assistance category equations originating with various levels of government. We estimated two equations each (one for frequency and one for severity of riots) for federal, state, and local public assistance over the period 1947-69. Beginning with 1970, the state and local categories were no longer reported

separately. Therefore, we estimated two equations each for federal and the composite state/local public assistance categories over the 1947–76 period. These 10 equations are contained in table 2.

One of the most dramatic features of table 2 is the magnitude (and significance) of what are shown as the metric slopes for riot frequency. These are not, however, metric slopes in the strict sense. Since we use the natural logarithm of frequency, these coefficients indicate the effects of a proportional change in the annual frequency of riots. When divided by 100, they yield the change in the dependent variable per 1% change in frequency at any level of frequency (e.g., see Stolzenberg 1980). For instance, in the first federal public assistance equation, a 1% increase in frequency (at any level of frequency) produces a \$1.32 ($= 131.83/100$) million increase in federal public assistance expenditures in the subsequent year. The standardized coefficients for frequency have been calculated on the variance of the raw frequency series and evaluated at the mean of that series.²⁷

Several points should be made concerning table 2. First, and perhaps most important, is that the riot variables are consistently important in influencing public assistance expenditures at all levels of government during the postwar years. This is the case irrespective of whether one focuses on the magnitude of the metric slopes (or proportional effects) or on relative standardized effects of the variables in each equation. Moreover, the standardized slopes indicate that the conflict variables (frequency and severity) are relatively more efficacious in producing public assistance expenditures than either of the central developmental variables—that is, per capita income (GNP/C) and the percentage of the labor force unemployed (% Unem).

The expectation that public assistance expenditures originating at more centrally located governmental levels would be most responsive to the urban riots is not borne out by the data in table 2. The equations estimated on the right-censored series (1947–69) show the greatest frequency effect ($\beta = .620$) for state-level public assistance and the greatest severity effect ($\beta = .351$) for local level public assistance expenditures. For the complete series (1947–76), neither frequency nor severity shows dramatically different effects across the federal and state/local public assistance expenditures categories.

There is a second unanticipated result. We expected riot severity to produce larger changes in program responses than frequency. However, this is not at all the case in the public assistance expenditure responses. The larg-

²⁷ Neither unstandardized nor standardized slopes are constant; they vary over the entire range of the independent variable (i.e., frequency) in the semilogarithmic specification. The standardized slope is commonly recovered from the semilogarithmic results by the following formula: $\beta = (b/\bar{x}) (\sigma_x/\sigma_y)$ (e.g., see Stolzenberg 1980). We have chosen to evaluate these slopes at the mean of x .

TABLE 2

OLS ESTIMATES FOR LAGGED DEPENDENT VARIABLE EQUATIONS OF DETERMINANTS OF CHANGES IN LEVEL-SPECIFIC PUBLIC ASSISTANCE EXPENDITURES, UNITED STATES, 1947-69 AND 1947-76

TIME PERIOD AND PUBLIC ASSISTANCE EQUATION	PREDETERMINED VARIABLES (Metric and Standardized Coefficients)							EQUATION STATISTICS					
	Y_{L-1}^a	GNP/C _{L-1}	Party _{L-1}	Military _{L-1}	% Unem _{L-1}	ln Fre- quency _{L-1}	Severity _{L-1}	α^b	\bar{R}^2	D-W	r^d	h^e	$\hat{\rho}^f$
1947-69:													
Federal P.A.....	.852***	.166	84.955	.001	-3.939	131.830*	...	-249.208	.989	2.022	15	-.71	-.06
	.732	.071	.036	.020	-.004	.270	...						
Federal P.A.....	.800***	.508	117.291*	-.001	9.103	...	36.609*	-.987.750	.989	1.792	13	I	-.05
	.687	.217	.050	-.014	.009129						
State P.A.....	.878***	-.123	-18.112	-.001	-19.570	181.840***	...	606.553	.979	2.076	9	-.23	-.03
	.656	-.090	-.013	-.013	-.032	.620	...						
State P.A.....	.782***	.332*	21.062	-.005	-8.767	...	46.914**	-.302.836	.967	2.399	10	I	-.19
	.584	.242	.015	-.106	-.014282						
Local P.A.....	.310	.148*	14.169	.001	5.410	67.834***	...	-217.789	.973	1.783	9	I	.03
	.275	.281	.027	.042	.023	.600	...						
Local P.A.....	.073	.357***	25.779	-.001	15.394	...	22.336**	-.651.014***	.969	1.758	11	I	-.02
	.065	.680	.049	-.040	.066351						
1947-76:													
Federal P.A.....	.990***	-.331	-11.544	.004	42.732	213.690**	...	612.154	.976	2.680	16	-2.65	-.43*
	.967	-.097	-.002	.023	.022	.210	...						
Federal P.A.....	.994***	-.087	47.886	.009	37.043	...	40.386	-.104.666	.967	2.271	12	-1.32	-.15
	.961	-.025	.010	.054	.019065						
State and local P.A....	1.024***	-.221	-35.184	-.001	-13.404	202.287***	...	664.712	.982	2.762	14	-2.39	-.46*
	.972	-.067	-.008	-.001	-.007	.200	...						
State and local P.A....	.990***	.076	8.135	.003	-14.386	...	40.988*	-.114.320	.975	2.321	12	-1.09	-.15
	.948	.023	.002	.019	-.008068						

NOTE.—All monetary variables are in constant (1967 = 100) values.

^a Denotes the lagged dependent variable for each equation.

^b Constant term.

^c Adjusted for degrees of freedom.

^d Number of residual runs of signs or sign changes.

^e Durbin's h -test ($I = \text{interdeterminate}$).

^f First-order autoregressive serial correlation estimated by Durbin's second method.

* Significant at $P \leq .10$.

** Significant at $P \leq .05$.

est difference occurs for the state-level category (frequency: $\beta = .600$, and severity: $\beta = .282$). We shall return to these unanticipated findings after examining related results.

The more specific expenditure-category equations—AFDC and GA—are presented in table 3. Focusing first on the AFDC expenditure equations, the two riot variables again show the largest relative effects on short-term expenditure changes, with the exception of the autoregressive slope.²⁸ In particular, the riot variables are again substantially larger than the unemployment effects (which are virtually zero) and larger than the per capita income effects; the latter are not statistically significant but are negative, contrary to orthodox expectations.

The third and fourth equations contain the parameter estimates for changes in General Assistance expenditures. Several important differences emerge here. In particular, the riot variables have the smallest relative effects in both equations, with the sole exception of the unemployment slope. Our indicator of capacity (per capita income) is positive and has the strongest relative effect in both equations. As with the previous equa-

TABLE 3
OLS ESTIMATES FOR LAGGED DEPENDENT VARIABLE EQUATIONS FOR
DETERMINANTS OF CHANGES IN AFDC AND GENERAL ASSISTANCE
EXPENDITURES, UNITED STATES, 1947-76

PREDETERMINED VARIABLES	EXPENDITURE EQUATIONS			
	AFDC	AFDC	GA	GA
Y_{t-1}^a	1.107***	1.067***	.628***	.630***
	1.045	1.007	.567	.569
GNP/ C_{t-1}	-.421	-.144	.105*	.114
	-.160	-.055	.415	.450
Party $_{t-1}$	-89.221	-55.646	-60.771	-58.720*
	-.024	-.015	-.172	-.166
Military s002	.004	-.004**	-.003**
	.017	.031	-.285	-.273
% Unem $_{t-1}$148	4.650	1.531	1.943
	.0001	.003	.011	.014
Frequency $_{t-1}$	162.840***	...	9.834	...
	.210130	...
Severity $_{t-1}$	35.547**	...	2.718
074059
α^b	1061.876	341.324	13.226	-12.260
R^{2c}992	.986	.852	.850
D-W	2.865	2.048	2.056	2.082
τ^d	20	14	16	16
h^e	-2.52	-.16	I	I
β^f	-.46*	-.04	-.13	-.17

NOTE.—See notes for table 2.

²⁸ The metric slopes for the lagged dependent variable in the two AFDC equations exceed unity. In a strict sense, this implies nonsense dynamics (e.g., see Nielsen and Hannan 1977). However, the slopes do not depart radically enough from the less-than-unity expectation to be an overwhelming concern for the purposes at hand.

tions, riot frequency has a somewhat larger effect than severity. The comparison of AFDC and General Assistance expenditure responses to racial disturbances, however, does conform to our expectations about the differential responsiveness of more centrally controlled programs, at least in terms of expenditures.

In short, the aggregate expenditure equations contained in tables 2 and 3 indicate (1) the relative prominence of racial insurgency in producing short-term expenditure fluctuations, (2) the relative unimportance of two theoretically important indicators of capacity and demand, (3) the basic divergence of this pattern (i.e., 1 and 2) for General Assistance expenditures which tended to be slightly sensitive to the riots but depended more on the general economic climate or capacity²⁹ (as indicated by income per head), and (4) a tendency for the AFDC purse to be slightly more sensitive to the riots than the GA counterpart.

Aggregate relief roll responses.—As with the expenditure results, we find that the riots (primarily frequency) contributed to short-term increases in both AFDC and GA program rolls (see table 4). There are differences, however, with respect to the political-economic processes which alter the number of receiving units as compared with aggregate expenditure changes. Perhaps most interesting is the consistent negative unemployment effect across both sets of equations. While unemployment bears only a very modest positive relationship to expenditure changes, the jobless rate (in a previous period) appears actually to remove beneficiaries from rolls in the subsequent period, at least for the GA program. It is important, however, to point out that this is not the case for current-period unemployment. Contemporaneous unemployment, while leaving AFDC rolls virtually unaltered, does produce strong increases in the number of GA recipients.³⁰

In general, the comparative examination of AFDC and GA changes indicates that the riots were the single most important determinant in pro-

²⁹ This distinction in pattern for GA expenditure changes is even more marked in the specification which takes all predetermined variables in the current period instead of the mixed specification of current and lag-1 effects presented in table 3. The estimates (standardized below the metric slopes) for GA expenditures in this case are:

$$\begin{aligned} GA_t^{\$} = & -140.75* + .52***GA_{t-1}^{\$} + .08*GNP/C_t - 55.58**Party_t \\ & - .003**M_t^{\$} + 50.39***\%Unem_t + 17.38*\ln Frequency_t, \end{aligned}$$

$$\bar{R}^2 = .93, D-W = 1.95, \tau = 11.$$

This specification shows GA expenditures to be somewhat more sensitive to demand (% Unem) than to riots in the current period. The results for the same equation but with riot severity substituted for frequency were highly similar. The standardized slopes for the equation given above are .47, .31, .16, .23, .38, and .24, respectively.

³⁰ The current-period standardized slopes for % Unem and ln Frequency were .647 and .930, respectively.

TABLE 4

OLS ESTIMATES FOR LAGGED DEPENDENT VARIABLE EQUATIONS OF
DETERMINANTS OF CHANGES IN AFDC AND GENERAL
ASSISTANCE ROLLS, UNITED STATES, 1947-76

PREDETERMINED VARIABLES	ROLL EQUATIONS			
	AFDC Families	AFDC Families	GA Recipients	GA Recipients
Y_{t-1}^a	1.040***	1.010***	.570***	.631***
	.972	.945	.568	.628
Benefit/Unit $_{t-1}$	-2.005	2.919	-.808	.262
	-.044	.064	-.086	.028
Party $_{t-1}$	-38.015	-68.663	58.881	36.582
	-.018	-.033	.134	.083
War $_t$	-41.871	-5.967	-322.347***	-269.619***
	-.020	-.003	-.736	-.615
% Unem $_{t-1}$	-35.047	-38.546	-64.023*	-65.874*
	-.042	-.046	-.364	-.375
Frequency $_{t-1}$	84.167***	...	47.503**	...
	.190530	...
Severity $_{t-1}$	10.610	...	11.794
039205
α^b	383.752	-57.426	750.513***	726.112***
R^2_c993	.991	.639	.577
D-W	1.816	1.428	2.222	2.334
τ^d	12	12	16	12
h^e50	.56	I	I
$\hat{\rho}^f$09	.37	-.22	.05

NOTE.—See notes for table 2.

ducing short-term expansion of the AFDC program. General Assistance was also heavily influenced by uprisings of the black population (comparatively, perhaps more so), but responded more dramatically to other political-economic conditions occurring over the postwar period.

Finally, by all indications (e.g., autoregressive parameters, R^2 s adjusted for the contribution of the constant [not presented here]; see Theil [1971, pp. 175-78]), features of the AFDC program over the period were near equilibrium levels. This, however, did not appear to be the case for the General Assistance program, which adjusted much more dramatically to business cycle and other conditions. For instance, dimensions of militarism (expenditures and periods of war) bite more deeply into the GA purse and receiving population than for the AFDC counterparts. In the concluding discussion we suggest some plausible interpretations for these differences in stability.

Aggregate benefit levels.—An additional related question can be asked. If, during the postwar years, black insurgency resulted in the expansion of relief-program expenditures and rolls, what was the consequence for the average aggregate benefit levels of these two major programs? In other words, did the riots, in addition to contributing to the breadth of assistance,

also increase real average benefit levels? If Piven and Cloward (1971) are correct, we should anticipate that the urban riots had no effect on changes in AFDC benefit levels. Although they do not discuss General Assistance benefits, our previous results demonstrating larger expansive effects of the riots for the rolls (see table 4) than for expenditures (see table 3) also suggest that the riots probably had little or no influence on GA benefits. The results shown in table 5 address these questions. Estimates of the effects of the predetermined variables already examined for expenditure and roll categories are presented for annual changes in real average monthly benefits—per family for AFDC and per recipient for GA.

These results suggest, rather unambiguously, that neither the number of riots nor their severity was important in increasing the benefit levels for either program. Instead, AFDC program benefits appear to be upgraded primarily as a result of Democratic party executives and reduced during periods of war. Interestingly enough, precisely the opposite accounts for the changes in GA benefit levels.³¹ One interpretation for these differential

TABLE 5
OLS ESTIMATES FOR LAGGED DEPENDENT VARIABLE EQUATIONS
OF CHANGES IN AFDC AND GENERAL ASSISTANCE
BENEFITS, UNITED STATES, 1947-76

PREDETERMINED VARIABLES	AVERAGE MONTHLY BENEFIT/UNIT EQUATIONS			
	AFDC Families	AFDC Families	GA Recipients	GA Recipients
Y_{t-1}^a	1.021***	1.087***	.732***	.657***
	1.060	1.129	.689	.618
GNP/ C_{t-1}	-.003	-.004	.010	.014*
	-.097	-.139	.283	.389
Party $_{t-1}$	3.874*	3.607*	-5.457	-5.855
	.087	.081	-.110	-.118
Military s0001*	.0001*	-.0002	-.0002
	.085	.089	-.141	-.143
War $_t$	-4.069**	-3.968**	5.564	6.946*
	-.092	-.089	.113	.141
% Unem $_{t-1}$	-.632	-.921	.402	.006
	-.035	-.052	.020	.0003
Frequency $_{t-1}$124419	...
	.010		.029	
Severity $_{t-1}$	-.247	...	-.499
		-.042		-.077
α^b	5.886	3.312	-11.525	-18.701
R^{2c}973	.974	.899	.902
D-W.....	1.852	1.972	2.249	2.163
τ^d	14	14	11	13
t^e	4.09	1.45	-1.91	.60
$\hat{\rho}^f$15	.17	.01	.12

NOTE.—See notes for table 2.

³¹ The increase in GA benefits during war years, however, is probably due to the substantially larger reduction in recipients than in aggregate expenditure levels. Likewise

party effects may rest with the more or less traditional positions of the parties concerning the locus of control and burden for relief programs.

The riots, then, appear to have served in some degree as an extensive force for change in U.S. relief policy, benefiting primarily those who were not already on relief rolls. Institutionalized or party politics, on the other hand, appear to have played a more selective role, shifting benefits from one program to the other and thereby probably producing differential benefits directly for those already on particular rolls and possibly affecting other constituencies indirectly. In short, racial insurgency was beneficial for those without existing welfare-institution access, while electoral politics played a more important role for those who had already acquired such access.

In addition to the direct influences of party and insurgent politics, it seems reasonable to expect that state managers of the Democratic party would respond more often or intensely to insurgency than their Republican counterparts. For example, our variable for party in the executive office captures the presence of the periods in which the Keynesian coalition (see Gold 1977) had its greatest impact on U.S. economic and social policy (i.e., the Truman, Kennedy, and Johnson years). Though not presented here, all tests for possible interactions between our party and insurgency variables produced no clear evidence that the Democratic party responded to the riots more intensely than Republican administrations. This finding, however, must be tempered by two additional points (one methodological and the other substantive): (a) the time frame observed is relatively short, with most riot activity occurring in a rather concentrated historical period; and (b) the majority (70%) of major legislative amendments to the Social Security Act of 1935 during our time frame were passed when the strong Keynesian coalition presidents were in office (e.g., see Stevens 1970). The tentative point then is that both institutionalized (party) and insurgent (riot) politics had their own separate but not clearly interactive effects on these characteristics of relief policy. However, particular administration policies (e.g., strength of the "law and order" platforms) probably have been and can be differentially conducive to the emergence of various insurgent politics (e.g., in terms of altering the costs associated with mobilization).

Our evidence ran contrary to expectations concerning the two specific subsidiary hypotheses presented earlier. First, riot frequency was consistently more efficacious in expanding relief programs than the severity of the riots. Second, the expectation that centrally controlled programs would be more riot elastic than those more decentralized in control met with virtually no support. This conclusion holds irrespective of whether the stan-

for the positive per capita income effect, which appears to reduce the rolls more than expand the purse. These points are inferred from results not all of which were previously presented here.

standardized coefficients or partial elasticities (not presented here) are relied upon. Some interpretations and implications of these unexpected results are discussed in the concluding section.

Finally, we have pursued this analysis by stressing the importance of focusing on detailed programs and their characteristics. That such programs were not found, in general, to respond homogeneously to the central economic and political processes examined here indicates the importance of avoiding gross composite categories of state activity ("public expenditures," "welfare expenditures," etc.).

SUMMARY AND CONCLUSIONS

Welfare institutions and relief giving are integral features of the history of capitalism. State relief dates back at least to the early 16th century and, some have argued, has not changed drastically in broad outline (e.g., Piven and Cloward 1971; George 1973; O'Connor 1973). This, of course, is not to deny that welfare programs have shown increasing secular trends and fluctuated from time to time in the history of early or late capitalism. Indeed, the attempt to explain short-term fluctuations in the characteristics of several major relief programs in the context of the racial insurgencies of advanced American capitalism has been the central focus of this paper.

The theoretical warrant for such an analysis rests with the differing conceptions of the state, class, and institutional structures which various perspectives assume. For orthodox or liberal theory, relief institutions respond to the general problems of industrial society. When "capacity" and "need" expand, both consequences of industrialization and economic development, then too will relief-giving institutions (e.g., Wilensky and Lebeaux 1965; Wilensky 1975). Although not explicitly stated, such arguments make crucial assumptions about the nature of the state—the institution or set of institutions charged with ultimate relief management. The state in its capacity of "neutral arbiter" is thought to respond rather automatically to the "growth" and "need" signals.

Alternatively, conflict- and Marxist-oriented theorists have been led to different theoretical explanations for the behavior of welfare or relief institutions, for secular trend as well as short-term cyclic fluctuations. Again, the fundamental axis is the nature of the state. The trend in the growth of welfare (indeed, the state itself) is seen by some as a logical outcome of the growth of the monopoly sector of the economy (O'Connor 1973). Short-term fluctuations are thought to depend more on the political business cycle or electoral politics and also the immediate threats of insurgents. In all cases, the fundamental function of welfare institutions is to aid in the maintenance or production of harmony in the face of deepening capitalist contradictions (e.g., uneven development). In other words, welfare or re-

lief is a major institutional manifestation of the state's attempt at legitimation, while policies which directly or indirectly assist capital or reproduce the class structure are referred to as part of the state's function regarding accumulation. Policy directed toward legitimation, at least on a large scale, is implemented most often when it must be—when the state, class, and institutional order are sufficiently threatened (e.g., Piven and Cloward 1971). This line of radical thought on the role of welfare institutions usually begins with Piven and Cloward's (1971) classic statement which focused heavily on labor insurgency during the Great Depression (but see Massad 1980) and the emergence of racial insurgency during the postwar decades. We have focused exclusively on a particular form of racial insurgency—the urban riots—during the latter period. The central objective has been a modest attempt at adjudication between some central elements of the orthodox and radical approaches to relief institutions in late capitalism.

We have argued, however, that both orthodox and radical theorists have maintained stronger positions than the evidence has previously been capable of supporting. Orthodox theorists typically do not question the nature or role of the state in attempting to understand components of the "public" expenditure process, including relief. Nor are they fond of positing forms of class conflict or insurgency as central in their explanations of state policy or expenditures. Marxist-oriented writers, on the other hand, have typically been highly concerned with questions surrounding the state, albeit with varying degrees of sophistication. However, many of the latter have often merely asserted the centrality of insurgency in explanations of the performance of relief or welfare institutions in capitalist society.

Several major limitations were identified in the existing empirical literature: (1) most important and pervasive is the virtual neglect of the role of insurgency or any form of noninstitutional politics;³² furthermore, in the studies which have attempted to focus empirically on the role of insurgency (usually riots and welfare in recent U.S. history), there are the shortcomings of (2) not directly relating riots to welfare-institution changes, (3) employing riot frequency and ignoring severity, and (4) failure to control adequately for other important determinants of relief fluctuations.

We have attempted to explicate some typically ignored theoretical issues and to redress some of these deficiencies in the empirical research purporting to assess the determinants of welfare changes. Our primary conclusion is that racial insurgency in the form of urban riots did, in fact, have important expansive expenditure and roll consequences (in the aggregate) for

³² In a substantial proportion of the empirical literature on all forms of state expenditures, one is hard pressed to find reference to any political variables, much less forms of noninstitutionalized politics. The public finance research contains many examples of this type of work (e.g., see the *Public Finance Quarterly*).

several major relief programs examined for the postwar United States. This rather pervasive finding in our analysis appears to be the strongest empirical support for this part of the Piven and Cloward thesis yet presented, including their own evidence. Running contrary to our initial expectations, however, was evidence which indicated (1) no substantial tendency for more centrally controlled programs to be more riot elastic than those locally controlled, and (2) greater efficacy of riot frequency than of severity in producing relief changes.³³

The major implications of our findings are at least fourfold. First, orthodox theoretical arguments purporting to explain short-term fluctuations in some forms of state welfare or relief activity independent of forms of non-institutionalized politics (insurgency) appear to be, at least in part, incorrect. For virtually every aspect of the relief programs examined here, except AFDC and GA benefit level changes, the urban riots had a nontrivial expansive influence. This, of course, does not indicate that all insurgencies produce nontrivial or expansive relief policies. The option of ignoring insurgent movements is one obviously available to the state along with overt repression (see Piven and Cloward 1977). Nor should our arguments be read as suggesting that other processes play no role in producing relief fluctuations. Our results are, in some respects, also consistent with orthodox expectations concerning the importance of "capacity" (e.g., per capita income) and "demand" (e.g., unemployment) influences.³⁴ Does this imply that the "correct" theoretical explanation is merely a simple composite of the two major perspectives? As we suggest below, we are not inclined to believe so.

Second, the finding that black insurgency was linked to short-term relief gains raises serious questions concerning the liberal assumptions about the nature and role of the state underlying orthodox theory. If we are correct that the developmental/modernization view of welfare-institution behavior embodies a liberal or pluralist view of the state and American polity, we see nontrivial insurgency influences on state welfare policy as incompatible with that set of assumptions (see Gamson 1975). The imagery of the liberal state does not necessitate such overt conflict to produce responsiveness, and the pluralist assumptions imply that such state action would

³³ It should be noted that these last two points are contrary to what we concluded (on these points) in an earlier paper which reported our preliminary examination of these issues employing a shorter (right-censored) series (i.e., 1948-70; in Isaac and Kelly 1979).

³⁴ Perhaps more important with respect to actually observing some such "effects" posited by orthodox theory (and also frequently reported) is the theoretical interpretation of what such effects may actually signify. A prime example would be the effect of per capita income growth which might, at least in part, represent the growth in monopoly capital and consequent surplus population (e.g., O'Connor 1973) as well as evidence that public expenditures tend to be primarily income elastic.

not necessitate such violations of the game (i.e., entry into the polity and influence gathering), at least not with action which might be considered beneficial to the welfare of insurgents. According to the logic of pluralism, such irrational behavior as rioting begets negative consequences for individuals and groups who employ it, not the expansion of institutions which may augment the insurgent population's well-being in the short term, even if we grant political co-optation as a possible by-product. As Piven and Cloward (1971) have argued, the well-being of powerless groups has never been the primary focus of welfare policy in capitalist society; instead, its primary focus has been to regulate the threat which large numbers of relatively powerless people may be capable of generating under certain historical conditions. As we noted earlier, insurgency had a more general or extensive influence across programs—that is, in terms of expanding coverage. Party politics (at least in the manner in which we measure it) had a more selective influence in differentially altering benefit levels of the AFDC and GA programs. Therefore, we are not concluding that institutionalized or electoral politics are totally unimportant for relatively powerless groups. Instead, optimal strategies for change beneficial to such groups are likely to necessitate reliance on a combination of both (see Alford and Friedland 1975).

Third, the relative lack of support for our expectation that riot severity would have more influence than riot frequency may be grounds for questioning the logic of "violence as a constraint resource" which motivated the hypothesis. Yet there are good reasons for not making that case. For example, in our city-level data the severity effect, though extremely small, was in fact larger than the frequency effect. But more important, the riot dimensions over this time period were all highly correlated—part and parcel of the same historical process. Stronger evidence on the question of the constraint value of the politics of violence might require a comparison of strictly nonviolent protest with the events employed in the present analysis. Good examples relevant to the present historical period might include the events which made up the beginnings of black insurgency in the post-war United States—lunch-counter sit-ins, boycotts, and peaceful demonstrations. The incidence of such tactics was higher during the earlier half of the period examined here and tended to decline as the urban riots escalated (e.g., see Burstein's [1979] data). Rigorous comparative analyses of these different forms of black insurgency might well produce evidence consistent with our initial expectation and that of the resource mobilization/management theories of collective political action consequences (e.g., Gamson 1975; Snyder and Kelly 1979).³⁵

³⁵ An alternative interpretation of the more efficacious frequency effect, based on the resource-management framework, might be that frequent (and perhaps widespread) riots have served as a more constant reminder of the lack of legitimacy surrounding the

Fourth, focusing on the riot-welfare nexus from the national vantage point has produced new evidence and insights. However, our expectation that centrally controlled (federal) programs would be more riot elastic than local and state-level counterparts was not consistently supported. Elements of an explanation may reside in the fact that the federal welfare bureaucracy is comparatively more autonomous from external pressures and expands more in accordance with internal bureaucratic imperatives of the state managers (see Block 1977). Moreover, the greater stability (or bureaucratic momentum) in the federal level expenditure process probably results in part from differential stability in revenue-generating mechanisms across governmental levels. Other plausible, and related, explanations may reside in the emerging population-ecology perspective on organizational growth (e.g., Nielsen and Hannan 1977). The stability coefficients (autoregressive slopes) involved in the AFDC as compared with the GA equations (tables 3 and 4 above) are at least consistent with these notions. In short, the theory of incrementalism—so central in public finance and political science (e.g., see Wildavsky 1975; Cameron 1978)—tends to best fit the federal level relief programs. But even there it is only a partial explanation at best.

Finally, in reviewing our own effort, we believe this investigation significant not only in the conclusions we have presented but also, and perhaps more important, in suggesting an agenda for (1) examining the political economy of state action (policy) and thereby (2) beginning to contribute empirically to our understanding of the role and nature of the late capitalist state. There are, of course, substantial limitations of the present study, which may serve as useful points of departure in addressing related questions. Perhaps most important in this regard is our neglect of other aspects of the class-struggle arguments of state expansion (see O'Connor 1973; Block 1977), other components and programs of the state welfare apparatus, and elements of Piven and Cloward's (1971, 1977) rich historical work—especially the insurgency-regulation hypothesis and the role of electoral instability (see Piven and Cloward 1979).

The strength of our results derives from the comparison of central theoretical relationships across different programs and units of analysis, but admittedly most heavily from the national aggregate time series. Additional effort could be applied to questions which might be empirically amenable only at the city or state level. For instance, the density of class forces in such areal or geopolitical units might condition the process of interest in important ways (e.g., see Friedland 1976). Subsequent analyses might also

state and institutional structure. In this sense, given a minimal severity level (which we in fact impose on our definition of the events employed here), the more consistent (or widespread) insurgent actions are, the greater the constraint value of the "politics of turmoil."

allow consideration of other versions of the orthodox perspective (e.g., see Grønbjerg 1977) which we were unable to examine in detail, as well as more concrete aspects of uneven development and the important theoretical linkage between the growth of monopoly capital and the state (see O'Connor 1973).

Perhaps most important, we have begun to look seriously at insurgency as one form of force (or input) affecting the development of state action via specific relief programs. But the frameworks beginning to take form under the rubric of the "class-struggle" perspective of the state hold out substantially more promise consistent with this line of inquiry (see Esping-Anderson et al. 1976; Block 1977; Poulantzas 1978) and suggest lines of analysis which we have ignored here. If one takes the explication and empirical assessment of the linkages of the class structure to the state in late capitalism as a major theoretical issue, it is important to focus more heavily on (a) the idea of programs and departments embedded in the state bureaucracy as representing "a condensation of the relationship of class forces" (Poulantzas 1978) and (b) the material consequences of various induced policies (e.g., who gains and who loses?). Depicting the forces which drive relief policy and the consequences thereof is likely to be important for ultimate understanding of the nature and role of the late capitalist state.

APPENDIX

Variables and Data Sources

I. City data

A. Welfare expenditures per capita. Total welfare expenditures. Observations for 1960 and 1969-70 were obtained from *The County and City Databook* (1962, table 5, and 1972, table 6, respectively). Total per capita expenditures for 1960 are also from this source.

B. Poverty. The percentage of families with less than \$3,000 annual income in 1960. From the Alford-Aiken governmental units analysis data file, originally coded from the 1960 U.S. Census.

C. City governmental form. Dummy variable for reform (= 0) and non-reform (= 1) city administrations. From the Alford-Aiken governmental units analysis data file.

D. Number nonwhite. Size of the nonwhite population in 1960. From the 1960 U.S. Census.

E. South. Constructed dummy variable for city location: 1 = cities located in border or deep-South states; 0 = non-South.

F. Riot frequency and severity. Riot variables—number of riots, number of injuries in riots, number of participants in riots, number killed in riots,

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and duration of riots in days—were coded for 1965–68 from the Lemberg Center's *Riot Data Review* (May 1968 and August 1968); the *Congressional Quarterly's Civil Disorder Chronology* (September 1967); and the Committee on Government Operations, Permanent Subcommittee on Investigations' *Staff Study of Major Riots and Civil Disorders* (1965 through July 31, 1968). Data for 1961–64 were coded from the *New York Times* and the *Los Angeles Times*.

II. National data

A. Federal public assistance. Total aggregate annual federal expenditures on public assistance programs: U.S. Bureau of the Census (1975, ser. H347; 1977). Adjusted to 1967 dollars (millions) by Consumer Price Index, U.S. Bureau of the Census (1975, ser. E135; 1977).

B. State public assistance. Total aggregate annual state expenditures on public assistance programs: U.S. Bureau of the Census (1975, ser. H348; 1977). Adjusted to 1967 dollars (millions) by the Consumer Price Index, U.S. Bureau of the Census (1975, 1977).

C. Local public assistance. Total aggregate annual local expenditures on public assistance programs: U.S. Bureau of the Census (1975, ser. H349; 1977). Adjusted to 1967 dollars (millions) by the Consumer Price Index, U.S. Bureau of the Census (1975, 1977).

D. Aid to Families with Dependent Children (AFDC) expenditures. U.S. Bureau of the Census (1975, ser. H353; 1977). Adjusted to 1967 dollars (millions).

E. General Assistance (GA) expenditures. U.S. Bureau of the Census (1975, ser. H354; 1977). Adjusted to 1967 dollars (millions).

F. AFDC families. Number of families receiving AFDC benefits (in thousands): U.S. Bureau of the Census (1975, ser. H358; 1977).

G. GA recipients. Number of individuals receiving GA benefits (in thousands): U.S. Bureau of the Census (1975, ser. H361; 1977).

H. Average monthly benefits per AFDC family. U.S. Bureau of the Census (1975, ser. H365; 1977). Adjusted to 1967 dollars.

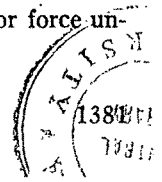
I. Average monthly benefits per GA recipient. U.S. Bureau of the Census (1975, ser. H367; 1977). Adjusted to 1967 dollars.

J. Gross national product per capita. U.S. Bureau of the Census (1975, ser. F2; 1977). Adjusted to 1967 dollars by the Consumer Price Index.

K. Party. Party of the executive in office constructed as a dummy variable: 1 = Democrat, 0 = Republican. U.S. Bureau of the Census (1975, ser. Y210).

L. Military expenditures. U.S. Bureau of the Census (1975, ser. Y458–460; 1977). Adjusted to 1967 dollars (millions).

M. Unemployment rate. Percentage of the total civilian labor force unemployed: U.S. Bureau of the Census (1975, ser. D87; 1977).



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N. Riot frequency and severity. For 1961–69 sources are the same as reported for city-level measures, but have been aggregated up to form national annual series. The yearly observations for 1947–60 and 1970–76 were coded from the *New York Times*.

O. War. Constructed dummy variable for war years (= 1).

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Community Power and Population Increase: An Empirical Test of the Growth Machine Model¹

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Attempts to relate community power to policy outputs have been criticized for lacking theoretical relevance and statistical significance. To overcome these criticisms, this research employs a power-policy model based on a political economy framework which utilizes Molotch's conception of the community as a growth machine. Analysis of the Permanent Community Sample provides strong power-policy relationships that support the major tenets of the growth machine explanation: As members of the business elite gain power, they can successfully promote population growth that provides them with greater profits through higher property values. The most widely cited common good for growth, lower unemployment, does not accrue. This supports the relevance of a political economy approach to community phenomena and suggests different directions for community power research.

Early community power research has produced hundreds of descriptions of the nature and distribution of local power. For most of these studies the description was an end in itself, but more recently it has been felt that the differences themselves must also make a difference (Warren 1977). That is, variations in community power should help account for variations in local policy outputs. Numerous attempts have been made to link power with policy, but the theoretical base often seems devoid of implications for political change and the statistical relationships usually range from weak to nonexistent. These twin problems of theoretical relevance and statistical significance represent major difficulties in the development and perhaps even the continuance of the subfield of community power as a major avenue of academic inquiry.

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As an example, note Roland Warren's (1977, p. 368) gloomy summation of community power research: "The exciting study of community power structures opened up in the 1950s has run a rather full course, and the ratio of firm conclusions to energy expended leaves the burden of proof on its proponents as to its explanatory value for local community phenomena." In a similar vein, John Walton's (1976, p. 299) dismal assessment of the field emphasizes the lack of concern with practical politics: "Correlations between structural or demographic characteristics of cities and the sheer quantitative resultant of tangled bureaucratic processes represented by levels of spending or program adoption are abstractions far removed from the stuff of politics. I believe it is for this reason that many have become dissatisfied with the directions the field has taken. As research became more technically sophisticated, it drifted further from its conceptual mooring to the notions of power, inequality, and the workings of the political process." Even those who have produced the correlations Walton criticizes are less than satisfied with the results. Surveying the weak statistical associations he found between community power and policy outputs, Aiken (1970, p. 516) concludes: "It seems apparent from the results here that the community power perspective, as it now exists, is simply not the most appropriate model to use." As a final example, Lyon (1977a, p. 429) asks, "Aren't we really saying that the direct measurement of community power is not worth the effort and that community power can contribute little toward explaining and predicting local policy outputs?"

The remarks above should suffice to illustrate that traditional community power research appears to many to be in need of new directions. Specifically, they suggest that what is needed is a theoretical model that is politically based and change oriented and that addresses the question of "who benefits" from community policies (Walton 1976, 1979). Furthermore, this theory or model should be able to produce a measurable power-policy relationship that is statistically significant and relatively important when compared with competing explanatory variables (Aiken 1970, Lyon 1977a). This paper attempts to employ such a theory of community power in order to build an empirical power-policy model that meets these requirements.

THE GROWTH MACHINE AS A THEORETICAL BASE FOR COMMUNITY POWER

In a provocative article on the structure and dynamics of community power, Harvey Molotch (1976, p. 313) argues that the key to understanding local politics is to conceptualize the city as a growth machine: "I aim

to make the extreme statement that this organized effort to affect the outcome of growth distribution is the essence of local government as a dynamic political force. It is not the only function of government, but it is the key one and, ironically, the one most ignored. . . . This is the politics which determines who, *in material terms*, gets what, where, and how."

Briefly, it is Molotch's (1976, pp. 314-15) position that community elites such as local businessmen form a coalition devoted to promoting growth because it provides them with a number of tangible economic advantages (e.g., increased markets and property values). Although these elites may disagree on other issues, the desire for growth finds wide support so that civic "boosterism" becomes institutionalized through chambers of commerce, newspapers, athletic teams, and so forth. It is maintained that the basic purpose of these local phenomena is to allow the community to compete with other communities for increased growth. However, in a departure from conventional wisdom, Molotch (1976, p. 318) claims that beyond the local elite, very few residents benefit from their community's growth. Tax burdens rise during growth periods; and the commonly expected benefit, a decrease in unemployment rates, does not occur. Thus, the local elite has convinced residents that the quality of life improves with growth when, actually, it deteriorates.

In this description of local growth, Molotch has developed an explanation of community power that meets the requirements specified by Walton. It is based in a political economy framework (Walton 1979) and answers the "who gets what" question. Furthermore, it has potential for change: Molotch (1976, p. 327) argues for antigrowth movements to wrest control from the business elites and describes the improved quality of life that can be expected from such a change. Such characteristics as a political economy framework or a potential for change do not, of course, guarantee the validity of conceptualizing the community as a growth machine. They do, however, indicate that the concept is potentially useful and important for our understanding of community power and, hence, should be subjected to further analytical and empirical investigation.

Since the explanation developed by Molotch is largely of a logical-deductive form, with little supporting empirical evidence, this paper constructs an empirical model to test the key relationship between elites and growth as well as other aspects of the growth machine explanation of community phenomena. If this model provides evidence of a strong statistical relationship between the power of local businessmen and community growth, it will respond positively to the two common criticisms of community power research mentioned above: (1) a lack of theoretical relevance and (2) a relatively weak statistical relationship.

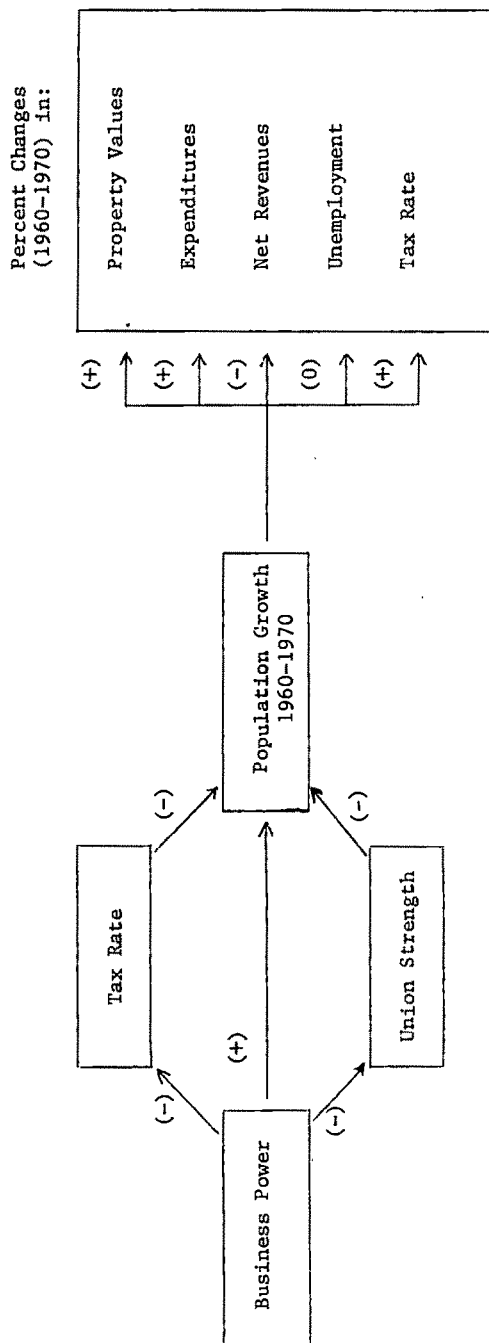


FIG. 1.—The growth machine model. The arrows graph predicted causal links and the signs (+, —, and 0) indicate hypothesized positive, negative, and absent relationships that are based on Molotch's growth machine model and are evaluated in table 1. Other possible causal relationships between these variables that are not explicitly predicted by the model are also evaluated in table 1.

THE GROWTH MACHINE MODEL

The growth machine model will be evaluated with data based on 48 communities from the Permanent Community Sample (PCS).² Clark's (e.g., 1971) intensive study of these communities provides direct measurement of many of the variables in the model. Other, more demographic variables are available from traditional sources such as the U.S. Bureau of the Census.

The model in figure 1 represents much of Molotch's theory with variables for which there is full operationalization (see Appendix) for the PCS. The key variable for our purposes is the power of the business elite. It is directly measured in the PCS by questions asked of knowledgeable informants to gauge the power of businessmen and business organizations

TABLE 1
STANDARDIZED COEFFICIENTS OF THE GROWTH MACHINE MODEL

DEPENDENT VARIABLES	N*	INDEPENDENT VARIABLES						R ² (Adj.)
		Vector I	Vector II	Bus- Pow	Union	Tax- Rate	Pop- Grow	
Business power....	48	A	A00
Union strength....	48	A	A	A00
Tax rate 1960.....	45	.306	A	.432	A21
Population growth.....	45	-.520	A	.400	A	A47
Property values change.....	43	A	A	A	A	.231	.192	.05
Unemployment change.....	45	-.252	-.227	.237	A	A	A	.11
Tax rate change...	43	A	-.233	A	-.244	-.480	A	.29
Expenditures change.....	44	A	A	A	A	A	-.394	.14
Net revenues change.....	43	A	A	A	.309	.425	.362	.30

NOTE.—A = the empirical absence of a theoretically possible causal relationship. All standardized coefficients shown are significant at .05 level with the exception of four betas that allow measurable increase in adjusted R² by their inclusion even though they are not statistically significant (Vector II and BusPow on Unemployment change; and TaxRate and PopGrow on Property values change).

* N's are total number of communities with full information for the relevant variables and therefore available for that segment of the model.

² The most widely used subsample of the PCS (Clark 1971) has an N of 51 cities. Because population growth outside municipal boundaries can obviously benefit elites, if they own property beyond the city limits or seek larger markets for their products and services, the county was chosen as the best possible unit of analysis for testing the growth machine model. Two other units would allow such growth to be measured, but growth rates for the SMSA and urbanized area correlate highly with county growth ($r = .87$ and $.76$, respectively), and the county unit has the advantage of allowing the computation of expenditures, revenues, tax rates, and property values. Three suburban communities from the PCS are omitted because they are all in Los Angeles County and the inclusion of that county would bias results owing to the dominating presence of the city of Los Angeles. The relationships proposed by the growth machine model that can be tested with SMSA and urbanized area data show no substantive differences from those in this paper.

in the community (Clark et al. 1977). While Molotch specifies other elites also as promoters of growth, businessmen are clearly viewed as the most important group (Molotch 1976, p. 314), and the PCS represents the only large community sample with such a direct measure of business power. If the Molotch thesis is correct, there should be a strong, positive relationship between this variable and population growth. This relationship is a pivotal one for answering the question, Does it make a difference "who governs"? According to the model, it should make a substantial difference: the greater the power held by business in the community, the greater the local growth.³

There are other relationships specified in the model. It is predicted that business strength will reduce the tax rate and the power of unions.⁴ The tax rate (or effort, or burden, depending on one's ideology and definition) is a ratio of local governmental revenues to local property values. The higher the revenues and the lower the property values, the higher the tax rate. Union power is measured by an index that estimates the perceived influence of unions in local decision making. In addition, it is expected that population growth will have effects on policy outputs rarely mentioned by growth proponents. As the growth rate increases, the model predicts increases in tax rates and per capita expenditures, a decrease in net revenues (Molotch 1976, p. 319), and no change in unemployment (Molotch 1976, pp. 320-25). Finally, it is expected that growth will increase property values and thereby benefit the elites who promoted it originally (Molotch 1976, p. 311).

There is, of course, a possibility that some of the relationships in the model are spurious and merely reflect basic historical, political, or geographic causes. Thus, the model needs a series of control variables. Unfortunately, with a limited sample size, the number of explanatory variables must necessarily be small in order to conserve degrees of freedom. Thus, 10 potential control variables suggested in the literature are utilized in a principle component analysis (Jöreskog and Sörbom 1979) which results in the creation of two vectors that combine to account for approximately half of the intervariable variance among the 10 control variables. These 10 variables include two measures of informal community power,

³ While there are obviously other types of growth that could be considered besides population growth, e.g., various types of economic growth, Molotch clearly sees population growth as the key variable in the growth machine model.

⁴ Tax rate is measured for 1960 in order to construct a change measure for 1970. However, the surveys that determine business and union strength were conducted in 1967. Thus the model is forced to reverse the causal and temporal order in this one instance. While it would be possible to construct that model with the 1960 tax rate as a cause of business power, the growth machine theory views business power as a cause rather than an effect. In addition, Molotch (1976, p. 312) sees the progrowth "business climate" as characterized by "good" labor relations and "reasonable" taxes. To operationalize them as *weak* unions and *low* taxes seems appropriate.

political decentralization (Clark 1971) and *decision-making structure* (Vanecko 1967; Brick 1975), and one measure of formal political structure, an *index of municipal reform* (Lineberry and Fowler 1967). In addition, five largely demographic variables measured at the county level are utilized: *population*, *percentage nonwhite*, *median age*, *median income*, and *median education*. Finally, a dummy variable of *city/suburb status* and the *age of the city* (Liebert 1976) are employed. Vector I is interpreted as a demographic measure that loads highest on city age and city/suburb status. It appears to reflect many of the basic regional and historical differences among communities described by Liebert (1976) as being associated with "functional scope," by distinguishing between the older, central cities and the newer, suburban cities. Vector II is a political construct that loads on the three formal and informal political variables and separates communities with reformed municipal governments and more elitist power structures from those with less reformed governments and a more pluralistic distribution of power (Lineberry and Fowler 1967; Clark 1971; Lyon 1977b). The exact interpretation of the vectors is not crucial to the present analysis in that they serve mainly to create a *ceteris paribus* situation in which the growth machine explanation can be more adequately assessed.⁵ To the extent that these vectors are important as causal factors in the model, it is predicted that the power of the business elite will compare favorably with them in statistical strength. Otherwise, the aforementioned difficulty of easily measured control variables being more important than measures of community power reasserts itself. It is necessary, then, that the power of the business elite have a significant effect on growth after the control vectors are introduced and that the business elite variable have a causal impact of similar or even greater magnitude than the two control vectors.

TESTING THE MODEL

The relative causal effects of the variables in the growth machine model are exhibited in table 1. The initial relationship in table 1 essentially shows the independence of business and union power from the two community vectors. The R^2 values of zero strongly suggest the absence of significant covariance between these measures of community structure and the power of business and unions.

For the two proposed concomitances of local business power, there is little initial support for the model. It is expected that business will seek

⁵ This is not to suggest that only two vectors can account for "community structure." We cannot even claim that they represent totally the 10 control variables. However, the independence of business power from these 10 variables and the effect of business power on growth still hold when the 10 variables are entered separately as individual controls. More detailed information about the creation and interpretation of these vectors can be found in Lyon et al. (1979).

weak labor unions and low taxes in order to develop an overall economic climate conducive to profit and growth. As indicated in table 1, business power is not empirically related to the measure of labor power, perhaps because the union variable is a measure of local union influence rather than unionization.⁶ Business strength shows a strong positive relationship with local tax levels. A possible explanation of this disparate finding focuses on the aggregate nature of the tax measure used. It represents the total tax burden of the community, and to the degree that business is able to secure special advantages through lower or delayed taxation, investment tax credits, and so forth, the measure may be inappropriate for an accurate test. In any event, the lack of a measurable relationship between business and labor and the positive association for business and taxation are not as predicted by the model.

For the key dependent variable in the model, population growth, the observed relationship conforms closely to the Molotch predictions, that is, the greater the power of business in the community, the higher the levels of local growth. Moreover, this relationship is a strong one that compares favorably in explanatory power with the community structure vectors. Thus, it appears that the level of business strength within the local elite is a substantial force in creating community growth and that this relationship is largely independent of many other elements of community structure.

The consequences of growth and business power on subsequent community phenomena are generally as predicted. For increase in property values, population growth exercises a weak but positive effect. According to the theoretical explanation of the model, this is an expected relationship because increased property values are one of the primary benefits the business elite receives from growth.

Another local output important to the model is the unemployment level. Molotch maintains that elites will attempt to convince the citizenry that lower unemployment levels result from growth; but as Molotch (1976, pp. 320-25) demonstrated, there appears to be no relationship between growth and unemployment. There is, however, a moderate link between business power and increases in unemployment.

The relationships between growth, taxation, and other financial outputs are complex and not always as predicted in the model. There is no relationship between increases in population and increases in the local tax burden. And while growth may not reduce the tax effort or burden, it appears to reduce expenditures significantly in that, for per capita expenditures, the relationship with growth is strong and negative. This suggests that population increases more rapidly than expenditures during growth periods. Furthermore, when the net revenues (total revenues less local expendi-

⁶ In fact, when business power is correlated with the presence of municipal labor unions, there is the expected negative relationship (Lyon et al. 1979).

tures) are considered, the effect of growth is positive. This contradicts Molotch's contentions of higher taxes, higher expenditures, and a net revenue loss for growing communities and implies generally favorable consequences for population growth.⁷

In sum, however, this analysis of the PCS still supports the major tenets of the growth machine model: as business elites gain local power, they are able to promote population growth which appears to provide them with greater profits through higher property values; but the most widely cited common good for growth, lower unemployment, does not accrue.

IMPLICATIONS

The foregoing test of the growth machine model has significant implications for (1) understanding the dynamics of local growth, (2) using a political economy framework to account for community phenomena, and (3) assessing the current state of community power research. With respect to local growth, it seems clear that Molotch's description and explanation of the growth process merit serious consideration. To the degree that the parts of the theory that were testable are supported in this analysis, portions less suited to empirical evaluation (e.g., the economic motivation for elite control and manipulation of community events) can be given at least some indirect support. And if local growth can be seen as a phenomenon that is promoted and caused by a special elite, this elite influence must be considered in order to understand or control the growth process.

There are some growth dynamics in this empirical model that are not anticipated by Molotch's theoretical model. The relationship between business power and taxation seems to be considerably different from the Molotch explanation. It may be that business requires high levels of government services and thereby requires high local taxes. In any event, this relationship needs further investigation and refinement. Similarly, the effects of growth require considerably more investigation and analysis than exist to date. While many of the consequences of growth are as Molotch predicts, others, such as the change in taxation and net revenues, are not. These differences do not necessarily invalidate the growth machine model because they are not as crucial to it as the business-growth relationship. In addi-

⁷ A concept as broad as "net revenues" is admittedly difficult to operationalize. However, we feel relatively confident in the positive relationship between growth and net revenues because six different measures were initially constructed using various combinations of several revenue and expenditure variables, and the five not shown in the text supported the positive association more strongly than the one used here (which seems best to represent the concept as used by Molotch). The relationship is weakened by the method used here because that revenue measure includes intergovernmental revenues, and rapidly growing communities received proportionately smaller funds from the federal government. Measures based only on local revenues show a stronger relationship.

tion, these output factors can be more adequately analyzed with samples larger than the PCS.⁸ To assess the effects of local growth, it is not necessary to measure or control for community power. Thus, analysis based on larger samples is possible and desirable (e.g., Greenwood 1975; Appelbaum 1976). It is only for understanding the causes of growth that community surveys of elite characteristics appear necessary; hence, samples must be limited in size.

The empirical model developed in this paper gives support to Molotch's and Walton's (and others', e.g., Harvey 1973; Castells 1977) position that a political economy approach is appropriate for understanding community phenomena. In this case, the contention is predicated on the proposition that the desire to increase the profit potential of local property is sufficient to unify the elite and that fostering community growth is a common and effective technique for increasing property value. Although this proposition is not completely addressed by the empirical model developed in this paper, the basic relationship of business, growth, and increased property values suggests the appropriateness of the model for community sociology. In other words, it appears that much can be gained by conceiving of a community "as the areal expression of the interests of some land-based elite" (Molotch 1976, p. 309). And in a more general context, this model shows support for Walton's (1979, pp. 141, 142) contentions that class control "predict[s] or explain[s] in some considerable part the pattern of urban development" and that "the [political economy] perspective does offer theoretical explanations amenable to empirical evaluation."

Finally, the model tested here says much about the current state of community power research. The findings reported in this analysis suggest that attempts to correlate the distribution of power with traditional policy outputs such as local expenditures may be ill conceived for two reasons. First, in many major comparative efforts the measure of community power is based on the distribution of power rather than on who has the power and what they can be expected to do with it. Although distribution is of immense theoretical importance, in terms of pragmatic import, the characteristics of the power holders seem more crucial. In this case, who holds the power (i.e., businessmen) has a much greater effect on growth than does

⁸ E.g., there is some intercorrelation in the output variables. The strongest intercorrelations occur when change in net revenues is correlated with change in tax revenues ($r = -.47$) and in expenditures ($r = -.53$). Individually controlling for these correlated output variables would produce two substantive changes in our conclusions, both of which would provide greater support for Molotch's model. Specifically, (1) the positive relationship between population growth and net revenues disappears, and (2) population growth is shown to increase the tax rate. Such controls are questionable, however, because the correct causal ordering is difficult to specify, and they use up degrees of freedom and sometimes reduce sample size. More rigorous specification and elaboration of the effects of growth can be made best with significantly larger samples.

the distribution of power (i.e., decentralization).⁹ The characteristics of power holders have been measured in other comparative community research (e.g., Clark et al. 1977); and from this analysis, it now appears that for explaining important community phenomena such as growth, the characteristics of power holders are more crucial than the distribution of power.

The idea that some community phenomena are more important than others suggests a second reason for the difficult position in which community power research currently finds itself. It is probably incorrect to think that the typical community output variables, such as expenditures for one function or another, are closely tied to elite interests. For example, Molotch maintains that growth is the most important community output and the only one that unites local elites. Thus, it is an output that should be affected by elite characteristics, and the model shows this to be the case. However, if an output is not an important concern of the elite, there is no necessary relationship between power and policy.

The idea that local elites are interested in only a small number of community issues is not a new one (e.g., Hunter 1953). Clearly, growth is an issue that concerns local elites, as shown by Molotch's theory and this empirical test. An important question is whether there are other issues that are equally important to local elites, and that will unify these elites in pursuit of a common local output. We believe only issues that are both extremely important and virtually consensual can be theoretically justified and empirically supported in a power-policy model to the same degree as population growth is here. It is unclear whether there are such issues, but it is now clear what is necessary to demonstrate their existence. The issue and output should be suggested by a well-developed theory (e.g., Molotch's growth machine explanation) and be supported as significant in an empirical model. The growth machine model defines the key leadership characteristics, the important issue and output, and explicitly specifies the relationships between them. It is not surprising, then, that this is one of the few empirical studies to show community power as an important cause of community phenomena.¹⁰ The implication for community power research, finally, is much like Walton's earlier prescription. There should be less initial emphasis on quantification and more on the development of sound quali-

⁹ Although it is not shown in the text, Clark's (1971) decentralization measure has no relationship with business power ($r = -.06$) and never develops a significant beta with growth under a number of different specifications in the model.

¹⁰ Because community power has so rarely been found to be causally important, the exceptions are notable. An important exception is Clark et al.'s (1977) finding that cities with strong mayors and weak businessmen are more likely to have fiscal difficulties. Another type of analysis that may prove to be an exception by raising the explanatory levels of power-policy models is the increasingly popular policy typology approach (Aiken 1970; Clark 1972; Ostrum 1974; Smith 1976; Peterson 1979).

tative theory. Given powerful theories to build on, more powerful statistical relationships can be expected to follow; and this much-maligned subfield may finally achieve the potential it held in the 1950s and 1960s.

APPENDIX

Operational Definitions of Variables

Business power.—It is a reputational assessment of business influence by 11 "knowledgeable informants" in each community for four past issues: urban renewal, mayoral elections, air pollution, and antipoverty activities. Clark et al. (1977) provide the PCS scores for this variable under the heading of "DCINBUS."

Union power.—Based on the responses of seven "knowledgeable informants" in each community, this measure reflects the perceived importance of labor unions in local decision making. The variable is in the Inter-University Consortium for Political and Social Research (ICPSR) data set (no. 0025) originally compiled under Terry Clark. The ICPSR index is reversed here so that higher scores will indicate greater influence.

Tax rate and tax rate change.—Tax rate is the ratio of local county expenditures to property values based on the *Census of Governments: Compendium of Government Finances and Taxable Property Values*, 1962, 1972 (U.S. Bureau of the Census 1964a, 1964d, 1974a). The ratio is constructed as:

$$\frac{\text{general revenue} - \text{intergovernmental revenue}}{\left(\frac{\text{assessed value of real property}}{\text{sales-based ratio}} \right)}$$

Tax rate change is computed as follows:

$$\frac{1970 \text{ tax rate} - 1960 \text{ tax rate}}{1960 \text{ tax rate}}$$

Population growth.—The percentage increase in county population from 1960 to 1970 is based on data from the *Census of Population*, 1970 (U.S. Bureau of the Census 1972).

Property values change.—The change in property values is measured by combining county variables from the *Census of Governments: Finances of Municipalities and Township Governments*, 1962, 1972 (U.S. Bureau of the Census 1964b, 1974b) in the form shown below:

$$\frac{\left(\frac{\text{assessed property value, 1960}}{\text{sales-based ratio, 1960}} \right) - \left(\frac{\text{assessed property value, 1970}}{\text{sales-based ratio, 1970}} \right)}{\left(\frac{\text{assessed property value, 1960}}{\text{sales-based ratio, 1960}} \right)}$$

Unemployment change.—Based on the county data in the *Census of*

Population: Characteristics of the Population, 1960, 1970 (U.S. Bureau of the Census 1961, 1972), the change in unemployment levels is estimated by:

$$\frac{\text{unemployment rate, 1970} - \text{unemployment rate, 1960}}{1960 \text{ unemployment rate}}$$

Expenditures change.—The change in per capita expenditure is based on county data from *Census of Governments* (1962, 1972) and is measured as follows:

$$\frac{\text{expenditures per capita, 1970} - \text{expenditures per capita, 1960}}{\text{expenditures per capita, 1960}}$$

Net revenues change.—Based on the county data from the *Census of Governments: Local Government in Metropolitan Areas*, 1962 (U.S. Bureau of the Census 1964c) and the *Census of Governments: Compendium of Government Finances*, 1972 (U.S. Bureau of the Census 1974a), the change in net revenues is estimated by comparing revenues with expenditures:

$$\left(\frac{\text{general revenues per capita, 1970} - \text{general expenditures per capita, 1970}}{\text{general revenues per capita, 1960} - \text{general expenditures per capita, 1960}} \right)$$

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

LEARNING, SOCIAL LEARNING, AND SOCIOCULTURAL EVOLUTION: A COMMENT ON LANGTON

In "Darwinism and the Behavioral Theory of Sociocultural Evolution: An Analysis" (*AJS* 85 [September 1979]: 288-309), John Langton suggests that processes of transmission, variation, and selection, essentially analogous to those of organic evolution, occur in the social world. The same suggestion about the process of sociocultural change has been made repeatedly in recent years (Ackermann 1970; Aldrich 1979; Burian 1977; Blute 1976, 1979; Boyd and Richerson 1976; Campbell 1965, 1970, 1974; Cloak 1975, 1976; Dawkins 1976; Hannan and Freeman 1977; Hull 1978; Lenski 1970; McKelvey 1978; Richerson 1977; Richerson and Boyd 1978; Ruyle 1973; Ruyle et al. 1977; and Toulmin 1972). While emphatically in favor of the spirit of Langton's suggestions, I would like to take issue with one of its fundamental features.

The greatest weakness in Langton's analysis is a confusion between elementary learning processes—sensitization, habituation, classical conditioning, and instrumental or operant conditioning (emphasis is placed on the latter)—and social learning, or at least social learning in the strict sense involving modeling—learning by observation or by verbally or otherwise encoded instruction. In the former case, a behavior is repeated or a rule is repeatedly applied in the life history of a single individual under the pressure of differential reinforcement. In the latter, a behavior or a rule is transmitted from individual to individual in a social group, pre-

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sumably under the pressure of sociocultural selection. At two points in his analysis, Langton indicates some awareness of this difference (pp. 294, 308), but everywhere else he confounds the two, repeatedly identifying sociocultural evolution with the process of operant conditioning. For example, we are told that "the struggle for reinforcement is the central explanatory concept of the behavioral theory of sociocultural evolution" (p. 297); that the fate of sociocultural variants depends on "whether they aid or handicap individuals in the struggle for satisfaction or reinforcement" (p. 302); that experimentation "became a sociocultural trait because of its reinforcing consequences" (p. 294); and that "the principles of reinforcement theory" can be used to explain the spread of the practice of handwashing among the staff of the Vienna General Hospital, this despite the fact that he imagines Semmelweis *ordering* his medical students to scrub (p. 295). Langton is not the first to display this confusion (see, e.g., Ruyle 1973; Ruyle et al. 1977).

Although his major source on learning theory appears to be Albert Bandura (1969, 1977), Langton fails to appreciate or at least fails to make clear his appreciation that Bandura has dedicated his life's work to elucidating theoretically and demonstrating experimentally the distinction between these learning processes. As Bandura has written: "It is evident from informal observation that human behavior is transmitted . . . largely through exposure to social models. . . . It is difficult to imagine a culture in which language, mores, vocational activities, familial customs, and educational, religious, and political practices are gradually shaped in each new member by direct consequences of their trial-and-error performances without benefit of models who display the cultural patterns in their behavior. . . . Although much social learning is fostered through observation of real life models, advances in communication have increased reliance upon symbolic models" (1971, pp. 1-2).

The theory of operant conditioning does not address the facts of social learning. Technically, Bandura has put the problem this way:

"The Skinnerian analysis of modeling phenomena relies entirely upon the standard three-component paradigm $S^d R S^r$, where S^d denotes the discriminative model stimulus, R represents an overt matching response, and S^r designates the reinforcing stimulus. It is difficult to see how this scheme is applicable to observational learning in which an observer does not overtly perform the model's responses during the acquisition phase, reinforcers are not administered either to the model or to the observer, and the first appearance of the acquired response may be delayed for days, weeks, or even months. In the latter case, which represents one of the most prevalent forms of social learning, two of the events ($R S^r$) in the three-term paradigm are absent during acquisition, and the third element (S^d or modeling stimulus) is typically absent from the situation in which the observationally learned response is performed. Like the Miller and Dollard

theory, the Skinnerian interpretation of modeling phenomena accounts satisfactorily for the control of previously learned matching responses by their stimulus antecedents and their immediate consequences. However, it *fails to explain how a new matching response is acquired observationally in the first place*. This occurs through covert, symbolic processes during the period of exposure to modeling stimuli prior to overt responding or to appearance of any reinforcing events." [1969, p. 127; 1977, pp. 36-37; emphasis added]

Let me rephrase Bandura's point in evolutionary language. Do we have to wait for a psychological event of spontaneous generation, mutation, or recombination for a behavior or a rule to appear in the life history of an individual? Bandura's answer is no, that what he has called the "observational learning effect" shows this is not always the case. Particularly with a complex behavior or rule, one group of individuals exposed to a model executing an act or applying a rule often shows a significantly greater incidence of its occurrence than does a control group not so exposed. If spontaneous psychological events were responsible, we would not expect the incidence in the two groups to be identical, but neither would we expect the difference in incidence to tilt persistently in favor of the group exposed to the model. In short, this observational learning effect shows that the cases of the new variant in the experimental group are not spontaneous events but instead are events of sociocultural transmission (often observed with variation). Of course, in a natural setting such interindividually transmitted behaviors and rules also have an initial origin in spontaneous generation, mutation, or recombination. The point is that when transmission by modeling occurs we do not have to await a new repeated origin in each individual. If we did, culture and social life as we know it would not exist.

What is the significance of the finding that reinforcement of the model may make successful interindividual transmission more frequent, that individuals may be more inclined to imitate a behavior or follow a rule they have observed being reinforced? It seems to indicate that possession, by a behavior or a rule, of the traits required for reinforcement in a particular situation contributes to its sociocultural "adaptedness." Reinforcement of the model behavior is not, however, a necessary condition for sociocultural transmission, and certainly many other characteristics of acts and rules also have been shown in the experimental study of social learning to contribute to their sociocultural adaptedness.

As Bandura notes above, subsequent to interindividual transmission, reinforcement is responsible for repetition in the life history of the second individual. Does that mean, then, that while there are two processes of transmission (intra- and interindividual) there is only one process of selection, reinforcement? No, it does not. While the theory of elementary

learning processes deals with the subsequent relative success of various behaviors and rules within the life history of the individual under the pressure of differential reinforcement, a sociocultural evolutionary theory would deal with the subsequent relative success of various behaviors and rules as they continue to spread interindividually, through a social group, under the pressure of sociocultural selection. To apply this analysis to Langton's Semmelweis case: Semmelweis himself acquired handwashing by random variation and differential reinforcement; his students almost certainly did not. They acquired it by observation or instruction, and it was maintained by differential reinforcement. The medical community as a whole, of the hospital and eventually beyond, acquired it by sociocultural "descent" from its ancestral origin in Semmelweis.

E. L. Thorndike, in wishing to deny the occurrence of observational learning or cultural transmission in animals, initiated a long diversion in the psychology of learning toward the elucidation of the mechanisms involved in elementary learning processes. I am among those who believe that the diversion was a fruitful one, resulting in the elaboration and experimental verification of theories about such processes and contributing in particular to our current understanding of the process of operant conditioning as an evolutionary analogue. However, the current revival of interest in social learning, a revival spearheaded by Bandura, has succeeded in again redirecting the interest of many psychologists, but now toward processes of potentially greater interest to social scientists. I would argue that, from the point of view of the social sciences, the most important achievement of this new phase has been the experimental establishment of social learning effects, the proof, in short, that interindividual transmission does indeed occur, something more or less taken for granted by social scientists at least since Tylor. In recent years, in fact, its occurrence in animal populations in natural settings has been widely observed and commented upon by ethologists (Galef 1976). If interindividual transmission and transmission with variation occur, surely sociocultural selection and sociocultural evolution follow. Hence, social learning effects may provide a new foundation for evolutionism in the social sciences.

It would be unfortunate, however, for such a newly founded sociocultural evolutionism to be identified with the theory of operant conditioning. Just because the conditioning process has been shown to be an evolutionary analogue, and because the process of sociocultural transmission, variation, and selection may eventually be shown to be so as well, does not mean they are concretely the same process. Despite disagreement on this fundamental point, I would like to express my strong agreement with the spirit of one of Langton's fundamental themes. One does not need detailed knowledge of the mechanisms of transmission and variation in order to begin building an evolutionary-style theory. It is sufficient to

be confident that these processes occur. This was the case in the history of the theory of organic evolution, it remains the case in the theory of operant conditioning, and I believe it will become the case in the future history of the theory of sociocultural evolution.

Finally, I would like to suggest that Langton's claims for the theory to date are greatly exaggerated. Although he writes that the social sciences have "the *capacity* to make the evolution of sociocultural systems as comprehensible as Darwin made the evolution of species" (p. 288, emphasis added), at times he writes as if this were already an accomplished fact: "The synthesis of an abstract, Darwinian model of systemic adaptation and certain principles of social learning established by behavioral psychology yields a theory of sociocultural evolution possessing the logical structure and explanatory power of the theory of natural selection presented in *On the Origin of Species*" (p. 288). While this vision remains an exciting possibility for the future, all that has been contributed thus far are rather loose suggestions for a rough theoretical framework. Almost no research attempting to confirm such a theory of sociocultural evolution has been conducted. The only two explicit applications with which I am familiar are Gould's (1979) demonstration that Mickey Mouse has evolved neotenuously and Kangas and Risser's (1979) application of species-packing theory to the structure of a strip of fast food outlets! Until those of us inclined toward this view begin to demonstrate that such an approach can successfully organize large bodies of known facts about the social world and shed light on substantive problems in various areas of the social sciences, it is, justifiably, unlikely to achieve acceptance among our colleagues.

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REPLY TO BLUTE

Marion Blute takes issue with what she erroneously believes to be one of the "fundamental features" of my interpretation of the behavioral theory of sociocultural evolution. According to Blute, "The greatest weakness in Langton's analysis is a confusion between elementary learning processes" based on differential reinforcement and "social learning in the strict sense" predicated on modeling. A careful review of my article will reveal, however, that it suffers from no such confusion. Blute, it seems, detects confusion in my analysis in part because she has not paid sufficient attention to the vital distinction that psychologists draw between acquisition and performance of a behavioral pattern (see Bandura 1977), and

thus she does not fully appreciate the important role that differential reinforcement plays in the overall process of social learning.

Blute acknowledges that my analysis does display "some awareness of this difference" between elementary and social learning, and she refers to pages 294 and 308 of my piece as cases in point. However, as I see it, my awareness of this difference is manifested much more clearly at other places (pp. 304-5, 306-7) where I discuss specific cases of sociocultural evolution. True, the technical term "modeling" is not used at these points (explicit mention of the process is made earlier on p. 294), but I assumed that *AJS* readers would readily discern that social learning based on observation was involved in both cases.

At one point in her commentary Blute asks: "Do we have to wait for a psychological event of spontaneous generation, mutation, or recombination for a behavior or a rule to appear in the life history of an individual?" Noting that Bandura's answer to this question is no, Blute implies that my answer is yes. But my answer is no, too, and I am hard pressed to believe that any social scientist would respond in any other way.

Take the evolution of the power serve in the game of tennis. As my discussion (pp. 306-7) of this instance of sociocultural selection makes clear, once Tilden, Kramer, and other innovators had developed the "back-scratching" motion through elementary learning, their competitors and other players observed what they did and then acquired the technique through modeling. Clearly they did not need to rediscover for themselves the backscratching motion through an elementary, trial-and-error process. I did not think it was necessary to spell this out in detail.

Let us hold in abeyance for a moment the issue of how behaviors are initially acquired through observation and modeling and question instead why certain actions are repeatedly performed after they have been successfully emulated. Why do tennis players, for example, continue using the backscratching motion to hit their serves after they have modeled this behavior for the first time? My answer, and Bandura's answer as well, is that the practice is repeated because of its reinforcing consequences. As Bandura (1977, p. 50) notes, "Successful diffusion of innovations follows a common pattern: new behavior is introduced by prominent examples, it is adopted at a rapidly accelerating rate, and it then either stabilizes or declines depending on its functional value." What Bandura means by "functional value" is plain. As he asserts (1977, p. 96), "Behavior is, in fact, extensively regulated by its consequences. Responses that result in unrewarding or punishing effects tend to be discarded, whereas those that produce rewarding outcomes are retained." Supported by this analysis, I remain convinced that the fate of sociocultural variations is ultimately dependent on whether they aid or handicap individuals in the struggle for satisfaction or reinforcement.

Reinforcement and operant conditioning appear to explain the performance and recurrence of behaviors acquired through modeling, just as they appear to explain the maintenance of behaviors acquired through elementary learning. But what accounts for an individual's initial acquisition of a socially displayed pattern of behavior? Bandura cites the action of covert, symbolic processes, and I tend to concur with his interpretation. Nevertheless, it must be acknowledged that this is hardly a settled question. B. F. Skinner, for example, has argued very persuasively (see esp. 1978) that reference to covert, symbolic processes is nothing but a mentalistic gloss which has no explanatory import. Moreover, contrary to Bandura's suggestion, Skinner does not hold that the acquisition of a behavior always demands immediate, reinforcing consequences. This is, in fact, an egregious simplification of his analysis of the "contingencies of reinforcement." Basically, as Skinner sees it, when an individual models a successful social behavior or conforms to a rule enunciated by, say, an authority figure, he is simply responding, as he has already learned, to discriminatory stimuli in his environment. In other words, he comes under the control of external variables rather than internal, symbolic processes. While virtually all psychologists working in the behavioral tradition now concede that many patterns of behavior are acquired without direct reinforcement, they have reached no consensus about the proper explanation of this phenomenon. All behaviorists do agree, however, that the continued performance of an acquired action is a function of the reinforcers it garners.

Blute suggests that my discussion of the Semmelweis case stands as a glaring example of my alleged tendency to confound elementary and social learning. On the contrary, I think this discussion shows that different stimuli can shape and maintain similar patterns of action in different individuals. Blute observes that "Semmelweis himself acquired handwashing [before making examinations of pregnant women] by random variation and differential reinforcement; his students almost certainly did not. They acquired it by observation or instruction, and it was maintained by differential reinforcement." This is obviously true, and I see nothing in my discussion that would lead to any other analysis. It is important to add, however, that Semmelweis and his students initially displayed the practice of scrubbing up for different reasons. Semmelweis was convinced on the basis of certain observations that this practice would help prevent childbed fever. His students at least for some time did not share his conviction, but they complied with his orders because they knew that aversive consequences would follow if they did not—that is, they would lose their positions in the hospital. As Skinner would say, they complied because they had already learned to respond to "discriminatory stimuli" associated with the authority structure of the hospital.

Blute concludes her critique with the suggestion that my claims for the

behavioral theory of sociocultural evolution are "greatly exaggerated." I do not think so. I see no reason to back away from my contention that the behavioral theory of evolution possesses the logical structure and explanatory power of the theory of natural selection, as it is articulated in the *Origin of Species*. As I emphasized in my article (p. 299), Darwin had no evidence that natural selection actually occurred in wild species. His primary empirical evidence for his theory was the results of "artificial" selection, that is, the achievements of animal breeders and horticulturists in modifying the characteristics of domesticated species through selective breeding. I think it can be argued that the literally hundreds of laboratory studies on behavior modification, using animal as well as human subjects, "confirm" the behavioral theory of sociocultural evolution at least as well as the results of artificial selection confirmed Darwin's theory of organic evolution.

The evidence for the behavioral theory of evolution extends far beyond laboratory studies, however. On the one hand, there are the innumerable historical and descriptive studies which document the existence of selection and evolution in the sociocultural realm. Here I am thinking of such studies as Paul Mantoux's (1962) classic investigation of the emergence of the industrial revolution. On the other hand, there are many excellent studies which invoke behavioral principles to explain sociocultural changes. The works of John Kunkel (1970, 1977) come readily to mind.

Blute begins her critique by citing a rather large number of social scientists and philosophers who have come to the conclusion that sociocultural phenomena and systems evolve in a Darwinian manner. Her list could be expanded considerably, but that would serve no purpose here. An equally impressive list could be devised of social scientists who have become convinced that sociocultural phenomena can be effectively explained by behavioral principles. In short, what we are now witnessing in social inquiry is the growing positive selection and integration of the evolutionary and behavioral perspectives. These developments can be explained, I believe, in terms of their reinforcing consequences. In other words, the increasing utilization of the behavioral theory of evolution in one form or another can be explained, to use Blute's words, by the theory's capacity to "shed light on substantive problems in various areas of the social sciences."

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A BEHAVIORAL THEORY OF SOCIOCULTURAL EVOLUTION: AN ILLUSIVE ANSWER TO THE WRONG QUESTION?¹

John Langton's recent article, "Darwinism and the Behavioral Theory of Sociocultural Evolution: An Analysis" (*AJS* 85 [September 1979]: 288–309), has drawn attention to evolutionary theories in sociology and has raised several questions and issues that deserve further comment and merit closer examination. Although at points the paper seems merely a confession of our ignorance about factors that produce sociocultural evolution (we are no better, but perhaps no worse off than Darwin was), at other points it appears to be an attempt to fill this void in our understanding and to provide us with a well-formed and powerful research strategy for analyzing cumulative developments and trends in human societies. Langton's explicit goal is to develop a theory of sociocultural evolution with the "logical structure" and "explanatory power" of the theory of natural selection developed by Darwin; yet one might question whether an isomorphic theory provides an adequate explanation of sociocultural evolution or merely posits an interesting parallel between Darwin's early work and the continuing work of some social scientists.² I assume that Langton's behavioral theory is an attempt to advance our understanding of processes that produce cumulative changes in the technology, information, and structure of human societies, and therefore I feel compelled to comment on what appear to be major shortcomings inherent in this theoretical approach to the problem.³

¹ I would like to thank Bruce Mayhew, Robert Miller, A. P. Smith, and an anonymous reviewer for their most helpful comments on the issues raised in this comment and on its style of presentation.

² The author's statement that "sociocultural systems evolve in a Darwinian manner" (p. 295) is either a very broad analogy (e.g., both have a random and a systematic component) or a serious distortion that ignores the major features of the technological and information-accumulating capacities of human societies.

³ This is not the first time that the law of effect has been offered as a solution for the major problems confronting theories of sociocultural evolution. The interested reader should consult the comprehensive treatment of the subject by Houghton (1976). It is Houghton's contention that theories of sociocultural evolution would benefit immeasurably if they incorporated a focus on individuals, their reinforcement contingencies, and other aspects of Skinnerian behavioralism.

The first critical flaw in Langton's behavioral theory is its failure to consider adequately intersystem processes, intersystem competition, and major social-environmental transactions and exchanges. Employing Lenski's (1970) terminology one is struck by the fact that although the paper does consider certain aspects of the process of *intrasocietal* selection (extinction), it never addresses questions of *intersocietal* selection (extinction) and consequently never comes to grips with the complex problem of determining the relationship between "choices" made by individuals or groups within a system and the survival chances of the system as a whole. The fundamental task that has engaged evolutionary theorists has not been to determine why people do or do not do certain things but rather to explain the differential survival of different (types of) systems under varying conditions and to determine the probable relationships between different social arrangements and technologies and the survival chances of societies. Why do some systems or types of systems survive and prosper (that is, become more frequent or come to constitute a greater proportion of human societies) while others vanish, disintegrate, or come to be incorporated into larger systems (see, e.g., Lenski 1970)? This question is not addressed or answered by theories of psychological reinforcement. It is not surprising that important macrosystem parameters affect the relative rates of reinforcement for different possible behaviors in a society, but the reinforcement does not directly determine the survival chances of individuals, groups, or societies that engage in these activities. Hence, the relationship between actions that are psychologically reinforcing and system survival is unspecified. If one recalls Pareto's (1963) classic distinction between the "utility for" and the "utility of" a community, it can be seen that the two are often at odds with one another. Actions that individuals within a society find enjoyable, rewarding, or reinforcing may not contribute to, and may in fact lessen, the survival probability of the social system of which they are a part. As a first step in identifying factors that contribute to cumulative change and developments in human societies, one must first identify factors that affect their survival and persistence (Segraves 1974). The behavioral theory ignores this aspect.

A second problem with Langton's theory is its failure to distinguish clearly between specific and general sociocultural evolution (Lenski 1970). The former denotes the changes and developments that take place within a particular society (its history, if you will), while the latter denotes the combined impact of internal processes, environmental transactions, and intersystem competition on the composition of the population of human societies. The historical record and prehistorical traces indicate that most societies fail to survive because of conflict or competition with others. Thus, by directing attention to the reinforcement contingencies within particular societies, Langton's behavioral theory fails to consider factors

which may be responsible for the basic trends in the population of human societies as a whole. Theorists who have attempted to determine the role of such factors as conquest, population pressure, and environmental degradation in producing basic changes in the characteristics of the populations of human societies have advanced our understanding of the process of sociocultural evolution; Langton's notion that the law of effect and reinforcement produces a march of progress (more effective arrow tips, etc.) has not. Since these external factors may in fact greatly structure the reinforcement contingencies within particular societies, we may do better to examine them directly rather than concentrate on an intermediary process by which they might be translated into human action (White 1949).

A third difficulty with Langton's conceptualization of sociocultural evolution arises from his extreme individualization of society and his atomization of culture. Langton's theory implies that societies are little more than aggregates of individuals locked in a "struggle for reinforcement," and that cultures are mere congeries of traits. As such it fails to consider adequately constraints on individual and group choices, and, more important, it ignores the fact that cultures and technologies evolve in complexes that evidence at least some degree of internal consistency (Wilkinson 1973). Given that human societies are in large measure systems of information and information processing where information is often stored and processed outside human crania, it is difficult to accept the notion that technologies and culture traits are simply a la carte items on cultural menus which can be selected independently by presumably autonomous actors. The constraints that the environment and the structure of information in human societies place on individual choices (and genius) led one evolutionary anthropologist (White 1949) to conclude that a better understanding of the process of technological and informational change would be achieved by ignoring the constants of human intelligence, wants, and desires and focusing instead on the systemic relationship between ideas and information in the system. One would be better off, he argued, if, when studying human cultures, one proceeded as if human beings did not exist! More recently it has been argued that, even in formally organized systems directed by individuals attempting to realize specific goals, the environment (broadly conceived) acts on the information and material organization of the systems independent of the subjective intentions and experience of the directing individuals (Hannan and Freeman 1977).

The autonomy of individuals and groups is further compromised by the fact that reinforcement contingencies are subject to manipulation by elites within societies, and even elites are limited by the technology and resources available to them and by the presence of or threat of competition with other elites. In class societies the majority of a society's members

find their choices constrained to varying degrees by elites, and though elites are often in a better position to maintain or increase their level of reinforcement, there is no guarantee that some other elite will not mobilize its resources more effectively, having stumbled on, invented, or harnessed some more effective technology or organizational procedure. The fact that competing elites find their activities reinforcing gives no added insight into this process and adds no information for predicting its outcome.⁴ The outcome of such a conflict would in all likelihood be determined by their respective capacities to mobilize energy and information and not by the relative enjoyment they receive from their behavior. Therefore, although one would appear to be on firm ground in assuming that individuals and groups would opt for positively reinforced over punished or nonreinforced behavior, in the abstract, even this assumption must be greatly qualified in order to account for the major constraints that social and environmental factors put on choices and activities.

Perhaps the most damaging criticism that can be raised against the behavioral theory is the fact that ultimately it offers a constant process as an explanation of differences. The law of effect is presumably a constant operative in all human societies at all times. If all human societies are systems consisting of aggregates of individuals choosing from cultural menus and alternative behaviors and technologies so as to maintain, if not increase, their level of reinforcement, why do some societies survive and others not? Why do some choices and technologies succeed and others fail? Can the differential mortality of different (types of) societies be explained on the basis of this law? As a constant aspect of all societies it provides no information for discriminating between systems in terms of their survival chances or for predicting the relative success of systems in competition.⁵ There is much evidence that hunter-gatherers found their societies and life-styles much more enjoyable and "reinforcing" than do the average members of agrarian societies, but the hunter-gatherer systems gave way because they could not mobilize comparable amounts of energy, information, and manpower and hence could not successfully com-

⁴ Bruce Mayhew has called to my attention the more general charge of Hebermehl (1976), who argues that the vacuity of concepts like reinforcement and the triviality of the law of effect are often concealed by elaborate technical apparatus and hypothetical-mathematical models which make it difficult to see that little, if anything, is being said.

⁵ If individuals choosing behaviors and technologies on the basis of reinforcement is the universal process that produces sociocultural evolution, what produces differences in evolutionary developments? Are there better choosers in some groups than others (a "great man" or a racist explanation)? Do some individuals choose the wrong reinforcement? Are some environments more reinforcing or more likely to reinforce technological innovation? Factors such as these would have to be invoked to explain differences or change. In introducing these factors, one would, of course, be introducing the major explanatory *variables*.

pete with agrarian systems. The basic trend in the population of human societies has, therefore, shifted away from hunting and gathering to more powerful technologies and not to more enjoyable ones. One reason Langton may have overlooked this problem with his behavioral theory may be the inherent ambiguity of the term "reinforcement." At some points he is clearly using the term to refer to psychological reinforcement; yet at other points it appears to take on the broader meaning of environmental feedback and the consequences of different activities and technologies for the survival of individuals, groups, and societies that engage in or employ them. This seems to suggest at times that the environment reinforces some systems and activities by according them higher survival probabilities. This stretches psychological reinforcement beyond its legitimate conceptual elasticity. The explanatory power of environmental feedback and the differential survival probabilities of different forms of technology and social organization should not be confused with reinforcement and the law of effect. Employing the stretched concept of reinforcement, one might be tempted to conclude that a group of individuals were extinguished when they *chose* to be punished by a wave of cavalry, a hail of spears, a drought, a famine, or an epidemic disease!⁶ When the concept is expanded, a number of variables are unwittingly introduced into the theory, and they serve to increase the theory's plausibility.

Before closing, it should be noted that Langton's individualization of the process and his atomization of culture may have resulted from an attempt to avoid reifying or hypostatizing metaphysical "forces" in the evolutionary process (Harris 1979) in order to develop a concrete, empirical theory of sociocultural evolution.⁷ This is indeed a worthy motivation, but Langton's procedure for accomplishing it entails a number of pitfalls and difficulties. The fact that much system activity is realized in or through the activities of individuals should not be taken as an indication that the individuals are aware of or understand the implications or consequences of what they are doing, and it should never be taken as an indication that they as individuals are in control (the anthropocentric illusion). Langton's theory implies that evolution is merely the aggregation of individual choices, and it implies that individuals, therefore, control the process of technological change and sociocultural evolution. This reduc-

⁶ The role of disease in affecting the outcome of major military conflicts has been touched on by Zinsser (1935), and McNeill (1976) implicates sociocultural-disease interactions in a number of major trends in social development and organization.

⁷ It may also be merely a reflection of an ideological-epistemological bias in the discipline which assumes that individuals are autonomous actors exercising "free will," and that societies and social structures are only aggregations of these individually motivated activities. It has been argued that theories which are immediately plausible and "obvious" are often only paraphrases or transcriptions of the dominant ideologies of the systems from which they emerge (Mayhew 1980, 1981).

tionistic tendency is itself regrettable since it ignores a number of macro-system and intersystem factors which have been alluded to above, but the notion that reinforcement is the key to understanding sociocultural evolution leads Langton to even further reduction. According to Langton, a major breakthrough in the understanding of sociocultural evolution will be achieved when we penetrate the crania of individual actors to determine why people find reinforcers reinforcing. "At the 'theoretical' level, the behavioral theory will be incalculably enriched when someone discovers why, from a physiological standpoint, reinforcers and aversive stimuli have the effects they do" (p. 308). In what ways, we might ask, will this help us explain why some systems survive and others do not?

In summary, it would appear that Langton may have given an illusory answer to the wrong theoretical and empirical questions concerning the evolution of human societies—an answer which, if accepted, would not only make us think we had answered questions that are still unresolved, but would seriously deflect and misdirect future research on the evolution and development of sociocultural systems.

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REPLY TO NOLAN

Patrick Nolan begins his critique by suggesting that the "explicit goal" of my paper was "to develop a theory of sociocultural evolution" comparable in structure and power to the Darwinian theory of natural selection. This, however, was not the explicit burden of my article. I neither stated nor implied that I was the first person to discern that a theory of sociocultural evolution could be developed by synthesizing behavioral learning principles and the Darwinian or ecological model of systemic adaptation. I was fully aware that this synthesis had already been effected (initially around the turn of the century),¹ and thus my explicit goal was only to analyze its major features and to indicate what it had accomplished.

Nolan is right, however, when he writes that I view the behavioral theory of sociocultural evolution as a *relatively* "well-formed and powerful research strategy for analyzing [and explaining] cumulative developments and trends in human societies." The gravamen of Nolan's comment is that the behavioral theory is not particularly powerful or heuristic because it suffers from certain "major shortcomings." In rebuttal, I hope to indicate that the behavioral theory only suffers from the flaws outlined by Nolan when it is misconstrued and oversimplified.

According to Nolan, "the first critical flaw" in the behavioral theory is its failure to consider the intersystemic dimension of the evolutionary process. For Nolan this essentially means that the theory ignores—and in fact, cannot answer—questions about the survival, persistence, or extinction of sociocultural systems as wholes. I disagree. True, my paper did not address this particular issue. But this does not mean, as Nolan implies, that the behavioral theory is unable to cope with or shed light on this problem. The effective domain of the theory obviously could not be completely delineated within the confines of a short journal article. On the basis of Utz's (1973) perceptive critique of Lenski's (1970) initial attempt to interpret sociocultural phenomena in the light of the Darwinian model, I concluded that the process of intrasocietal selection posed the most crucial and refractory problem for the development of a viable theory of evolution, and thus I focused my attention on this condition rather than on intersocietal selection and competition.

If a society confronts a challenge which threatens its actual, physical existence, its continued survival depends on whether it manages to evolve

¹ The behavioral theory of sociocultural evolution began to take shape when William James (1880) explored what he called the "remarkable parallel which . . . obtains between the facts of social evolution and the mental growth of the race, on the one hand, and of zoological evolution, as expounded by Mr. Darwin, on the other," and when, somewhat later, his student E. L. Thorndike (1908) suggested that selection in the sociocultural realm could be explained by James's recently discovered law of effect.

cognitive and behavioral strategies which will allow it to eliminate or at least assuage the danger. But note—and this is the crucial point which Nolan persistently obscures in his critique—that the behavioral theory does not claim that sociocultural evolution can be fully explicated in terms of the law of effect and reinforcement psychology. As I stressed in my article (pp. 297–99), the experience of reinforcement produced by the environmental consequences which follow certain actions accounts only for sociocultural selection. But sociocultural evolution also entails variation or innovation, that is, the apparently random and more or less continuous production of novel forms of behavior and cognition (pp. 291–94). Like biological mutations, new sociocultural patterns are generated by forces (as yet little understood) which are different from the factors responsible for their selection (Campbell 1965, 1974). Thus, contrary to Nolan's suggestion, the behavioral theory does not try to answer questions about systemic survival and persistence by focusing exclusively on the reinforcement relationship; the theory also takes into account the independent process of innovation, and it recognizes that the ultimate fate of sociocultural systems and biological species often depends on the novel variations they happen to produce.

It is true, as Nolan maintains, that some actions which particular individuals find reinforcing may lessen the survival probability of the social system. But it is not true that the behavioral theory ignores this fact. Indeed, as I see it, the concept of the struggle for reinforcement casts new light on the sociological truism that the interests of the individual and the interests of the community are often at odds.

A second problem with the behavioral theory, in Nolan's estimation, "is its failure to distinguish clearly between specific and general sociocultural evolution." I will pass lightly over this contention because in my estimation it involves a conceptual issue of marginal significance. Specific evolution and general evolution are broad, analytical categories, and I cannot see how plugging data into them will contribute much to the explanation of sociocultural change.

A third flaw in the behavioral theory, Nolan believes, is that it "implies that societies are little more than aggregates of individuals locked in a 'struggle for reinforcement,' and that cultures are mere congeries of traits." Essentially, this is what the behavioral theory implies (for excellent defenses of this perspective, see Durham [1976, pp. 94–95; 1979] and Ruyle [1973]). In fact, the behavioral theory views sociocultural systems as sets of traits—that is, socially shared habits of thought and action—which are all, in the final analysis, nothing more or less than learned, phenotypic characteristics manifested by the particular individuals living in societies. But this does not mean, as Nolan suggests, that the behavioral theory of evolution sees sociocultural traits as "simply a la carte items on

cultural menus which can be selected independently by presumably autonomous actors." The theory implies this only when it is misconstrued and in particular when its emphasis on enculturation is ignored. Individuals continually struggle for reinforcement during their lives, but their success in this struggle typically means that they learn to display under appropriate circumstances the customs and collectively shared ideas of their societies (as Kunkel [1975, p. 50] explains).

Here it should be emphasized that the behavioral theory does not imply that sociocultural evolution "is merely the aggregation of individual choices." Indeed, such an assessment of the theory completely distorts its perspective. The emphasis of the theory is not on the choices of autonomous actors but on random variation and environmental selection. Individuals do not, according to the behavioral theory, pick and choose among behavioral patterns. They do not, in effect, "opt" to become the carriers of particular sociocultural traits, and they do not simply "elect" to have new ideas or to discover new, functional patterns of behavior. Instead they learn certain forms of thought and action because of their genetic endowments and the contingencies of reinforcement to which they have been exposed.

Finally, we come to what Nolan describes as "perhaps the most damaging criticism that can be raised against the behavioral theory"—namely, "the fact that ultimately it offers a constant process as an explanation of differences." However, rather than being a damaging criticism of the behavioral theory, this argument exposes, once again, what is perhaps Nolan's most glaring simplification of that theory. The theory does not advance a constant to explain differences. Instead, it advances at least two constant processes to account for the differences between sociocultural systems. First, to reiterate, the behavioral theory argues that within any society there is a more or less constant generation of novel forms of thought and action, some of which are positively selected but most of which are rejected in the sense that they never become institutionalized or widely practiced. Hence, sociocultural systems are different (and accordingly enjoy different probabilities of survival) because to a significant extent they were constructed from—or, more precisely, selected out of—different pools of novel variations. Second, the behavioral theory seeks to explain the constant process of sociocultural selection by reference to the struggle for reinforcement, just as the Darwinian theory seeks to explain the constant process of natural selection by reference to the struggle for existence.

The behavioral theory does regard the law of effect as a "constant operative in all human societies at all times." But it must be remembered that the law of effect is really an inductive generalization which essentially

states that, as a matter of fact, behavior is "extensively regulated by its consequences" (Bandura 1977, p. 96). Properly construed, the law does not suggest that individuals will discover or always display forms of behavior which are rewarding or which guarantee their survival or the survival of their societies.

The law of effect is a constant factor in human learning, but it does not have static implications because it is deployed over a continually evolving set of dependent and independent variables. New reinforcers are discovered from time to time, and, more crucial, new patterns of behavior are constantly emerging. The positive selection of these new patterns often changes the contingencies of reinforcement under which older, established sociocultural patterns have existed, thus causing them to undergo negative selection (see my article, pp. 302-5).

To substantiate his contention that the law of effect cannot help us to understand the "differential mortality of different (types of) societies," Nolan argues that "there is much evidence that hunter-gatherers found their . . . life-styles much more . . . 'reinforcing' than do the average members of agrarian societies, but the hunter-gatherer systems gave way because they could not . . . successfully compete with agrarian systems." As a universal generalization, the first part of this argument (for which Nolan in fact cites no evidence) appears to be untenable on its face. The emergence of the agricultural revolution clearly meant that some societies found the cultivation of crops to be more reinforcing (i.e., more effective and predictable as a way of obtaining food) than hunting and gathering. Once this is granted, we can readily acknowledge that agrarian societies displaced hunter-gatherer systems. Quite simply, the agrarian societies had learned a set of activities which allowed them to support a larger and more powerful population and thus to compete more effectively for resources. The thorny question is, as I see it, How did they learn those activities? I think the behavioral theory provides a powerful strategy for finding the answer.

Near the end of his critique, Nolan indicates that he cannot see how a physiological explanation of reinforcement will contribute to the understanding of sociocultural evolution. This is not surprising. Nolan does not seem to realize that sociocultural evolution is essentially a grand, inter-generational process of social learning. Nolan never once mentions learning in his comment, and his grasp of behavioral psychology seems shaky. He does not seem to understand the technical meaning of the concept of reinforcement; for example, he gives the notion "voluntaristic" connotations which no behaviorist would accept. Thus, it is not hard to understand why he does not realize that a physiological explanation of why we learn through the consequences of our actions will probably contribute as much

to our knowledge of sociocultural evolution as the discovery of the double helix by Watson and Crick contributed to our understanding of biological evolution.

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Review Essay: Comparative Evidence and Historical Judgment

Injustice: The Social Bases of Obedience and Revolt. By Barrington Moore, Jr. White Plains, N.Y.: M. E. Sharpe, Inc., 1978. Pp xviii+540. \$17.50.

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For Barrington Moore's generation the twin catastrophes of Nazi and Soviet totalitarianism have been the most powerful historical lessons of their own lifetime. In one way or another his work has been a creative response to this experience. All of Moore's writings have focused on a combination of crucial moral and empirical questions: the role of political power in the accomplished, attempted, or abortive "great transformations" to modernity; the issue of historical inevitability and alternatives; the horrendous price exacted in human lives and happiness; the possibility and prospects of a more humane society. Any attempt to resolve such large questions forces a writer to transcend academic specialization. Moore went beyond the interdisciplinary program of the Department of Social Relations and of the Russian Research Center at Harvard and combined the roles of social historian, sociologist, moralist, and philosopher of history at a time when that was not the expected way of making a career. He set himself off from the majority of his generation, which embraced a positivist belief in science as the accumulation of specialized and formalized knowledge. After three decades the outcome is clear: the sociology lecturer at Harvard, like Simmel and Weber an academic outsider, has emerged as one of the most important figures of his generation.

Moore's writings are major achievements in what is nowadays called macro sociology but historically has been the core of the sociological enterprise—the historical study of societies in an evolutionary or developmental vein. At the same time, however, his approach has been antireductionist, "antitheoretical," and averse to "sociological imperialism" (p. 157). Time and again he has warned against "undue theoretical elaboration" (p. 13). Instead, he has been interested in historical generalization, in historical synthesis, on both an empirical and a normative level. Philosophically, he is a truculent rationalist, politically a soft-spoken liberal, sociologically a sober adherent to the principle of social reality—witness his dislike of political romanticism and its theories of alienation and his defense of rational authority and a work ethic. He holds fast to an old-style notion of historical progress but is appalled by its human costs. This accounts in part for his ambivalence about Marxism and socialism. He cannot really

see that they have changed the world for the better, but regrets their failure and is even willing to let the future revise his skeptical judgment. But in spite of the moving testimony of his friendship with Herbert Marcuse (in the *Reflections*), it seems to me that ultimately he does not share the latter's romanticism and utopianism, although they agreed on the perniciousness of Soviet Marxism. Moore, then, insists on keeping the future open for progress and is careful to stay clear of conservative theories of cultural pessimism.

When many Americans became radicalized and embraced communism in the midst of the Depression, Moore's professional response was to learn Russian. His first book, *Soviet Politics* (1950), was written against the background of the Popular Front, the Stalinist trials, the wartime alliance, and the incipient Cold War. The rest of the title was already programmatic: *The Dilemma of Power: The Role of Ideas in Social Change*. The second book (1954), too, although still limited to Russian-area studies, carried a title indicative of Moore's central interests: *Terror and Progress*. With the third volume, *Political Power and Social Theory* (1958), Moore went beyond the scholarly monograph to formats that he has retained since: the critical methodological essay, the moral reflection, and the comparative study of progress, freedom, revolution, and totalitarianism in pre-industrial, industrializing, and industrialized societies. *The Origins of Dictatorship and Democracy* (1966), his most widely read and admired study, offered a global comparison of lord and peasant in the making of the modern world, analyzing the revolutionary origins of capitalist democracy in England, France, and the United States and the routes to the modern world in Asia. This study responded to the research interests, prominent from the mid-fifties to the mid-sixties, in totalitarianism, economic development, and especially the course of labor protest in the early stages of industrialization. By turning to the comparative study of the landed upper classes and the peasantry, Moore modernized moribund rural sociology and became the precursor of the predominantly Marxian study of agrarian class conflict and revolution, which is such an important part of today's concern with the "Third World." The moral turbulence of the sixties found its echo in Moore's *Reflections on the Causes of Human Misery* (1970), in which he broadened his attack on fashionable trends in social science into a critique of American society and polity as a form of "predatory democracy."

Now we have Moore's grand attempt at summing up his moral and sociological insights. It is always risky for a scholar to write a sum of his knowledge and insights toward the end of his career. Between lengthy description, historical and theoretical synthesis, and philosophical abstraction there lurks the danger of saying the obvious or offering too personal a solution. Moore approaches a very difficult task with his customary mixture of blunt self-assurance and studied diffidence. He offers us the personal sum of his knowledge as a "preliminary" inquiry and a series of historical speculations. The work, however, is not only a summing up but is also meant to tidy up the unfinished business of a long career. Two major cases had been missing in his 1966 global tour—the minor ones he considered

historically unimportant (see *Origins*, p. xiii). Moore had originally planned to include chapters on Germany and Russia "in order to show how the social origins of fascism and communism in Europe differed from those of parliamentary democracy" (p. xii). In modified form this agenda is taken up now in a lengthy case study of the German labor movement within its urban setting and in a comparison with the Russian experience. This 300-page treatment, the centerpiece of the book, comes wrapped up, so to speak, in an anthropological, psychological, and philosophical discussion of concepts of justice, moral outrage, and the problem of moral relativism (some 200 pages).

Moore's older concern with terror and progress appears to have been channeled into the direction of the new political and scholarly interest in justice and equity. (It is not proof to the contrary that Moore refused to read Rawls during the writing of the book.) For him this is less a new departure than a conceptual broadening. Similarly, he has moved closer than before to the broad movement in contemporary social history that writes "from the bottom up"; he wants to deal with people "at or near the bottom of the social order" (p. xii). He draws on the very limited autobiographical evidence from the 18th century onward; rare interviews in the early Nazi period; the neglected pre-1914 attitude survey by Adolf Levenstein, an ex-worker; and the secret survey of the social composition of the Nazi party in 1935. But he keeps his distance from romantic populism as well as the structuralist history of the Annalists; he remains very much concerned with the big political events of modern history.

In part 1 Moore proceeds deliberately in an ahistorical fashion, spending 100 pages on constants and variables in the sense of injustice. He consults anthropological literature and singles out ascetics, untouchables, and inmates of Nazi concentration camps as examples of voluntary and involuntary acceptance of suffering. He then balances this bleak picture with the uplifting topic of how individuals "put iron into their souls" and come to oppose oppression. However, this section turns out to be little more than a lengthy and inconclusive review of the experiments by Stanley Milgram and Solomon Asch, Lawrence Kohlberg's cross-cultural investigations of stages of moral development, Freudian interpretations of the revolutionary personality, and Robert Coles's study of black and white young activists. Moore's most general point is purely descriptive: "Breaking the appearance of inevitability is crucial to overcoming the moral authority of oppression" (p. 151).

In turning to the historical level and the case of the German labor movement, Moore does not propose to test any hypotheses. The connection between the general concern with obedience and revolt and the German case has a deeper justification for him, which is revealed by this earlier remark in the *Reflections*: "The failure of revolutionary violence, perhaps even the failure to exert sufficient violence in crucial circumstances when huge alternatives hang in the balance, may lead to tragic results that last for generations. German history is especially suggestive in considering this issue, though there is always room for debate about what the underlying

tendencies actually were and what prospects actually existed for the success of a more vigorously revolutionary policy" (p. 30).

Much of the new book can be read as an exploration of the ramifications of this statement. The German problem looms so large because the great catastrophes of this century are linked here. There is first the often-debated question of whether the Social Democratic labor movement could have prevented or effectively opposed the First World War, and, second, the question, often asked by Marxists, whether a successful German revolution after 1918 might have prevented the isolation of the fledgling Soviet Union and thus made historically unlikely Stalinist totalitarianism with its millions of losses in lives. And there is the final question whether a successful revolution was morally and historically "necessary" to prevent Hitler's holocaust.

Before making historically grounded judgments about the moral responsibility of actors for events, Moore, disliking the mythologies about the proletariat, considers it imperative to identify the size and shape of lower-class groups and their attitudes. He selects three periods: the revolution of 1848, when the imagery of the proletariat emerged; the years before the First World War, when the Social Democratic labor movement seemed to make great strides; and the wartime and immediate postwar years, when the labor movement split before the working class was confronted with a historically open situation. In 1848/49 the German industrial working class was still very small and inarticulate, but artisans were numerous and vocal. In spite of a medley of "reactionary" and "progressive" demands, their social critique appears to Moore a vigorous attack on the new "inevitability" of capitalism. He provides a statistical assessment of the minuscule size of the proletariat and analyzes the ideological currents discernible in the records of artisan conventions. But time and again he reaches for the rule of experience and the balanced, synthetic statement: "I strongly suspect that doing nothing remains the real form of mass action in the main historical crises since the sixteenth century. That is not to deny the decisive importance of occasional popular outbursts, which, however, can never draw on more than a tiny fraction of the population" (p. 157).

In analyzing social and cultural trends before 1914 Moore stages a kind of disappearing act of the proletariat. In spite of Imperial Germany's rapid industrialization, the industrial proletariat is harder and harder to find, the more statistics are consulted, and in the end is visible only as a very small minority, in one calculation amounting to less than 6% of the total population. In reviewing the autobiographies of (former) workers and the Levenstein survey of working-class attitudes, Moore encounters a sometimes strongly felt sense of injustice but no revolutionary mentality. Therefore, he can argue that there was no "massive reservoir of potentially revolutionary sentiment" (p. 184) and dismiss as "merely a polemical exaggeration" (p. 217) the charge against the Social Democratic leaders of having failed to provide leadership for a huge revolutionary mass.

Moore also reflects the current emphasis on regional study. He considers the evidence from Imperial Germany's most industrialized region, the Ruhr

district, and provides an illuminating contrast between the militant miners with their shared traditions and the passive iron and steelworkers with their social fragmentation in the workplace. However, in 1914 neither group exerted any radical pressure.

Imperial Germany was destroyed by the ravages of an unexpectedly long and bloody war. Military defeat and mutiny created a power vacuum in which the proletarian radicalism of the most highly privileged workers, the metal workers in Berlin, suddenly emerged as a significant force. However, Moore believes that there was no realistic possibility for a socialist revolution, only for a liberal one. This required the creation of a new police force and army; a purge of the civil service, including the courts; some form of workers' codetermination; and a partial socialization of basic industries. When the Social Democratic leadership and its allies failed to make progress toward these goals and the Right tried a coup, the only major proletarian uprising ever to occur in a highly industrialized country took place in the Ruhr district in the spring of 1920, but was quickly put down. The long road to the Nazi catastrophe was open. In a broad discussion of determinism and voluntarism Moore argues that moral responsibility must be made historically specific. He blames Friedrich Ebert and other Social Democratic leaders, but also their opponents to the Right and Left, and judges that the proletarian "council movement was one more of human history's potentially liberating alternatives crushed by stronger forces" (p. 327). He suggests that "relatively slight changes in timing and tactics in this fluid situation could have brought about quite different consequences" (p. 396). Yet he abstains from the moralizing of some liberal American historians as well as from the impatient condemnations by European leftist intellectuals and instead concentrates on a painstaking assessment of the balance of forces and historical alternatives. Thus, Moore pursues a situational analysis, which downgrades the import of long-range social and economic factors and rejects political apologies and historical analyses that assert a structural determinism.

In his last historical chapter Moore goes against the grain of some entrenched political and scholarly opinion by arguing that Nazism too expressed in some ways the moral outrage and sense of injustice of many people, including industrial workers, who adhered to standards of conventional morality, hard work, thrift, honesty, and loyal obedience. He insists on this point "in order to avoid falsifying the entire analysis" (p. 398). Next to Soviet totalitarianism, Nazism becomes for him another instance of the repressive and illiberal aspects of moral outrage. He sympathizes with those who want to find crucial differences between rightist and leftist forms of revolt but finds that "the twentieth-century portion of the record is far from encouraging" (p. 431).

Moore's overall conclusions are partly rules of experience addressed to students of history and partly statements of historical wisdom addressed to historical actors. Progress is impossible without the resolute "conquest of the illusion of inevitability" (p. 462), but this depends also on the level of economic development and on specific historical constellations. In crisis

situations there are frequently some opportunities for progress, but they are easily undermined by people turning upon one another anarchically. Therefore, Moore distrusts the widely held belief that progress is likely to arise out of great historical conflagrations and opts for the small liberating steps.

The heterogeneity of the work makes evaluation difficult. Moore's attempt to combine a historical case study with the comparative approach of the *Social Origins* and the philosophical considerations of the *Reflections* creates much disparity. In particular, there is an unresolved and probably unresolvable tension between the level of the historical case study and the level of the general analysis of the constants and variables of the sense of injustice, in spite of the author's effort to weave his German conclusions into the last two theoretical chapters. Chronology, narrative, comparison, generalization, and moral evaluation are very hard to combine. Depending on one's viewpoint, there is too much or too little of each. What will appear to some readers an admirable sum of historical wisdom is likely to look to others like pointless truism or senseless abstractness. I happen to find myself in general agreement with Moore's philosophical position, his prudently limited historical generalizations, and most of his historical judgments, but I also think that he has tried too hard to force disparate dimensions into one mold. The result is an unwieldy book. It may work for Moore as a personal equation. I am afraid it will not work for most of his specialized peers, who are likely to be impatient: the new philosophers of justice and equity will not find the argumentation formal enough; experimental researchers of equity will not consider the rules of experience capable of operationalization; macro sociologists committed to theory construction as a means of causal analysis will be dissatisfied with the "low" commonsensical nature of generalization; comparativists with global interests in world systems and world economies will find the focus on Germany too narrow—after all, Germany has pretty much disappeared from the agenda of comparative research; the shrinking number of old-style and new-style historians of German history will find the ground familiar. But Moore may not have written the book primarily for the edification of his peers, of whom he is often very critical. It may be that bright students, who have not yet specialized, can read their way through the book, following its dimensions and digressions with equal attentiveness and learning from many of its wise statements. Here they can encounter a historical vision attuned to the sense of the growing limitations of Western civilization without having to abandon the hope for, and commitment to, some notion of historical progress. Herein would lie a considerable pedagogical achievement.

Review Essay: Social Gerontology and the Sociology of Age

Last Home for the Aged: Critical Implications of Institutionalization. By Sheldon S. Tobin and Morton A. Lieberman. San Francisco: Jossey-Bass, Inc., 1976. Pp. xii+304. \$13.95.

The Aged in the Community: Managing Senility and Deviance. By Dwight Frankfather. New York: Praeger Publishers, 1977. Pp. xi+221. \$8.95 (paper).

The Gray Lobby. By Henry J. Pratt. Chicago: University of Chicago Press, 1976. Pp. x+250. \$15.00 (cloth); \$5.95 (paper).

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From the first and second books listed above, the reader gains a dismal view of old age in America today. For the frail elderly person who makes the decision to survive, the costs are seen either as institutionalization (Tobin and Lieberman) or as "deviant" subsistence in the community (Frankfather). For the future, however, the third book (Pratt) points to the power of old people to change their status. These books are a small sampling of many recent special studies of old age. Few of these studies are by sociologists. Most are uninformed by conceptual models from the sociology of age. Yet they provide insights into the nature of age and aging, they raise questions, they afford information that can specify, clarify, or challenge the conceptual models. Thus, if the emerging sociological understanding is still too general and abstract (Riley 1980), numerous fine points can be sketched in from the plethora of special studies of old age. In particular, the three books under review can be seen as contributing indirectly to sociological understanding of age as a structural feature of society, and of aging as a social—as well as a biological and psychological—process. They help to specify sociological theories of the age-stratification system, though they largely overlook the dynamic aspects of this system.

Last Home for the Aged contributes important new understandings to one aspect of older people's place in society. Sheldon S. Tobin and Morton A. Lieberman focus on that poignant sector of the aged population facing institutionalization, the penultimate life-course transition (if dying is the ultimate transition). In an ingenious research design, using batteries of psychological tests, 85 older people (in their 70s and 80s) were traced during the 1960s from preadmission through the first year of residence

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in a long-term care facility, "matched" samples of old people still in the community ($N = 40$) and old people who had lived in institutions for one to three years ($N = 37$) were used for comparison, and three cases were studied intensively.

The key finding of the Tobin and Lieberman study challenges the conventional wisdom that institutional life itself explains the cognitive deterioration, apathy, and helplessness widely observed among elderly residents. Instead, the greatest psychological damage appears *before* entrance to the institution. As soon as the entrance decision has been made, the familiar symptoms start to appear: cognitive constriction, unhappiness, reduced dominance in social relations, etc. "The older person in the anticipatory institutionalization phase already looks like an institutionalized older person" (p. 213). This finding (like similar findings in other studies) contributes significantly to the sociology of age by illuminating the nature of this life-course transition, for it identifies the trauma, not with adapting to the new role of resident in an institution, but with relinquishing the former roles—extrusion from one's familiar place in the community, loss of independence, the sense of abandonment (p. 213). Few massive effects are attributed to the actual experience of institutionalization: for those who survive the first year, a high degree of stability in functioning is reported. (The researchers recognize the usual knotty dilemma, however: findings describe only the selected few who survived, since at least half the sample entering the institution either died or deteriorated seriously during this year.)

Many other details supplied by this study show, for example, how peers in the institution are shunned as reminders of becoming irreversibly old; or how a period of initial adjustment to the home is marked by decreased concern for self-care, greater body preoccupation, increased hostility toward others. These and other findings lead the authors to recommendations for practice with the elderly. Tobin is professor in a school of social service administration, Lieberman in departments of behavioral sciences and psychiatry. Their recommendations include home-based services that can limit premature institutionalization, and improved forms of institutional care such as geriatric restoration centers or terminal care facilities (hospices).

If Tobin and Lieberman elucidate old people's pain in leaving the community, Dwight Frankfather elucidates their pain in remaining in it. *The Aged in the Community* gives a Goffmanesque picture of a community ("Claiborne") in which many older people, failing in the daily struggle for food, shelter, safety, or medical attention, turn to deviant acts. There are manifold instances of sneaking into a hospital emergency room to bathe or spend the night, or resisting with violence commitment to a mental hospital. The more ingenious and tenacious the older person's deviant behavior, the more likely it is to be labeled "confused" or "senile."

As research, Frankfather's study is naive. Based on the author's participant observation and interviewing of old people and social agents in 14 treatment settings (from mental hospitals and nursing homes to the fire department and corner cafeteria), the usual statements of research pro-

cedure are largely replaced by methodological admonitions. There is little attempt to locate the case study in any larger population of communities, in any compilation of relevant research findings, or in any theoretical framework (Frankfather's field is social welfare). Key terms like "the aged" and "deviance" go undefined, and "senile" (later replaced by the "confused elderly," p. 23) refers sometimes to the less privileged and less competent elderly (p. 185) and sometimes to older people generally (p. 19).

Despite its weaknesses, however, the monograph raises serious questions as to the adequacy of community resources to aid needy older people in "preparing legal, welfare, and Medicaid documents; purchasing groceries and household supplies; preparing meals; feeding the cat; maintaining and repairing homes, from price shopping for exterminators to trash removal; teaching personal hygiene and the management of personal finances; and mollifying angry or anxious neighbors" (p. 193). These struggles for survival are overlooked by professionals and other social agents who, in their preoccupation with pathological aspects of senility, are often unable or unwilling to support families in maintaining older people outside institutions.

Taken together, these two books do more than describe important aspects of the status of older people today. More generally, they help sociologists to specify the implications of the serious imbalance between people and roles in the age-stratification system of the United States, a topic of concern for the sociology of age (see Waring 1975; Maddox 1979). As life expectancy has risen and the mortality curve approaches rectangularization, there have been dramatic increases in numbers and proportions of old people in the population. Meantime, the age structure of roles has lagged behind. The structures of the family, the household, the work force, have been slow to make room for all these older people. And many individuals now live beyond an age at which they are permitted to function effectively. For example: "An elderly lady lives alone in an apartment. She loses her taste for food, and eats only toast. She develops vitamin deficiencies. She doesn't drink enough fluids, and becomes dehydrated. She soon becomes confused, and is brought to the hospital" (Frankfather, p. 16). This is just one of many instances drawn from "Claiborne," where all varieties of dependent or unconventional behavior are defined as senile, and where such definitions can become self-fulfilling prophecies.

The two studies discussed so far not only specify the paucity and inadequacy of roles for older people, they also recommend social changes. And Henry J. Pratt in *The Gray Lobby* specifies one source of potential change: the political power of an age stratum. If the role structure is inadequate to the needs of the aged population today, old people themselves may be able to make some rectification. By comparing the powerful old-age associations of the 1960s and early 1970s with the less successful associations of the 1920s and 1930s, Pratt (a political scientist) seeks to specify the triggering mechanisms through which the social problems of an age stratum gain a prominent place on the agenda of public concerns. His anal-

ysis emphasizes "the greater skill shown by the more recent old-age activists in manipulating symbols, enlarging the size and attentiveness of the relevant publics, and evolving an ideology capable of sustaining group commitment and élan" (p. 189). While the earlier movement aroused public interest but did not contribute directly to enactment of the Social Security legislation in 1935, according to Pratt the recent movement—through mass support from senior citizens and a "policy system" composed of government officials and leaders of old-age groups—has been able both officially and informally to affect the formulation, initiation, decision-making processes, and legitimation of public policy.

Details of Pratt's analysis are important to the sociology of age because they show how the needs of an age stratum can be given high political visibility, high levels of official concern, and a remarkable federal response. In particular, his work contributes to the sociological theory of age stratification as it resembles other forms of social stratification—those based on class, race, ethnicity, sex (Foner 1979). According to this theory, age solidarity in large groups requires that consciousness of kind spread throughout the age stratum, and that group goals and group leadership develop. Pratt's analysis suggests how such developments can come about. What it does not stress is the distinctive feature of age stratification within the family of stratification systems: namely, the inevitability of the underlying processes of cohort flow and aging. The inevitable flow of cohorts sets the strata apart from one another, fostering age solidarity within a stratum and potential age cleavage or conflict between strata. But the process of aging, the inevitable mobility through the age strata, tends to loosen age solidarity and to bind together people of differing age. Everyone ages. How, then, can an old-age movement be sustained when its leaders and its membership must continually shift? How can elderly retirees demand that the working population pay higher taxes, when recalling the economic burdens of their own middle age? How can old people press their own needs to the possible detriment of their children's interests?

Such questions relate to many others that are not addressed in these three books. In describing the place of older people in the age-stratification system today, the authors largely overlook the uniquely dynamic character of this system. Fundamental to the sociology of age are the inevitable processes of cohort flow and aging that, in confrontation with social change, give the age-stratification system its dynamic character, and that yield the continuing interplay between individual aging and the changing age structure of society.

Diverse aspects of this interplay are touched upon, however, in many other studies of old age not reviewed here. As the transformation of the age structure has highlighted the importance of the middle and later years, the literature of social gerontology has burgeoned. Gerontology, which stresses the many facets of age and aging, has drawn heavily on the concepts and methods of several disciplines, including sociology. In turn, the developing sociology of age, though it concerns all ages, can be greatly

enriched by the many special studies of older people, such as the three reviewed here, from social gerontology (see, e.g., Elder 1975; Butler 1975; Binstock and Shanas 1976; Hess and Markson 1980).

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Book Reviews

The Theory of Power and Organization. By Stewart Clegg. London and Boston: Routledge & Kegan Paul, 1979. Pp. x+176. \$18.00.

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The Theory of Power and Organization begins inauspiciously with a comparison (based on the work of another writer) of American and European organization theory, including a table listing eight differences between them and a chart replete with diagonals and two-way arrows. The gist of the comparison is that American organization theorists are positivistic, psychologistic, technocratic, ideologically conservative, and—though it is not put quite so plainly—servants and allies of capitalism, whereas European theorists are the reverse of all these—genuine “structural” sociologists, critical of the “status quo” from a “Marxian” standpoint. Merton, Gouldner, Dubin, Blau and Scott, and Etzioni are, to be sure, declared to be exceptions to the American norm, but nowhere is it recognized that these men, none of whom is young, have by now produced several cohorts of students who have concentrated on the very themes and topics American theorists are alleged to neglect: interorganizational relations, system-environment relations, intraorganizational conflict, and the impact of technology on organizations. The names and writings of such younger organization theorists as Stinchcombe, Street, Perrow, Lehman, Hage, Heydebrand, and many others are never mentioned. Moreover, nearly all of them teach in sociology departments rather than business schools, although the authority Stewart Clegg relies on claims the opposite to be true of American in contrast to European organization theorists. I know very little about business schools on the European continent, but there are six in Britain, all of fairly recent origin, and several leading British organization theorists are on their faculties, including at least one whom Clegg not only cites frequently but thanks for “friendship, support and advice” in his preface. So much for the entire loaded comparison. Not all that important, perhaps, since it is only presented as “a preliminary to critique of sociology as a meta-discourse” (p. viii), but even metatheorists are supposed to get their facts straight.

After an opaque chapter on sociological method written in what might be called “meta-English” and bowing dutifully to Wittgenstein, Habermas, Derrida, and Foucault, Clegg reviews current conceptual debates over whether or not power is causal, intentional, dispositional, and capable of acting at a distance through anticipated reactions. He sets up an opposi-

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tion between power as individual action and power as structure, favoring the latter. Despite the superior philosophical sophistication of the recent discussion, this polarity does not seem to me to differ significantly from older oppositions of individual and group, personality and culture, "consciousness" and "social being," and, more broadly, the realms of psychology and sociology. I fail to see why an emphasis on "two-person interpersonal relationships" as fundamental to the analysis of power relations requires us "to regard power relations between America and Russia as reducible to the interpersonal relationships of the Presidents of each country" (p. 43). No one, after all, confuses a summit meeting between Carter and Brezhnev with an encounter between two friends in a bar. In sociology, the concept of "role" was invented to preclude such an error. Clegg's argument is just a variant of the old self-serving claim of some sociologists that to impute motives to persons is in and of itself to commit the dreadful sin of psychological reductionism. His ultimate aim, however, is less to make a case for a *sui generis* sociology than to reject "liberalism" as a "theory of politics . . . whose ontological base is the individual, his/her acts, motives, intentions, etc." (p. 44) in order to affirm a very special version of the primacy of structure: Althusserian Marxism.

Before elaborating this version of structure, however, Clegg takes on Steven Lukes and Anthony Giddens in two exceedingly murky chapters that make prolific use of that all-purpose word "dialectical." Despite their confrontation with the power/structure antinomy, Lukes and Giddens are both criticized for holding in the end too "individualist" and "voluntarist" a conception of power deriving from "their respective inability to free structure from the fiction of the subject in their theoretical work" (p. 75). Yet earlier in the book Clegg had seemed to accept the Wittgensteinian view that human action always involves choice, or the possibility of doing otherwise, and had for this reason rejected the alleged positivism of Robert Dahl's belief that power can be understood as involving "causal relations of a strictly Humean type" (pp. 39-40). Clearly, Lukes and Giddens remain a bit too British in their concern with "agency," a bit too tainted by "liberalism," for Clegg's taste. But, to take an example presumably close to Clegg's heart, I do not see why to say that a worker under the early "capitalist mode of production" could choose to work under oppressive conditions, to starve, to emigrate, or to resort to crime is to take an excessively individualist-voluntarist view of action that minimizes the "structural constraints" limiting his choices to such a narrow and painful range. Here Clegg wants to deny the actor the "reflexivity" and choice he sees as intrinsic to the activity of theorizing.

The last half of the book advances a Marxist conception of the unity of power and structure that is then applied to organizations or at least to capitalist enterprises as organizations. Gramsci's concept of "hegemony" is adopted and extensively discussed. "Hegemony," unfortunately, is not a very precise idea in Gramsci's writings: it seems to combine aspects of "ideology" in the Marxist sense, "culture" as understood by anthro-

pologists, and "*Herrschaft*" in Weber's sense, embracing both the coercive and the consensual or legitimate exercise of collective power (although Gramsci's accent is usually on the latter). Clegg uses "hegemony" and its adjectival derivatives even more broadly, often as a synonym for legitimate authority applicable at the micro level of the family or the factory floor as well as at the macro level of institutions and the class structure, sometimes even as the equivalent of "normative" or "cultural." Young Marxist academics seem to labor under a curious compulsion to find substitutes of unquestionable Marxist pedigree for the most familiar concepts of "bourgeois" sociology. The popularity of "hegemony" is a case in point.

Clegg makes a few suggestive, if not original, observations on organizations: for example, his contention that increasingly complex technology increases the need for consent ("hegemony") in contemporary capitalist society at the expense of coercion; also, his claim that technical progress blurs the separation of staff and line positions and creates new "structural" bases of power outside the official hierarchy of command, thus intensifying the difference between formal and informal organization first stressed in the Hawthorne studies. But Clegg's main line of argument is to locate organizations within the constraints of the "capitalist mode of production": the class struggle between owners and laborers, the need to extract surplus value from labor power in new ways as the "organic composition of capital" changes, and the drive to accumulate and expand on a world scale. He does not argue in support of these propositions about capitalism—he simply assumes they are established truths, even though all of them have been questioned by thinkers who continue to call themselves Marxists (or, at the very least, neo-Marxists). More important, he never so much as refers to the facts that organizations apparently very much like those in capitalist countries exist in state socialist societies and that workers there, as recent events in Poland have dramatized, have grievances which, though more acute, are recognizably similar to those of workers under capitalism. Althusser and his most prominent followers, all quoted voluminously by Clegg, are subject to the constraints (structural? hegemonic?) imposed by their membership in the French Communist party, but Clegg is presumably not a member, so why these "silences" in his text? Again and again, as in so many Marxist disquisitions, one notes that "industrial" could be substituted for "capitalist" as the qualifier of "mode of production" without any obvious loss of meaning.

Essentially, this book represents an attempted marriage between ethnomethodology with a Wittgensteinian accent (the dominant point of view in an earlier book by Clegg) and Marxist structuralism. Whether or not the marriage is adjudged a *mésalliance*, and whatever value one may find in either or both of the partners, Clegg's book is not the only evidence that both of these theoretical "moments" have by now rigidified into an ingrown Alexandrian scholasticism of interest only to its own adepts. Though published less than two years ago, *The Theory of Power and Organization* is already a period piece.

Central Problems in Social Theory: Action, Structure, and Contradiction in Social Analysis. By Anthony Giddens. Berkeley and Los Angeles: University of California Press, 1979. Pp. 294. \$20.00 (cloth); \$8.95 (paper).

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Anthony Giddens's fertile mind, his amazing productivity, and his outstanding critical and analytical skills have propelled him into the forefront of British critical social thought in a short time. His lucid commentaries on the major figures of classical sociological theory, from Marx to Weber and Durkheim in particular, have become indispensable guides to the intricacies of their thought. In the past few years, Giddens has widened his concerns and attempted to bridge the gap between Anglo-Saxon and continental modes of analysis by providing his readers with extended critical commentaries on German existential, phenomenological, and hermeneutic thought as well as on Habermas and his cothinkers. In *Central Problems in Social Theory* he broadens his interests further by offering critical reflections on French structuralism in linguistics and social analysis, and extends his sharply critical dissection of functional analysis as well as his sympathetic consideration of ethnomethodology and British linguistic philosophy.

As this short enumeration of some of the many strands of social thought Giddens dissects will convey he has outstanding absorptive powers, an amazing ability to react in critical manner to every turn in the zeitgeist, be it on the continent or in the Anglo-Saxon world. But as this book, more than its predecessors, makes clear, Giddens pays a high price for his expository and critical virtues. In listening to so many voices, the reader fails to hear clearly Giddens's own. For example, in this book, he feels impelled to take a stand on all the latest fads and fashion of what Raymond Boudon calls "le tout Paris" of social thought and thus gets distracted from what he clearly intends to do: construct a scaffolding for his own theoretical system. Like a honeybee he dips into a great variety of flowers, but the reader is not yet able to assess what the end product of all these flights of theoretical imagination finally will amount to.

Giddens's main project, to be sure, is clear enough. He wishes to overhaul all inherited social theory from the classics to hermeneutics, structuralism, and functional analysis and to replace it by a theory in which human agency, and the acting subject, have pride of place. Arguing that most social thought has seen human actors as what Harold Garfinkel has called "cultural dopes," unreflective enactors of social scripts written by "society," Giddens wishes to bring men and women back again—though surely not in the manner of George Homans. His "theory of social structuration" argues that social actors are knowledgeable about the social systems they produce and reproduce although they may not be aware of all the consequences of their actions. Even though he seems to be

attracted by analyses that focus on interactive strategies in the manner of Goffman or of symbolic interactionism, and even though he has profited a great deal from hermeneutical and structuralist analytical strategies, Giddens objects to their neglect of the importance of power and constraint. But when it comes to the actual elaboration of this ambitious undertaking, I, at least, feel somewhat cheated. We are served a complicated new terminology of social analysis, together with shrewd comments on the shortcomings of earlier approaches, but the analytical payoff seems meager. His new vocabulary does not amount to a new theory.

Nothing that Giddens writes can leave one indifferent. The book abounds in illuminating passages and brilliant observations, but the center does not seem to hold. It is at best a prolegomenon to the construction of a future integrated theory, full of sensitizing ideas yet falling far short of the architectural solidity of the major theorists of the past. One does not really know what difference it would make if concrete problems were tackled in terms of Giddens's new approach. His dazzling display of critical fireworks leaves too few traces in the memory after one has laid the book aside. One can only hope that in his next work Giddens will speak more clearly in his own voice without being distracted by the din and noise of controversy, polemical infighting, and the wish to display his familiarity with all the current varieties of social and philosophical thought. Perhaps he can then show that the uses of his framework of analysis would indeed make a difference.

Human Nature, Class, and Ethnicity. By Milton M. Gordon. New York: Oxford University Press, 1978. Pp. xiii+302. \$11.95 (cloth); \$3.50 (paper).

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It is often said that the mark of a good scholar is that his writings can withstand the test of time. *Human Nature, Class, and Ethnicity* is a convincing demonstration of Milton Gordon's sustained and lasting scholarly contributions. All but one of the essays in this volume were published previously, and they highlight a scholarly career that reaches back nearly four decades. They also represent Gordon's most important writings—including his earlier work on marginality, social class, subsocieties, and subculture, and assimilation and pluralism; and his later work on human nature and social action.

Most students of intergroup relations are familiar with Gordon's earlier work. However, many may not appreciate the depth and range of his contributions, and I suspect that few are aware of his developing interest in human nature. Although the articles representing the best of his earlier work are still useful and are now conveniently available in one volume,

it is his writings on human nature in the first two chapters that will draw attention to *Human Nature, Class, and Ethnicity*. The first chapter, appearing in print for the first time, is a long essay on human nature and sociology. The second chapter, originally published in 1975, incorporates the human nature approach in a general theory sketch of race and ethnic relations. Anticipating reactions to these chapters, Gordon is careful to emphasize that his "current salient concern with the subject of human nature" does not "represent an abrupt break" with his previous work "carried out within the standard sociological framework" (p. xii) but should instead be seen as complementing his earlier work. "It is for the sake of achieving a fuller understanding of social phenomena," writes Gordon, "that I advocate the development of a serious concern for the nature of human nature by all social scientists" (p. xii).

In his opening essay, "Human Nature and Sociology," Gordon shows convincingly that despite the overwhelming emphasis on cultural and social factors in sociological studies, there has been in the past few years a reassessment of the importance of human nature by representative sociologists. As one of the central contributors to this reassessment, he advances "a theory of human nature" that conceptually distinguishes hereditary factors from social influences and at the same time emphasizes "their mutual and constant interaction" (p. 48). Thus, such concepts as "emotional capacities," "cognitive capacities," and "overarching drive motivations" are explicated to stress the importance of considering the biological equipment of human needs. And such concepts as "aggression" and "motivations," subdivided into multiple types (e.g., dependency-induced cooperation, instrumental cooperation, authority-induced cooperation, forced induced cooperation, and normative induced cooperation), are elaborated to clarify the notion of "derived behavior patterns" that emerge from the interaction of some elements of the biological equipment of needs with the cultural and social environment.

However, what Gordon has provided is not a theory of human behavior, in the strict sense of the word, but a framework for analysis that has systematically incorporated, codified, and synthesized the empirical findings and theoretical assumptions of such scholars as Jean Piaget, Gregory Rochlin, Ernest Becker, John Dollard, William J. Goode, Stanley Milgram, Jerome Kagen, and Seymour Epstein. Nonetheless, it is not readily apparent that the arguments he outlines have to be taken into account to enhance explanations and predictions of group behavior. And his contention that "construction of a valid theory of human nature is indispensable to the sociological enterprise" (p. 4) is not convincingly demonstrated in this essay.

However, to question Gordon's claims for the centrality of human nature analysis is not to ignore the merits of this approach. In his excellent essay "Toward a General Theory of Minority Group Relations," he uses bio-social development variables to provide one of the best explanations for predisposition of groups to exhibit racial or ethnic hostilities and conflicts. He argues that because ethnicity, unlike social class background, cannot

"be shed by social mobility," it "becomes incorporated into the self" (p. 73). Thus, "man defending the honor or welfare of his ethnic group is man defending himself" (p. 74). And since the human being is essentially a narcissistic, often aggressive, defender of self, the potential for ethnic conflict is always present. However, it should also be emphasized that this basic biosocial predisposition, as Gordon himself acknowledges, is mediated both by interaction process variables and by societal variables. Indeed, *variations* in ethnic conflict are determined not by biosocial predispositions but by social and cultural variables. Accordingly, the real value of the human nature analysis in the field of race and ethnic relations lies not in improving our ability to explain the persistence, increase, or reduction of ethnic hostilities and conflicts in particular situations and places, but in alerting us to the ever present potential for ethnic conflict in any traditional or newly emerging multiethnic society. In this connection, one would hardly expect a Milton Gordon to be surprised about the recent outbreaks of ethnic hostilities in the liberal society of Sweden.

In the final analysis, until the biosocial variables can really be shown to enhance the explanatory and predictive import of intergroup theory, social scientists will treat them as "given." And the real burden of explaining variations in group behavior will fall on the interaction process variables and the societal variables. It is ironic that support for this very argument is found in Gordon's brilliant chapter, "Toward a General Theory of Minority Group Relations," in which, despite the emphasis on human nature, his most powerful explanations of racial and ethnic group behavior incorporate social variables at both the intergroup and societal levels.

Nevertheless, science is frequently advanced less by the answers provided than by the questions raised. By raising the question of human nature in well-written and thoughtful essays, Milton Gordon has joined Pierre van den Berghe and other scholars in establishing a serious agenda for theory and research on human behavior. This approach will generate lively controversy. With creative thinkers like Milton Gordon involved in the debate, there is good reason to feel that the controversy will be productive.

The Social Function of Social Science. By Duncan MacRae, Jr. New Haven, Conn.: Yale University Press, 1976. Pp. xv+351. \$15.00.

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Duncan MacRae proposes that the social sciences should serve human welfare by applying themselves to the solution of practical problems. For MacRae that aim requires ethical clarification within and among the

social science disciplines, and it requires as well a reorganization of the university to accommodate "valuative" discussion.

MacRae opens *The Social Function of Social Science* by observing the powerful and pervasive influence of the natural-science model on the social sciences. The defenders of positivism have affirmed and reaffirmed the necessity of cleansing social science of all valuative elements in order to attain the rigor of the physical sciences. Critics of positivism, on the other hand, have assailed objectivity as a sham and have called for a taking of sides. MacRae, however, advocates a third way: "I shall argue here that there is another path to follow; that reliable scientific knowledge of man and nature is an important resource for policy choice but can coexist with rational ethical discourse; that some of the values of science may be transferred to this ethical discourse; and that scientific propositions and ethical assertions, while clearly distinguishable, may be fruitfully combined in academic disciplines concerned with the study of man and society" (p. 5). MacRae thus wishes to make valuative discourse rigorous and to link such discourse to social science inquiry. The social sciences, informed by rational methods of inquiry and evaluation, should become applied sciences directed to the "general welfare." Reorganized to include evaluation, the social sciences would also educate the citizenry to employ rigorous methods of thinking about the practical choices they need to make.

MacRae recognizes that applied science (both natural and social) in its present form aids the producer of goods, services, and policies more than it does the consumer. He has faith, however, that the social sciences can reorganize themselves so as to serve the "general interest." In a word, he proposes that effective interdisciplinary communication be institutionalized so that valuative discourse can be conducted with no less rigor than scientific discourse. By paying particular attention to clarity, consistency, and generality, the ethically informed social sciences can educate the citizenry. Such education would purportedly lead to a rational consensus and hence to practical political policies more consistent with the interests of the society as a whole.

MacRae's proposal presumes that the social scientist possesses an expertise which provides superior insights into the workings of society. The proposal further presumes that if ethical discourse were regulated by the norms of social science, it would rise above the "vague" and "imprecise" valuative conversations of Everyman in everyday life. Relying on Garfinkle's research, MacRae contends that "in everyday usage, words and sentences are interpreted with the aid of information about their context, that is, about the speaker's past history, his motives, his present circumstances, and his relation to the audience" (p. 81).

An assessment of MacRae's proposal therefore requires a careful examination of its underlying premises. Is it true that, in social science usage, words and sentences are any less dependent than in everyday usage on context and on the speaker's (writer's) values, motives, biography, circumstances, and relation to the audience? Perhaps it is true that

the social scientist has here and there achieved a greater clarity than the layman. Can the claim be substantiated, however, that on the whole the social scientist acquires superior understanding of the social world by means of his expertise? But let us for the moment grant MacRae his assumption that rigorously rational modes of inquiry are characteristic of the social sciences and that such modes of inquiry yield greater clarity than everyday discourse. Can we then take the next step with MacRae and suppose that if rigorous rational discourse were passed on to the citizenry, the result would be a consensus for social policies in the "general interest"? The reply to that question should be evident. For it is clear that every large and complex society consists of a multiplicity of groups and organizations, each with its own values and interests. And it is difficult to imagine how value systems differing in their ultimate premises can be freely reconciled to form one single solidary consensus.

The Idea of Race. By Michael Banton. Boulder, Colo.: Westview Press, 1977. Pp. 190. \$15.50.

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The Idea of Race is both an enterprise in the sociology of knowledge and a study of race relations theory during the past two centuries. The roots of the idea of race lie in the intellectual ferment that began when Western European people became aware of, and began to reflect upon, the physical and social differences among themselves. Michael Banton describes the results of these reflections in sections entitled "The Racializing of the West" and, later, "The Racializing of the World." Midway through the book he discusses the voluminous work of the strange Anglican minister and self-styled "raceologist" Charles Kingsley, who reacted to the African slave trade, the theoretical muddle about race, the impact of Darwinian thought, and the dramatic American agony over slavery. In successive chapters entitled "Social Interaction" and "Structure and Function," Banton shows how in America interest in race turned away from physiological mysticism, the effort to construct immutable types, and the distraction of racial Darwinism toward the empirical approach to the study of race relations. In the last two chapters of the book he attempts to sort out the accumulating mélange of racial concepts and theories and to synthesize modern thought in this field into a consistent system. Under the title "ethnogenesis" he argues that race can be understood only by recognizing the images minorities hold of themselves as well as the conceptions the dominants entertain. In the final chapter he dismisses the modern concept of "racism" as having little heuristic value in scientific study, continues a long-standing controversy with John Rex, a British

colleague, and discourses on how to formulate useful racial concepts and propositions.

In a special way the history of the study of race exemplifies and parallels the early history of sociology. At first the social philosophers perceived the differences among and the collective characteristics of people as fixed immutably in their inherited physiological characteristics. Working from this base, they tried to construct physiological "types," later called "races," which they could employ to explain the social phenomena that they perceived. The immutable aspect of these physical types was challenged by Darwinian evolutionary theory. There ensued three-quarters of a century of frustrating intellectual indecisiveness and effort. It was the Americans, who had had the most devastating experience with race, who led the way out of this muddle. Beginning with Park in the early 1920s, race and race relations were conceptualized in *social* terms. This fertile insight opened the way for rapid development of the modern sociology of race relations. Park's social process perspective was later refined and supplemented by the structural-functional approach to empirical study.

By implication, *The Idea of Race* comments in still other ways about the issue of race and race relations in the modern world. Banton shows that the very idea of race was invented in Western Europe, particularly in Germany, France, and England. Initially, therefore, the idea of race did not refer to differences of color. These early European scholars were seeking to explain cultural and behavioral differences among themselves by reference to physiological characteristics and differences.

It was only after the Europeans came into extensive contact with non-whites that the concept of race became a powerful symbol in world affairs. In the mid-19th century the untrammelled forces of theology, economics, and politics were fused with physiology and color in the modern concept of race. Thus, as Banton reveals, the earlier single-factor deterministic logic could no longer cope with the idea of race. Sociology thus became the appropriate scientific vehicle for conceptualizing and analyzing this idea.

The Idea of Race constitutes an overview and evaluation of diverse materials around the central idea of race and set at the level of theory. In order to perform this task, Banton had to decide what to include and emphasize and what to leave out or subordinate. Some readers may differ with his choices. I, for one, find it difficult to understand why Charles Kingsley deserved a full chapter. Other readers may disagree with Banton's evaluations of the theories and studies discussed. It is also to be regretted that this book fails to come to grips with the contradictory pressures produced by the ethnocentric evaluation of nonwhites as incapable and unfit for membership in established societies that were made by the English-speaking peoples and the persistent demand of the Puritan ethic to strive, succeed, and achieve. This normative clash, introduced and perfected in the American slave system, was diffused throughout the world. As a consequence, an ineluctable thrust toward racial conflict was

built into the structure of all multiracial world societies. Race relations are, therefore, as Cox insisted, conflict relations.

Despite its faults and limitations, teachers, students, and researchers in race, ethnic, and minority relations will find *The Idea of Race* a stimulating and useful addition to the voluminous extant literature in the field. Substantive, well thought out, and qualified by Banton's scholarly personality, this book will repay a thoughtful, careful reading.

The Organization of Interests: Incentives and the Internal Dynamics of Political Interest Groups. By Terry M. Moe. Chicago: University of Chicago Press, 1980. Pp. x+282. \$21.00.

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While pluralism has been widely criticized for assuming that every relevant interest would find expression in an organized group, its legacy of rich empirical studies of the role of organized interests in the political process has not been matched by its critics. Mancur Olson's application of the distinction between collective goods and selective benefits to the field of interest groups vitiated the conclusion that political power was widely shared by focusing attention on the differential operation of impediments to organization among the various classes and sectors of the economy. But Olson's work, as Terry Moe recognizes, has been more successful in explaining why groups fail to organize than in accounting for successful organization. Thus Olson has stimulated a profusion of theoretical responses while empirical studies of interest groups using his framework have languished.

The Organization of Interests seeks to remedy this imbalance by qualifying two assumptions of Olson's model sufficiently to generate plausible empirical outcomes without forfeiting the theoretical rigor of Olson's argument. Since even the revised model remains fundamentally (but not exclusively) an economic model, Moe restricts its application to economic interest groups. Chapters 2 through 4 construct a logic of organizational formation, maintenance, and internal politics presuming rational economically self-interested actors but relaxing Olson's assumption of perfect information concerning costs and benefits. Moe concludes that the free-rider problem may be partially circumvented because association members will overestimate the weight of their contributions to attaining a collective good. His insightful application of monopoly theory in describing the capacity of a dominant entrepreneur to stave off rivals and utilize tying arrangements to facilitate the transfer of monopoly power between the association services market and the political representation "market" is a formulation which merits further attention. After investigating the implications of this revised economic model, Moe introduces a second modi-

fication of Olson by supplementing economic self-interest with noneconomic motives. Part 2 of this book is an examination of the empirical literature on groups, culminating in a chapter which analyzes survey data from association members. The brevity of this chapter and its limited relevance to the fundamental theoretical concerns addressed by the text justify focusing here on the theoretical first part.

The first modification: Moe avoids the subjectivist chaos that the qualification of imperfect knowledge might entail by introducing the entrepreneur as a focus of analysis. The entrepreneur can overcome the free-rider problem by manipulating the flow of information to members and persuading them, particularly the smaller ones, that their contribution is indispensable to securing the collective good, shifting their perceived marginal benefit curve upward. But how does the entrepreneur's control over information arise? Moe addresses this question explicitly in a discussion of member bargaining: "But the major determinant is his central position, with all that it implies in terms of resources and strategic advantage" (p. 58). In other words, when Moe introduces the entrepreneur he is not introducing a risk-taking, resourceful character who will create an organization to extract previously untapped profits, he is introducing a managerial role in an established organization.

Indeed, were he to introduce the founder of an economic interest group he would have to abandon his premise of solely economic motivation before he had established his economic model, for these founders often sacrifice self-interest to promote their organizations. Moe tries to avoid the circularity of presupposing the organization to explain its formation by confining his attention to the question of why a new member might join an existing association, but this question is not sufficiently radical to address the theme of organization formation which he raises. If he would go beyond organization maintenance, he must return to the pre-organizational "state of nature" and examine the origins of organizations. If an economic model cannot even account for the formation of economic interest groups, the priority of economic motives over the supplementary noneconomic motives will be uncertain.

The second modification: James Q. Wilson's *Political Organizations* (New York: Basic, 1973) documented extensively the extent to which noneconomic motives play a role even in economic interest groups. To accommodate these findings, Moe supplements the economically self-interested bargaining model with the solidaristic and purposive incentives described by Wilson. By introducing piecemeal the modifications brought by these motives, shifting a marginal cost curve downward here, promoting rivals to enter the market there, Moe can preserve the basic economic logic. But solidaristic and purposive motives raise theoretical problems which demand a radically different approach from that of the economic self-interest model. These motives demand treatment in terms of the concept of authority. As Charles Lindbloom states in *Politics and Markets* (New York: Basic, 1977), a work which systematically distinguishes the bargaining relation of the market from authority relations, "Control

through exchange always requires that a person give up some value to induce another to do as he wishes, and control through persuasion takes time and energy. By contrast, control through authority is often *costless*, as when some people find it convenient to be authoritatively coordinated and someone else enjoys coordinating them" (p. 19, emphasis mine). Whatever the fixed costs of maintaining authority, the marginal costs of its exercise are often zero, and such a form of control defies analysis solely in terms of the market. Moe acknowledges this by excluding non-economic groups from his analysis, but the presence of these motives in economic interest groups casts doubt on any explanation which incorporates the motives while excluding the theoretical underpinnings associated with them. Moe fails to achieve the descriptive depth of Wilson's *Political Organizations*, with its interweaving of market and authority frameworks, while he has already sacrificed the elegant simplicity of Olson's analysis by adulterating it with noneconomic motives.

Change in Public Bureaucracies. By Marshall W. Meyer. New York: Cambridge University Press, 1979. Pp. ix+251. \$19.95.

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In this era of New York's fiscal crisis, Cleveland's fiscal fiasco, and even Chicago's fiscal wobbles, Marshall Meyer's book is of timely interest. Using a sample of 215 city, county, and state finance agencies, he examines changes in these public bureaucracies between 1966 and 1972. Although 1972 precedes the current period of crisis and retrenchment in American local governments, many of Meyer's observations still inform the situation. Moreover, they do so precisely because his overall orientation stresses "that the structure and behavior of public bureaus are largely shaped by environmental forces, especially forces in larger social and political environments" (p. 14). Such a statement might seem obvious to a political scientist. From an analyst of organizations the perspective represents an important advance.

Much comparative research on organizations focuses on internal structure and differentiation, ignoring the environmental inputs. The work of Peter Blau, Marshall Meyer's own teacher, is notable in this respect. Researchers such as Paul Lawrence and Jay Lorsch have developed explicit theories of organizational structure and differentiation in the environment, but their data are extremely limited. *Change in Public Bureaucracies* explicitly incorporates environmental shifts, by its longitudinal study design, along with a sample large enough to permit rigorous statistical analysis.

One surprising result is that local finance agencies show little stability of formal organization over time. Traditional organization measures—

numbers of divisions and sections, levels of supervision, and span of control—show relatively low autocorrelations between the 1966 and 1972 data. This in itself is an important finding because it counters earlier assumptions of organizational stability over time. What does produce changes in these aspects of formal organization is shifts in size. Finance agencies that grew larger between 1966 and 1972 also experienced greater elaboration of structure, as seen in more divisions, levels, and sections.

In contrast, the impact of external characteristics on organizational structure shows more consistency. Environmental demand is expressed by size of the general (or corporate) fund, size of the total funds administered by the local government unit, size of the population served, and total number of government employees. All of the demand variables increased between 1966 and 1972, a period of generally rapid growth for local governments. But, curiously, the measures of environmental demand often have a negative effect on the size of financial agencies and on the number of divisions and sections in them. At this point Meyer introduces the concept of leadership conditions as a mechanism mediating the effect of the environment. The greater the turnover in leadership and the more leaders depend on higher levels of government, the stronger the negative impact of environmental demand.

Higher environmental demand also leads other agencies to compete for more finance responsibilities, which in turn reduces the number of responsibilities held by the original finance agency. Finance responsibilities include 13 separate activities, among them tax collection, auditing, purchasing, managing government real estate, preparing operating and capital budgets, investment management, and insurance management. The effect of competition on agency responsibilities is considerably mitigated by the original jurisdiction of the agency. Three clear types emerge.

The finance function in cities, counties, and states is handled by a comptroller's office, a finance department, or a department of administration. The first is the oldest form and the most narrowly circumscribed. The comptroller or auditor acts as an accountant regulating and monitoring expenditures. Little policy analysis is involved and since the tasks are fundamental and essential, this type of organization has changed little over time. The finance department includes revenue collection, purchasing, and investment, as well as basic accounting and auditing. Over time the division between financial and nonfinancial policy has become blurred, and finance departments find more competition for their functions and a consequent diminution in responsibilities. Departments of administration, most of them formed after 1960, experienced less attrition of responsibilities because their mandate already included nonfinancial aspects of administration such as personnel and data processing.

One strength of Meyer's book lies in the analysis of differences between comptrollers' offices, departments of finance, and departments of administration. The treatment here contains both substantive content and complex analysis over time. One could wish for more such substance elsewhere. One excellent anecdote (p. 4) relates how a police chief caught

his on-duty officers attending a local movie theater through a perusal of computerized information on traffic tickets—an illustration of personnel management using the data processing output of a finance department. But there are few such examples of analyses dealing with content. This lack of concern with substantive as compared with structural issues spills over into the treatment of environmental demands. Finance agencies are bureaucracies embedded in a political context, but the reader is rarely made aware of this context.

Many theoretical traditions bear on the political environment. For example, an institutional perspective might suggest that reform government structure has an impact on municipal finance agencies. Or, more generally, one might question whether the jurisdiction of the entire local government—whether state, county, or city—has no relevance. In the decision-making tradition, the relations between the finance director and chief executive would appear significant. Do they share political goals and views? Were they elected on the same ticket? In New York Abe Beame, successfully, and Harrison Golden, unsuccessfully, have used the comptroller's office as a platform for the mayoralty. Theorists of bureaucracy, in the Anthony Downs and William Niskanen tradition, would be concerned with the orientation of the finance director. Is he likely to seek expansion or only protection of his agency? It seems more than plausible that the autonomy and longevity of the finance director, and the responsibilities of the finance agencies, respond to such political differences in the larger environment. If they do not, that fact is important enough to consider and report.

None of these comments denies the relevance of Meyer's emphasis on agencies responding to their environments; they only suggest that he conceives of this environment too narrowly. As we look at the late 1970s and early 1980s, the overall fiscal and political conditions of local government may be particularly dramatic in changing the structure and functions of finance agencies. Declining economic resources and citizen pressure to cut taxes make fiscal issues central to all policy decisions. Demands for productivity studies, revenue projections, and cost-cutting innovations are increasing. A narrow accounting and auditing orientation in finance agencies may no longer be sufficient. As a consequence, the most stable of the three types of structure in 1972—comptrollers' offices—may have proved the least stable by 1980. Certainly, however, *Change in Public Bureaucracies* provides a thoughtful, precise, and important baseline for any future studies.

Organizations and Environments. By Howard E. Aldrich. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1979. Pp. xiv+384. \$14.95.

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This book is useful for those who believe, as do Howard Aldrich and I, that most issues of social organization in industrial or postindustrial society are preeminently matters of formal organization. The applied orientation of the humanist social psychologists of the 1950s, and the glorification of management implied in the equally functionalist open-systems viewpoint which dominated the late 1960s and the 1970s, give way to some degree under the weight of an analysis presented here. Although *Organizations and Environments* is still somewhat functionalist, it does not take as a matter of faith that management speaks for the whole or that organizations are necessarily built from the top down by rational designers. It therefore represents a useful beginning for those whose empirical research and subsequent theorizing may constitute the bridge between empirical organizational sociology and recent structural sociology, which emphasizes competition, conflict, and the patterns of change over time which historical analysis seeks to identify.

Aldrich presents a carefully developed set of arguments designed to further an approach to understanding organizations based on the following premises: (1) that examining populations of organizations distinguished by common manifestation of some organizational form can lead to insights which studying individual organizations is not likely to yield; (2) that focusing on change of organizations is more likely to generate contextually rich explanations for the currently observed state of organizational affairs than is focusing on static structures; and (3) that the two procedures just recommended are likely to generate research and theory of general sociological interest, thus strengthening the ties between the study of formal organizations and the study of social organization in general.

These arguments are presented in a manner reflecting high scholarly standards. Aldrich goes to great pains to use the literature both extensively and thoughtfully. In so doing he shows how the theoretical positions he advocates do little violence to the dominant perspective, which focuses on the adaptation of individual organizations, and in fact complement it.

After telling the reader what he is about in a clearly written introductory chapter, the author quickly gets down to business with a description of his view of a population ecology model of organization. A clear distinction is drawn between the subject at hand and social Darwinism. In particular, at the end of the chapter, he notes the logical error in inferring from the "survival of the fittest" dictum that "what exists must be fittest." There is no implication in the model that organizational forms

have been evolving toward some ultimately superior form or that such a superior form is even possible. Nor is it implied that one sees in any particular cross-sectional view of the organizational world what is particularly effective or good. In fact, many of the organizations we see today are on their way out and will not survive into tomorrow. Also, many organizations which flourish are by other standards ineffective, nasty, or socially destructive. "Fitness" refers to how many organizations of a given kind one observes in a locally bounded system.

Following Campbell, Aldrich describes natural selection processes in three stages: variation, selection, retention. In doing so he seizes on a useful logical distinction but errs in projecting the sequence. It is not true that variation occurs first, then selection, and then retention. All three occur simultaneously. This is not simply a matter for purists but has substantial implications for conceptualization of empirical studies: how one picks the time frame for observation, how frequently one makes observations, how the models are constructed, and how they are estimated.

A similarly confusing problem involves limitations on the usefulness of the model. Government agencies rarely fail. Small organizations fail at a higher rate than large ones. One might quarrel with the factual bases of such assertions, but more important is the fact that one chooses a time frame and therefore the rate of failure among big organizations relative to little ones may be irrelevant. If one is doing field research, one trades off number of organizations observed against failure rate with respect to time. During any given period, fewer elephants than fruit flies die, but no biologist would argue that natural selection processes do not affect elephants or that such a theoretical perspective should be excluded from the analysis of their evolution. The salient virtue of population ecology theorizing is that it can be applied usefully across the whole spectrum of organizations. Most organizational theory, particularly the "open-systems" kind, is actually a theory of big organizations. Most organizations are not big. Most organizations do not last very long. A theory which excludes most organizations is of dubious importance. On the other hand, the ability of the theory to deal with the kinds of organizations which have been of greatest interest in the past is unquestionably important. In my opinion, it is a mistake to limit the field of applicability at an early stage. Time will tell whether natural selection theorizing will be useful for the study of all kinds of organizations, and it will be useful or not primarily in empirical research. For present purposes, it seems worth noting that the capability for quick adaptation to a changing environment, or for domination of that environment, is likely to be present at the expense of other features which might increase fitness in different ways. If this is true, population ecology models can subsume adaptation perspectives so long as the researcher is willing to focus attention on populations of organizations rather than on individual organizations.

After laying out the model which underlies the entire enterprise, Aldrich presents three chapters (chaps. 3-5) which develop concepts used to describe organizational environments, sources of organizational variation, and

selection processes as rooted in the environment. These chapters are well written and incisive. Aldrich skillfully uses ideas deeply imbedded in theorizing about organization-environment relationships as commonly practiced by those whose work has not featured population thinking.

A serious attempt is made to link recent work on "loose coupling" with population ecology in analyses of organizational innovation and sources of variation. I have as yet been unable to distinguish between organizations which are loosely coupled and those which are simply disorganized. Aldrich's discussion does not help much and seems to ignore the plausible argument presented as long ago as 1962 by Blau and Scott that implementation of creative plans or choices often requires highly centralized and disciplined organizations. Organizations made up of parts each of which adjusts to its own particular environmental problems may in fact be the least creative or adaptive because each subunit vetoes any collective action which limits its own freedom of action. Put concretely, schools and other educational organizations do not come to mind immediately when one thinks of creative organizations. But the study of educational organizations spawned the concept of loose coupling itself.

Organizational creativity is as much a power phenomenon as anything else. The ways in which organizations acquire resources and the manner of their allocation clearly have much to do with how they respond to a more or less changeable and threatening environment. Aldrich is, however, probably right that selection processes *within* organizations may work in such a way as to promote adaptive behavior when viewed from the perspective of the collectivity.

At the end of the fifth chapter, on environmental selection, the book's focus begins to change. It becomes less concerned with the population ecology point of view and moves toward a consideration of the impact of resource flows on individual organizations, particularly as those effects are mediated by strategic choice and other actions carried out by high-level managers and administrators. The other direction in which attention moves is toward a consideration of more inclusive units of analysis. The creation of organizations (chap. 7) is discussed with heavy emphasis on the state and macro economy as a source of patterned encouragement or discouragement for building new organizations. This discussion depends heavily on Stinchcombe's discussion in the *Handbook of Organizations*, edited by James G. March (Chicago: Rand McNally, 1965).

The eighth chapter is on organization persistence. Probably the most interesting ingredient in this chapter is the skillful use of examples, such as Zald's YMCA study, to illustrate the otherwise highly abstract arguments.

In chapter 9, the book departs from the consideration of organizational populations, touching on the subject only briefly through the last five chapters. Most of the rest of the book is on networks of organizations and interorganizational relationships. Most of chapter 9 is inspired by Hirschman's work on exiting behavior of individuals. An interesting argument is presented showing the limitations of rationalist analyses of exit

when the organization does not have control over exit or entry. This discussion blends nicely with that presented in chapter 10, which is about boundary-spanning roles. The development of these roles is seen in large part as emanating from conflictful relationships with other organizations, the subject of chapter 11. This chapter uses ideas common in social-psychological treatments of exchange to begin a consideration of organizational networks. The subject is introduced through a consideration of organization sets, which might be studied at the population level themselves. Unfortunately, both of these theoretical interests have spawned more speculation than theory, and the discussion is not as well developed with research examples as are some of the others.

Chapters 12 and 13 are concerned with interorganizational relationships as they affect efforts by organizations to manage their interdependencies and as they figure in societal elite structures. At this point, Aldrich clearly intends to expand his organizational interests in directions placing less emphasis on rational solutions to problems in which managers' self-interests are confused with organizational interests.

The book has some weaknesses which, I think, stem from its dual purpose as a text and theoretical treatise and from the fact that there is so very little empirical literature on which to erect such an extensive conceptual scaffolding. The book is difficult to use as a text because it focuses so completely on Aldrich's theoretical position. The lack of empirical research makes the book rather abstract. In consequence, students will find some of it tough going. For logical reasons, Aldrich often fleshes out concepts with various alternative interpretations when the reader, even a sympathetic one such as myself, keeps waiting for the proof of the pudding. So as a work in original theory, the book is more successful in laying out the conceptual framework of an approach than in providing the manner of its application in substantive research.

All in all, however, *Organizations and Environments* achieves the goals Aldrich lays out for himself in the beginning, and it deserves the attention of scholars who think organizational sociology can be more than it is, and ought to be a source of stimulation instead of a preserve for the arcane, which it often is.

The Policing of Families. By Jacques Donzelot. Translated by Robert Hurley. New York: Pantheon Books, 1979. Pp. xxvii+242. \$10.00 (cloth); \$4.95 (paper).

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This is the most refreshing book of historical sociology (social history) I have seen in a decade. Purportedly a history of the family in France

over the past 200 years, *The Policing of Families* manages to escape completely from the limited and limiting theoretical perspectives which dominate social history: modernization theory, Freudo-Marxism, and liberal empiricism. Jacques Donzelot takes his theoretical cues from Michel Foucault, an important historian and social theorist whose works are still not adequately appreciated by social scientists in this country, even though they are all available in English. In *Discipline and Punish* (New York: Pantheon, 1978) and in *The History of Sexuality* (vol. 1, *An Introduction* [New York: Pantheon, 1978]), Foucault has developed a theory and method for analyzing the history of social institutions that stresses the inseparability of discourses and practices, knowledge and power. Donzelot, a student and associate of Foucault, has taken his cue from his mentor and written a brilliant book on the intersection of society and family.

Donzelot argues that the modern family is neither a victim of society (the position of Christopher Lasch) nor the perfect fulfillment of human aspirations (the position of the Parsonians, for example, Edward Shorter). Instead, for him the family is the outcome of a complex interplay of strategies and forces emanating from many points both within and outside the family. In addition he shows that there are at least two types of family, not one as is normally assumed.

The middle-class family emerged in late 18th-century France through an alliance of medical men and wives, an alliance that sought to break with older traditions and advance the trend of privatization, already well established among families at this social level. By the mid-20th century, however, the scene had changed. Doctors had been replaced by "psys" (psychoanalysts, psychologists, and counselors of all types). At the interface of family and society, among middle-class families a set of images came into prominence which derived from psychoanalysis and, in a very complex manner, achieved dominance in regulating relationships among family members and between the family and society (especially the schools).

The historical pattern for working-class and poor families in general was quite different. Instead of "regulation by images," a "tutelary complex" was constituted in which the family was narrowly circumscribed by a set of controlling agencies. In the early 19th century the problem was the opposite of that of the middle class: not to enforce the privacy of the family by supporting it against outside social forces but to bring outside forces to bear on the family, to impose middle-class norms on the working-class family, to break the tie between the working-class family and the working-class community, to expel men from the taverns and children from the streets so that the cozy nest of domesticity might be established. Hence a multitude of social practices had to be eliminated or reshaped. Orphanages had to be abolished, women and children had to be removed from factories, compulsory education had to be set up, salutary health practices had to be taught to working-class women, contraceptive devices had to be disseminated. The task was not easy. Even-

tually it required a host of new social agencies: juvenile courts, welfare services, reformatories, schools, and many more. Even then the results were mixed.

In a brief review one cannot do justice to the complexity of Donzelot's argument. Suffice it to say that no one before him has shown the connections between so many diverse social practices and discourses. *The Policing of Families* is an immensely rich book that needs to be studied carefully and read slowly. It is sure to place the social history of the past two centuries on an entirely new footing. My only reservation is that it omits a discussion of the emotional structure of the family and therefore cannot account for the enormous appeal of romantic love and the nuclear family, one which is still remarkably strong.

Chinese Family and Kinship. By Hugh D. R. Baker. New York: Columbia University Press, 1979. Pp. xii+243. \$17.50.

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Family sociologists have long been fascinated by the Chinese family, and chapters on the subject are almost obligatory in introductory family textbooks. Many features of Chinese family life provide interesting contrasts with Western patterns, both traditional and modern. Family cohesion and loyalty seem more strongly developed than in the West and make it possible for the young people of an entire village to migrate to the other side of the world and still maintain intense ties with their native place and kinsmen. In some regions strong lineages celebrate their solidarity by carrying out ancestor worship for founders who lived 25 or 30 generations earlier. In such lineages the names of all of those in the intervening generations may be faithfully recorded in clan genealogies, and some of these lesser ancestors will be the subject of worship ceremonies as well. Although the average family was only modestly larger than its counterpart in various parts of early modern Europe, on occasion in elite families the household could expand to include more than 100 members; such families provided the drama for *The Dream of the Red Chamber* and other famous Chinese novels. A rather extreme form of arranged marriage was also common, in which the couple often met for the first time only on the day of their wedding, as the bride emerged from the veiled sedan chair that had fetched her away from her home.

These and other fascinations abound in the study of the Chinese family. But until now there has not been a general introductory treatment suitable for describing these fascinations. Many general articles exist, as well as the introductory textbook chapters, but they tend to be superficial and often idealized. On the other hand, there has been a proliferation of specialized monographs on aspects of Chinese family life in recent years,

but these cannot be readily recommended for nonspecialists. Thus the appearance of *Chinese Family and Kinship* is a welcome event. Hugh D. R. Baker is an anthropologist and lecturer in Chinese at the University of London. His fieldwork in a Hong Kong village resulted in a monograph, and he has also been writing a regular series of newspaper articles on Chinese customs for a number of years. Baker's familiarity with the scholarly literature and his skill in communicating to the general reader make him an ideal candidate for the task of presenting a comprehensive description of Chinese family life. And this work is highly readable at the same time that it accurately reflects the state of our knowledge about the dynamics of Chinese family life. Most of the central topics are covered, including family structure, the life cycle, the role of women, lineage organization, ancestor worship, and the role of kinship in the larger society. There is little in the book that is new to specialists of the Chinese family, but as a general treatment for family sociologists and general readers it earns high marks.

If there is a major problem, it involves the regional and time focus that Baker utilizes. Most of the book focuses on the "traditional" family, which means the forms that existed in the 19th century when Westerners first began to scrutinize the institution intensively. Also, the material covered is heavily slanted toward the versions of Chinese family life that existed in Southeastern China, particularly in Hong Kong, where Baker and most others have had the opportunity to do research. Despite some broad uniformities in Chinese family life throughout the country, the forms and customs in Hong Kong and Taiwan are not the same as everywhere else, and they do not necessarily represent the most typical or "fully developed" forms of Chinese family life. The final chapter discusses 20th-century trends in family life in Hong Kong, Taiwan, and Mainland China, but the coverage is too brief to give an adequate picture of how the Chinese family has adapted to different versions of the modern world. The topic of how the Chinese family has been altered in different settings deserves much fuller treatment than Baker gives it. Admittedly information on family changes in the People's Republic of China has been scarce up until now. However, the appearance of several new studies and the permission the Chinese government granted several Western anthropologists recently to carry out village studies in the mainland should help fill this information gap. We can hope that Baker's book will do well enough to enable him to bring out a revised version in the future and that in his revisions he will try to redress the balance by dealing more heavily with recent trends and with other regions of the country. Even as it stands, *Chinese Family and Kinship* is first rate and does an admirable job of conveying the many fascinations of traditional family life in China.

The Rainbow Serpent: A Chromatic Piece. Edited by Ira R. Buchler and Kenneth Maddock. The Hague: Mouton Publishers, 1978. Distributed by Aldine Publishing Co., Chicago. Pp. x+227. \$32.95.

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From the mid-19th century onward, explorers, missionaries, and anthropologists described indigenous religious life in many parts of Australia. Their accounts of rituals and myths made frequent reference to the aborigines' beliefs in the existence of supernatural serpents or other reptilian creatures that appeared to occupy a significant place in tribal or regional cosmologies. Although in both the primeval dreamtime and the historic present these mythic beings were often associated with rain, rainbows, waterholes, sexuality, and the swallowing of people, such characterizations were by no means invariant or unequivocal. Indeed, despite the manifest importance of these creatures in aboriginal thought, the considerable ambiguity that attended their depiction (whether in myth, song, ritual, or art) greatly puzzled the earlier European investigators, few of whom analyzed in any depth the meanings or symbolism of the Rainbow Serpent and its analogues.

In 1930 several anthropologists who had worked among aborigines participated in a modest symposium organized by Radcliffe Brown on the role of the Rainbow Serpent in aboriginal religion. Given the then functionalist temper of Australian social anthropology and, especially, of the convenor, it is not surprising that the conclusions presented in the published papers were, despite certain disagreements, relatively unproblematic. The social functions of the concept of the Rainbow Serpent were confidently asserted in a manner that for years largely inhibited further speculation in this area of aboriginal religion.

Since the 1950s, however, the rapidly growing body of intensive field studies of systems of aboriginal religious belief and ritual has produced a wealth of data that cannot readily be accounted for in terms of simple social-functionalist hypotheses. One consequence has been that, within the past decade, a number of anthropologists have turned to structuralist modes of analysis (whether French, Dutch, or British eclectic) both to order and to illuminate the rich complexity of the aboriginal materials. *The Rainbow Serpent* exemplifies this shift of interest in that two of the three authors retrieve from the functionalist limbo a significant problem whose subtleties were not fully perceived in the 1930s, and they attempt to identify and clarify them in terms of structuralist methods and arguments.

The book, which appears in the World Anthropology series, comprises four sections, each written by one of the editors or by the late Charles Mountford. In the introduction Kenneth Maddock sets out the problem. In a concise and lucid manner he indicates the ways in which earlier anthropologists have treated the image of the Rainbow Serpent, points

out what he takes to be the deficiencies of those approaches and explanations, and then suggests a line of inquiry that he thinks will be more productive. He concludes that Rainbow Serpent "is a partial, concrete and zoomorphic rendering of what might more abstractly be conveyed . . . as 'totemic essence', 'life principle', 'spirit' or 'divinity'. . . . All the forms assumed contextually by this underlying reality exhibit processes of transformation by which their appearances fluctuate. . . . It is better to address oneself to this thought-complex than to focus on one of the many transitory shapes" (p. 19). In a later section, "Metaphysics in a Mythical View of the World," Maddock applies this mode of transformational analysis to his own and to other anthropologists' data drawn from a group of aboriginal societies in northern Australia. In a persuasive demonstration of the explanatory utility of this approach, he shows that what at first sight might be taken simply to be an inchoate and ambiguous notion of Rainbow Serpent among these people is instead an intelligible expression of a complex metaphysics of being and becoming that ramifies through and informs their cosmology.

The long section 2, "The Rainbow-Serpent Myths of Australia" by the late Charles Mountford, has a very different, almost old-fashioned aim. Using data from his own extensive fieldwork and from other writers' works (including photographs and aborigines' drawings), Mountford provides a geographical survey of the incidence of Rainbow Serpent beliefs across the continent, together with summary accounts of many of the myths. Primarily a work of empirical description with few theoretical pretensions, the synopsis is a valuable compendium of evidence and references. At the least it should stimulate the interest of investigators of aboriginal religion, whatever their theoretical disposition.

The final section (some 90 pages), "The Fecal Crone" by Ira Buchler, is in his words "the first step toward a structuralization of the Murngin corpus and, eventually, a structural synthesis of Australian mythology and praxis" (p. 119). To this end Buchler takes from the many publications on the Murngin a number of myths (including some about serpents) and other ethnographic data, which he subjects to a full-blown structural analysis, replete with diagrams, in the French mode. The paper is too long and far too complex to be summarized here—a task by no means facilitated by the author's exuberant and at times overly cute literary style. Suffice it to say that his essay is the most elaborate and thoroughgoing attempt to date to analyze aboriginal myths in the manner of Lévi-Strauss, and for its obvious and sustained ingenuity it merits serious, critical examination. I suspect that the reader's reception of it will be conditioned largely by his opinion of Buchler's basic assumption that meaning "is a matter of internal coherence, the transcendental deduction of mutually convertible significations on a variety of levels" (p. 131).

The Rainbow Serpent will, I believe, be of interest to a wider range of scholars than those specifically concerned with social-anthropological research among the Australian aborigines.

20th Century Gothic: America's Nixon. By Sherri Cavan. San Francisco: Wigan Pier Press, 1979. Pp. ix+330. \$15.00.

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For those of us who have had an uneasy feeling that the saga of Richard Milhous Nixon should provide us with more than a mere surface history of the man who became the first president of the United States to resign the office, Sherri Cavan's sociopsychological analysis, *20th Century Gothic*, must be seen as an important step in the right direction. Cavan's approach to the rise and fall of one of our era's most complex and contradictory individuals is to treat his career as a chapter in the unfolding drama of modern American life. Her portrait of Richard Nixon assumes that personality or character development is a progression affected by the forces of history on the one hand and the forces of the life cycle on the other. In the dark vision from which Cavan writes, Nixon is compared with Hitler as a political person produced by a "Gothic" national culture who eventually comes to dominate that culture. By Gothic Cavan means the weltanschauung that sees evil lurking around every corner in any challenge to the established order, whether it be religious, social, economic, or political. If the reader accepts the thesis that in times of rapid social change a sinister view of reality takes root, that many anxious people come to believe in it, and that eventually even virtuous men are driven to the extremes of nihilism in defense of traditional morality, then this analysis has great power.

Cavan has accepted the research challenges implied by such a thesis and has undertaken a dual search for the forces affecting this man and his country and the changing, yet interwoven, character of each. It is a monumental task, breathing new life into studies of this genre. As such, it is not surprising that there are some flaws; however, on the whole, the enterprise is satisfying and edifying, both substantively and methodologically.

Cavan's argument is complex and sophisticated. She has an impressive ability to take the many strands of a man's life and a nation's socioeconomic development and pull them together into an understandable gestalt. Nixon is seen as a product of his time in a country that emphasizes success and material goals over the more philosophical ideals of integrity, democracy, and general human welfare. Furthermore, as American economic life moved toward corporate and conglomerate control, people who failed to attain material success and were thus on the outside looking in were feared by those more fortunate as a dangerous challenge to the status quo. This polarization was exacerbated by the advent of the youth rebellion of the sixties. A product of small-town America who made it up from relative poverty, Nixon identified with the "haves." He was sponsored politically by the conservative establishment as one of their useful

"salesmen." Early on, he gave evidence of a mentality in which dissenters were to be feared—first as a general threat to the way of life he hoped to achieve, and later as a personal and specific threat to his own individual success in obtaining or maintaining power. Cavan suggests that Nixon's mounting paranoia eventually led him to betray the very moral order he claimed to be defending. Further, she declares that many Americans were (or are) doing the same, apparently believing as did Nixon that constitutional betrayal for the sake of preserving what they think is rightfully theirs is justified.

Data for this study were gathered from many sources, including autobiographies, biographies, material in Nixon's hometown of Whittier, California, interviews with significant people in both Whittier and San Clemente, and microfilm editions of newspapers available in Whittier which report both local and national events from their own special perspective. Cavan drew Watergate material from many media sources—newspapers, news magazines, television broadcasts of the hearings, autobiographies and other accounts of those days, and the White House transcripts. (One wonders why more sociolinguists have not attempted analyses of the latter, a rare gold mine of talk in high places.)

Culture-personality and national character studies were initially the province of psychologists and psychologically oriented anthropologists and had their heyday in the fifties. They focused on both individuals and entire cultures or nations. Although Cavan claims to have received some guidance in her methodology from Erik Erikson's *Life History and Historical Moment* (New York: Norton, 1975), she is not restricted by a Freudian framework and can be more catholic in her selection of data. Cavan has a bias of her own, however. Obvious sympathy for the political left/liberal viewpoint comes through in the selection of some data and in some areas of the analysis. This is far from a fatal flaw, however. Cavan is careful to qualify many of the possible causal links as hypotheses, rather than "truths" (a carefulness lacking in many of her predecessors), and as such they are indeed plausible.

After the introduction, the book is divided into three parts. Chapters 2–6 are concerned with the chronology of Nixon's life as it intersects with America's socioeconomic development. Although informative, they are mainly descriptive. Chapters 7 through 10, which fall under the general heading "Themes," deal primarily with the sociology of the small group that made up Nixon's close associates, his record of increasingly serious betrayal of others, and his concern with public relations rather than public service. A revealing linguistic analysis of metaphor, innuendo, and, literally, gangster talk in the Oval Office is also included. The analysis of power lines, both formal and informal, among Nixon's staff and the discussion of the place of women in the Nixon administration are most enlightening. Drawing primarily from the material in earlier sections, Cavan then devotes two chapters to what she calls "Reflections." One, titled "Nixonian Logic," is a short but persuasive excursus on how Nixon's mind seems to work based on both his pronouncements and his behavior. Finally, Cavan

compares, more successfully than one might expect, Richard Nixon and Adolph Hitler as Gothic antiheroes. Although Nixon's crimes seem petty beside the monumental villainy of Hitler, both leaders had the we/they mentality and used it to arouse the electorate, and both were more interested in selling the citizenry images than in informing them.

Cavan writes gracefully and well. The book is somewhat uneven, however, both in quality of argument and in logic of presentation. There are short paragraphs of analysis divided by asterisks and lacking transition between topics, making for choppy reading at times. Cavan makes a better case for Nixon's unique life experiences as causes of his character development than she does for the socioeconomic development of the country as Nixon's outlook writ large. It is hard to accept her judgment that this nation is in the unrelenting grip of a Gothic perspective when it has supplied us with Judge Sirica, Peter Rodino, Sam Ervin, Elizabeth Holtzman, and Barbara Jordan.

Although Cavan's entire thesis is not totally convincing, it is important simply because she enunciates clearly a current in our national culture and makes her case strongly enough that a reader must constantly give it serious consideration. The fundamental forces that produced Nixon have not disappeared. And that is Cavan's message. In the methodological area, she has definitely broken new ground. Personality-national character analyses have been wrested from the Freudian orientation that has dominated such works and given a new, more comprehensive direction. The entire life cycle (not just childhood and early youth) gets its analytic due, and the multiple forces that are possible influences on both individuals and societies are considered. Though Cavan may not agree, I believe this may be the ultimate triumph of her study.

Celebrations of Death: The Anthropology of Mortuary Ritual. By Richard Huntington and Peter Metcalf. New York: Cambridge University Press, 1979. Pp. xv+230. \$21.95 (cloth); \$6.95 (paper).

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The discussion of death has become fashionable. Courses on death and dying have been introduced into grammar schools, high schools, universities, and churches. Thanatology has become a discipline with its own scholars, journals, and professional societies. Popular fascination with various aspects of death has kept books by Elisabeth Kübler-Ross (*On Death and Dying* [New York: Macmillan, 1969], *Death: The Final Stage of Growth* [Englewood Cliffs, N.J.: Prentice-Hall, 1975]) and Raymond A. Moody (*Life after Life* [New York: Bantam, 1975], *Reflections on Life after Life* [New York: Bantam, 1978]) on the bestseller lists for months.

A sociology of death and dying has been slower to emerge. (The first session on death and dying ever scheduled at an American Sociological Association national meeting occurred only in 1975.) Although much of the recent literature may be interesting to sociologists, most of it is either journalistic reportage, psychological self-help advice, or supernatural speculation rather than rigorous sociological analysis. This is not to suggest that a sociological literature on death and dying does not exist, but it still must borrow freely from the work of historians and anthropologists.

Anthropologists and archaeologists have much to say about death rituals in human evolution largely because bones and burial artifacts are the only evidence to survive early cultures. Anthropologists Richard Huntington and Peter Metcalf offer sociologists a chance to illuminate our own society's treatment of death in the light of ethnographic data from non-Western cultures. They point out the particular value of studying the social rituals surrounding death: "The issue of death throws into relief the most important cultural values by which people live their lives and evaluate their experiences. Life becomes transparent against the background of death, and fundamental social and cultural issues are revealed. . . . Death and its rituals not only reflect social values, but are an important force in shaping them" (pp. 2, 5).

Huntington and Metcalf intend *Celebrations of Death: The Anthropology of Mortuary Ritual* to be neither an encyclopedia of the world's funeral customs nor a history of the anthropological study of death rituals. Rather, they explore several issues in the anthropology of death ritual through illustrative case studies. In part 1, "Universals and Culture," they examine attempts to explain funeral rites and death-related behavior by some universal cause—be it the emotions of fear and sorrow or a common symbol, such as the corpse. They conclude that all such postulated universals are too variable or too ambiguous to provide a general explanation. In part 2, "Death as Transition," they consider death rituals as rites of passage, in which the identities of the dead and closely related living must be readjusted. Since community life must continue beyond individual deaths, death rituals must reaffirm life values, commonly through symbols of sexuality and fertility. Part 3, "The Royal Corpse and the Body Politic," examines the social, political, even cosmological implications of the deaths of leaders, chiefs, pharaohs, and kings. In societies in which royalty embodies the authority and perpetuity of the political order, the death and putrefaction of the monarch's mortal person represent a fundamental challenge to that order. Death rituals must dramatize an orderly continuity: "The king is dead! Long live the king!"

Huntington and Metcalf's concluding chapter brings the theoretical insights gathered from their comparative examples to bear on American funerary practices. Considering the variety of death rituals around the world, and the cultural heterogeneity of American society, one would expect wide variation in funeral practices from one region or social class or ethnic group to another. Instead, funeral practices in America are re-

markably uniform, generally typified by the rapid removal of the corpse to a funeral home, embalming, "viewing" of the corpse, and burial. Such uniformity seems to imply a common religious ideology, but none is readily apparent given the array of denominations and the multitudes of the unchurched. Huntington and Metcalf locate this common religious ideology in what Robert Bellah has called the American civil religion ("Civil Religion in America," *Daedalus* 96 [1967]: 1-22); indeed, they characterize it as "a death-centered religion." It is the same ideology that Lloyd Warner saw unifying American communities through Memorial Day ceremonies which culminated in the cemetery (*American Life: Dream and Reality* [Chicago: University of Chicago Press, 1953]). This portrayal of funeral practices as rites of the American civil religion is such a striking and provocative linkage that the reader can only wish that the authors had allowed themselves more than three pages to pursue it.

Meticulous methodologists may question the authors' sampling techniques: while understandably disavowing an encyclopedic treatment of world funeral practices, Huntington and Metcalf select examples either because they are among the classics of anthropology (from the work of Durkheim, Evans-Pritchard, Frazer, Radcliffe-Brown, and others) or because they are among "those areas that we [the authors] know best" (p. 184). The authors acknowledge "a strong tendency on the part of anthropologists to leap from minute and unique social fields toward global conclusions" (p. xv), but they never confront this problem of sample representativeness in their own work. Nevertheless, sociologists interested in death and dying in America would do well to examine their theoretical insights for domestic applications.

Social Movements and the Legal System: A Theory of Law Reform and Social Change. By Joel F. Handler. New York: Academic Press, 1978. Pp. xiv+252. \$17.00.

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During the past 25 years, litigation has become an important weapon among reformers seeking leverage on the established social order or other forms of progress toward social change. Joel Handler has constructed a theory designed to explain why some attempts to use law as an instrument of social change are more successful than others. Five groups of variables affect the outcomes of law-reform activities by reform movements: (1) the characteristics of social reform groups (What incentives help leaders mobilize support for their efforts?); (2) the distribution of the benefits and costs of the changes proposed by reformers (Are benefits and costs small and widely dispersed in society, or are they large and concentrated in special-interest groups?); (3) the "bureaucratic contingency" (If the

law reformers win, will compliance with their demands require action by a large array of bureaucratic authorities who have considerable administrative discretion?); (4) characteristics of judicial remedies (Are effective, enforceable remedies available?); (5) characteristics of the law reformers (Are well-supported lawyers available with adequate technical and political resources?). In *Social Movements and the Legal System* Handler tests this theory against information provided by 38 case studies in four general areas: environmental litigation, consumer protection, civil rights, and social welfare.

Few of Handler's 38 narratives present success stories; the obstacles to legally stimulated change are formidable. The bureaucratic contingency looms particularly large. Unless bureaucrat-proof remedies can be fashioned by judges or other strategically placed officials, bureaucrats with discretion can evade both the purpose and, for that matter, the letter of the law. The "free ride" also presents a problem. Since all can benefit from changes for which only a few have sacrificed, how can support for change be motivated? This is an especially important problem when the benefits of change are small and widespread and the costs must be borne by a particular special interest.

Handler is not insensitive to the possibility of indirect benefits of legal strategies—publicity, leverage, tactical advantages, consciousness raising—or even long-run indirect enhancements of pluralism in American society—more effective advocacy of currently underrepresented interests and groups. Still, he is, on the whole, skeptical. The picture he paints is (to quote Mayer Zald's surprisingly critical introduction to the volume) "gloomy."

I have a nagging suspicion that Handler's skeptical accounts and dismal forecasts have deep ideological roots. As Zald suggests, Handler "identifies with the 'outs' and has studied situations where the 'outs' have not become the 'ins'" (p. xi). I would go further: Handler appears to believe that all the claims of his law reformers are valid and just. The *relative* legitimacy of their various claims and demands is not a variable in the theory. Thus he leaves the reader no reason to suppose that there is any essential difference between, for example, the right of blacks in the South to vote and the right of welfare recipients to more money than they currently get. The former claim derives from the fundamental meaning of democracy; one of the welfare claims that Handler discusses was avowedly based on a tactical attempt to bankrupt a welfare system. There is no indication that Handler thinks this issue has any sociological relevance. Indeed, although he discusses legitimacy, he distrusts the whole concept, fearing that appeals to ethical premises serve more to reassure the underprivileged than to give them anything tangible. He even goes so far as to assert that "there is nothing in the empirical literature to suggest that legitimacy is very powerful" (p. 218). He complains that the impact of legitimacy is hard to pin down in a precise way in research. This may well be true, but he does not even try. The impact of legitimacy cannot

be studied if it is assumed to be a constant—if whatever the “outs” say they want or deserve is equally justifiable. Handler’s narratives usually suggest that opposition to reform is based upon selfish interests or obstinate bureaucratic complacency. He never suggests that there may be two sides to some question or that compliance might be realistically difficult even for those who want to comply and who themselves have a vested interest in change. More generally, Handler’s list of variables does not stress differences in the position of the targets of social change. Relative differences in their legitimacy, their capacity to comply, their structural location, or their political support are ignored.

Concern for clear empirical evidence, which makes Handler wary of concluding that legitimacy has much significance, is praiseworthy. On the other hand, his own study must confront a serious research problem: what is “success”? His entire study, considered as an essay in empirical sociology, clearly hinges on the definition of its dependent variable. Yet “success” is a very slippery concept. Handler discusses the problem at some length (pp. 34–40) but does not achieve a definitive operational measure. In practice, in his concrete accounts of instances, he uses a sliding measure, reckoning some outcomes as achievements but always pointing out that there are still evils to be corrected, that the battle has not been fully won. He gives the impression that his own view is that the establishment is still in control and that, in this sense, none of the efforts of law reformers have come to much. Again, the ideological roots of this position are apparent.

The book is not without value; I learned a great deal from it. But the asymmetry of Handler’s approach does not inspire confidence in the capacity of the sociology of law—always an ideological battleground—to transcend adversary positions.

The Hidden Economy: The Context and Control of Borderline Crime. By Stuart Henry. London: Martin Robertson & Co., 1978. Distributed by Biblio Distribution Centre, Totowa, N.J. Pp. xi+194. \$16.00.

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This little volume will eventually find its place on many bookshelves next to such works as Mary Cameron’s *The Booster and the Snitch* (London: Collier-Macmillan, 1964) and Donald Cressey’s *Other People’s Money* (Glencoe, Ill.: Free Press, 1953). Stuart Henry’s *The Hidden Economy* is an enlightening and thought-provoking examination of what appears to be an international pastime. The “hidden economy” consists of “fiddles” and “perks,” claimed by some to be the oldest of property crimes and, as Henry argues, an “everyday feature of ordinary people’s lives.” “Fiddles” are a variety of acts ranging from pilfering to filching; “perks,” a

class of fiddles, are "fringe benefits" of the job, be they paper and pens from the office for home use, or enough parts from the assembly line to build a replica or two of the company's product. These activities are viewed as part of an exchange system of goods and services among friends, some of whom may at times "depend" on such "cheap" bargains to "get along." But, according to Henry, "there is much more to the hidden economy than the pursuit of gain" (p. 6). It is just this that Henry attempts to show by adeptly leading the reader into the hidden economy through the first six of eight chapters. In doing so, he draws frequently from his previous research and writings on the subject and the work of several of his colleagues and sometime collaborators, snatches of historical materials, and tape-recorded interviews of 20 participants. In his two concluding chapters, Henry first examines traditional attempts to control the hidden economy by means of "private justice" and courts of law. Then he argues the fruitlessness of such methods and proposes a well-reasoned alternative calling for a form of communal control.

Chapter 1 includes a discussion of basic concepts and notions which guide the analysis, a description of the historical bases of the hidden economy, and a very brief statement of the research strategy and the sources of information. Chapter 2 is largely descriptive, focusing on the time, place, and participants in fiddles and perks, and on the formation and maintenance of "trading networks" operative in a variety of contexts ranging from the office to the local pub. "Dealers" and "stealers" and the "process of becoming" are described, often in the participant's tongue, in a successful attempt to illustrate both the verbal and nonverbal language of the economy. Various underlying themes are also explored, each of which is to be elaborated upon in subsequent chapters ultimately leading to the "unveiling" of the hidden economy in chapter 6.

One of the principal themes, developed in chapter 3, involves conceptions of honesty and dishonesty. Here Henry uses the terms "rationalization" and "neutralization" to argue that, in one sense, the language of trading networks permits the hidden economy to thrive by providing participants with explanations for their behavior. Noting the influence of both Cressey and David Matza (*Delinquency and Drift* [New York: Wiley, 1964]), he posits that "the concepts, words, and phrases we use in our everyday conversations may themselves hide the illegality of the hidden economy and therefore render us unwitting participants in illegal acts" (p. 55). In this context, Henry parallels Matza's discussion of "techniques of neutralization." In fact the latter serves as a guide for Henry almost to the point of distraction. While Henry is to be complimented for his illustrative material, this discussion is the most wearisome section of the work, particularly when compared with the more insightful and creative chapters to follow.

It is in chapters 4, 5, and 6 that we are treated to the most exciting aspects of Henry's analysis. In chapter 4 he speaks of criminal stereotypes derived from both academic and media commentators which conjure im-

ages of depravity and greed and which, in turn, obscure the true nature of the hidden economy. Further, these images provide substance to the rationalizations which legitimate fiddling and justification for the "honesty" of the participants whose activities neither serve as a principal source of income nor operate on the same scale as is the case for professional thieves and fences. Certainly they are not part of an oppositional culture. According to Henry, and he is convincing, it is just because the "ordinary people" being discussed are not "committed to a counter-culture . . . [that] they form their own personal moral sense of what is right and wrong. . . . Personal morality rests on doing things to help other people" (p. 78). Later, in chapter 6, he argues in a somewhat similar vein that the hidden economy is also misconceived as a "market economy" driven by the money motive. Here he notes that, despite surface appearances, the "pricing policy . . . is determined less by market values than by the closeness of the relationship between trading partners" (p. 91); the "rewards" are social rather than monetary, coming largely in the form of "competitive play" and "reciprocity." In chapter 5 Henry describes in detail the informal nature of trading and bargaining for "cheap" goods through closed and close networks of friends—networks in which the ultimate sanction is that of exclusion due primarily to "failure" to participate fully in the first place.

In the last two chapters Henry looks first at traditional efforts to control the hidden economy. Then he pulls together the various strings threaded throughout his analysis to describe and justify his alternative model for control. Legal controls will fail, he argues, given acceptance of the terminology of the market economy and related assumptions of rational, money-motivated behavior and the legal conceptions of dishonesty derived from criminal stereotypes. Private justice (e.g., security forces), on the other hand, will fail because of potential abuses ranging from the inconsistency of arbitrary decisions to the exploitation of employees. In each case, he adds, the costs will be enormous in terms of time and worker response. Henry argues instead for "planned use of normative regulation" in the form of community controls utilizing community courts in which employers and employees are equally involved. "By allowing *equal* representation and involvement at *all* stages of the administration of law, both offender and offended are offered justice" (p. 173). Self-regulation, while popular in the East and eastern Europe, remains largely untried in the West. Henry is quite convincing in his argument that it at least be tried. While some may find Henry naive, the strength of his analysis and interpretive framework lends credence to his position. If nothing else, his critique of traditional modes of control is instructive.

Class and Merit in the American High School: An Assessment of the Revisionist and Meritocratic Arguments. By Richard A. Rehberg and Evelyn R. Rosenthal. New York: Longman, Inc., 1978. Pp. xi+337. \$7.95 (paper).

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The interrelationships between educational institutions and systems of stratification are of central theoretical and empirical importance to the sociology of education. The past two decades have seen a proliferation of works devoted to this topic, and Richard Rehberg and Evelyn Rosenthal's *Class and Merit in the American High School* falls squarely in this tradition. As a research monograph focusing on educational outcomes at the individual level and on educational processes internal to the school, its goal is to test the relative validity of two opposing models of education and society: one views education as a means through which class privilege is passed from the elites of each generation to their children; the other views education as a means of distributing societal rewards on the basis of merit.

The analysis is based on data collected by Rehberg between 1967 and 1971 in several schools in New York state. The data are longitudinal. The first wave was collected when the students were ninth graders; the last when the majority had been out of high school for several months. In all, 2,788 students participated. Survey questionnaires were used throughout. The schools were urban, suburban, and rural; public and parochial.

The book contains nine chapters, three appendixes, and all the research instruments. In chapter 1, Rehberg and Rosenthal lay out their conceptions of the "meritocratic" and "revisionist" perspectives. Finding a set of school-process variables (curriculum placement, interpersonal influence, guidance and counseling, educational ambition, and academic performance) common to both, they propose to test the relative merits of the two perspectives through an assessment of "the magnitude of the total causal effect of class background" and "the total causal effect of scholastic ability and other indicators of merit on the school-process variables" (p. 21) in their sample. Chapter 2 covers design and methods and includes a detailed discussion of the use of causal models.

Each of the following five chapters concentrates on a particular aspect of the data. Chapter 3 examines the students as ninth graders. The path analysis explores the extent to which curriculum location and educational ambition are affected by social class and scholastic ability. Chapter 4 looks at grade 10. Curriculum placement in tenth grade and counselor's educational encouragement are added to the model. Chapter 5 focuses on academic achievement as measured by students' grades. Chapter 6 details the impacts of all the previously analyzed process variables on the students' twelfth-grade postsecondary educational decisions. Chapter 7 looks at actual enrollment in postsecondary education after graduation. Throughout,

findings are presented separately for men and women. Chapter 8 is used to discuss some of the gender differences and to introduce gender into the path model as an independent variable. Chapter 9 provides a summary of findings, conclusions, and a discussion of implications.

The authors' principal finding is that the data support the meritocratic model of educational processes and refute the revisionist model. They conclude that, in their sample, "it was individual merit more than family social class that influenced progress from grade nine to grade twelve and subsequent entry into college or the labor market" (p. 254).

The major strengths of this book are the clear and concise manner in which the analysis is developed and the importance of the topic addressed. The rationale behind each analytical decision is made explicit, and data showing the impact of alternative strategies are presented. The book would be excellent for use in a methods class as an example of the use of causal models or in a sociology of education course in which greater appreciation of these methods is desired.

The major limitation of the book is its tendency to exaggerate the revisionist position. According to Rehberg and Rosenthal's interpretation, even evidence they uncover of the persistence of class-linked educational advantage is viewed as compatible with the meritocratic rather than the revisionist model, since the advantage is not deemed "large." One author, cited in chapter 1 as representative of a rather extreme depiction of revisionist thought, is cited later as having in fact taken a more balanced view in his recent work, but this citation is merely an aside. In general, the book suffers from a paucity of theoretical discussion. Variations among representatives of the revisionist perspective are not covered. Problems involved in operationalizing these perspectives are barely touched upon. The use of empirical data by revisionists to support or modify their ideas is not included. Consequently the unsophisticated reader is likely to come away with an almost derisive attitude toward revisionists and a false complacency regarding the degree to which the issues they raise have been definitively laid to rest.

In sum, Rehberg and Rosenthal have given us a well-developed statistical analysis of data bearing on an important sociological issue. It is a valuable empirical study, but unfortunately it does not do justice to the broader theoretical and methodological context within which it must be evaluated.

Worlds Apart: Relationships between Families and Schools. By Sara Lawrence Lightfoot. New York: Basic Books, 1978. Pp. xiii+257. \$12.95.

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Much of the contemporary rhetoric among school reformers and some social scientists is based on the view that only poor and minority parents are

excluded from their children's school life, despite the fact that several studies have documented the prevalence of institutionalized defense mechanisms to exclude parents in a variety of contexts. More often than not, these patterns have been interpreted, from the teacher's perspective, as functional (they reduce role conflict and establish her authority).

Therefore, it is a relief to find that Sara L. Lightfoot's analysis of patterns of exclusion not only recognizes their universality but also deals with their dysfunctions for families. Her discovery is based on observations and interviews, in slum and middle-class settings, which revealed that while educators stress the need for home-school cooperation, most schools have instituted arrangements that "systematically exclude parents" in order to establish "boundaries between insiders (teachers) and interlopers (parents)." These patterns reflect the teachers' need for autonomy and the belief that early separation from the family is in the child's best interest, frequently justified in terms of negative stereotypes that undermine families and mothers.

This contradictory situation prompted Lightfoot to search for evidence on the dynamics between families and schools. To her "dismay," she found that social scientists have largely ignored these relationships and rarely considered the participants' perspectives. Theorists have created conceptual dichotomies that rationalize separation of the two spheres and socialization theories that contribute to and legitimate negative images of families.

The consistency of the research findings led Lightfoot to conclude that the origins of contemporary conflict between families and schools are multidimensional and "cannot be reduced to contrasts in race and class" (p. 34). *Worlds Apart* consists of five chapters which explore some of the structural and cultural origins of the conflict from a sociohistorical perspective and which identify past adaptations that suggest alternatives for future reforms.

Lightfoot analyzes the negative images surrounding three groups: teachers, mothers, and black families. The central thesis is that these negative images reflect and perpetuate inequities rooted in American social and economic institutions. Cultural images, which shape perceptions and influence behavior, are the product of collective myths, idealized role expectations, and past experiences; thus they include positive and negative elements and frequently contradict historical realities and everyday life.

Data derived from historical records, social science literature, and interviews with mothers and teachers suggest that the tensions between mothers and teachers result from five sources: role ambiguity, discrimination, the discrepancies between idealized role expectations and reality, social change, and the failure of the schools to create opportunities for meaningful interaction.

Chapter 1 deals with the differences in structural properties and cultural purposes which support separation as well as articulation of home and school. While the structural-functional perspective has contributed to our understanding of the boundaries between families and schools, it has fostered research methods which isolate the two spheres. Rigid dichotomies

which assign socialization and affective relationships to the home and education and specialized relationships to the school are inconsistent with reality. The delineations are not perceived by mothers and teachers, who are frequently caught up in a struggle over "boundaries, territories, and spheres of influence" (p. 221).

To understand the basis of competition between mothers and teachers, each role is examined (chap. 2) in terms of historical images, role definitions, and psychological theories. On the one hand, mothers and teachers have been exalted in the literature and myths, and their roles have been invested with superhuman expectations. On the other, their roles have been devalued in this society, and their behavior constrained by norms which foster passivity and self-denial. At the same time that technological developments have enhanced the value of education and expanded the teacher's role, they have diminished the mother's role. Psychoanalytic theories have contributed two interpretations of maternal anxiety over the child's schooling: mothers who work are "overinvolved" in order to relieve guilt; those who stay home are unfulfilled and "overidentify" with the child.

Opportunities to resolve these tensions through "authentic interaction" are rarely provided in school settings where interactions are contrived rituals. Teachers are constrained to project an image of "smooth control"; parents fear that criticism will have a negative effect on the child's relationship to the teachers. "Their generalized low status makes them perfect targets for each other's abuse, not daring to strike out at the more powerful and controlling groups . . . responsible for their demeaned social and economic position" (p. 69).

A third chapter, devoted to two teachers' descriptions of their relationships with parents, illustrates how macro-level processes influence micro-level interactions. The data indicate that teachers feel most threatened by middle-class professional parents and black families on welfare, who they see as most likely to criticize their performance or resist their authority. Exclusion of the former is usually based on one of the above psychological syndromes; exclusion of blacks is usually based on sociological images which place the blame for the child's school failure on the family. Teachers denigrate the mother's child-rearing practices and perceive the family as disorganized and deviant (they do not "value education").

When researchers have considered family-school relationships, Lightfoot asserts, families are usually viewed from a middle-class bias. The focus has been on "pathology and deviance and the dominant theme is the abuse of mothers" (p. 13). Social science theories based on distorted images of black families are "subtle and sophisticated . . . strategies for the domination and control of blacks" (p. 130)—the subject of chapter 4—and contribute to and reinforce the teachers' stereotypes. Evidence from revisionist histories and research conducted from a black perspective challenge traditional views that fail to consider the strengths of black families, their exclusion from formal schooling, their high regard for educational attain-

ment, and the sources of their ambivalence about schools (schools are agents of liberation and alienation).

Reform, the historical evidence suggests, involves conflict, redefinition of roles, and structural change. While the process is slow, schools have been transformed when powerless groups "view themselves as active, vocal and initiating" (p. 185). "Community members and teachers will only begin to dismantle their stereotypic roles and reveal their individuality if the social settings in which they gather provide opportunities for interaction" (p. 200). To maintain the negative dissonance between families and schools is "dysfunctional to the child's self-definition, maturation and movement from one environment to the other" (p. 209).

This work is original and provocative. By looking at a contemporary problem from a historical perspective, analyzing relationships from divergent theoretical approaches, and focusing on the participants' point of view, Lightfoot presents a unique treatment of familiar material which underscores the complexities of social problems that defy simplistic solutions.

Citizens and Politics: Mass Political Behavior in India. By Samuel J. Eldersveld and Bashiruddin Ahmed. Chicago: University of Chicago Press, 1978. Pp. xii+351. \$26.00.

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Citizens and Politics is an important book, a major contribution to our understanding of political behavior in India, but it needs to be read carefully. Those who might be tempted to skim should be warned: the data do not say nearly as much as Samuel Eldersveld and Bashiruddin Ahmed often imply they do.

This is no reflection on the very impressive data. They are drawn from interviews conducted in 10 languages with male citizens following the 1967 and 1971 elections. These surveys—the first national surveys of the Indian electorate—were undertakings of heroic proportions, and they provide the basis for major advances in the study of Indian political behavior. Eldersveld, Ahmed, and many others connected with the Centre for the Study of Developing Societies in New Delhi deserve immense credit for their efforts.

In their book Eldersveld and Ahmed use these survey data to make several interrelated arguments. One is that the Indian electorate has the characteristics associated with electorates in "developed" or "mature" democracies. Although the cross-national comparisons are unsystematic, the authors cite much evidence to suggest that levels of political interest, knowledge, party identification, and political involvement in India are not so different from levels in countries like the United States or the United

Kingdom. A second argument is that party identification and political involvement are widely spread throughout Indian society and are linked to attitudes supportive of democracy and modernity. This argument blends into a third and more developmental one. Eldersveld and Ahmed suggest that new institutions, particularly political parties, have increasingly drawn people into the political process since 1947. This has led to more identification with parties, more sophisticated political involvement, and more support for the system. These trends, according to the authors, were still continuing in the late 1960s and early 1970s. A 1977 postscript triumphantly claims the 1977 election results as "the harvest, the validation of this developmental process" (p. 302).

Unfortunately, the analyses which support these important arguments are not without blemishes. Some of these are merely annoying: subsample tables without *N*'s, no mention of statistical significance even when using small subsamples, and complex path models built around very low correlation coefficients. Other shortcomings are more serious.

Some of the analysis seems outright misleading. A case in point is a study of "defectors," people who identify with one party but prefer another on the issue of government control of the economy (pp. 106-8). For subsamples in this analysis *N*'s are not clearly identified but are approximately 75, 20, 15, 13, and 8. Only when it comes to distributing an *N* of 2 across five categories do Eldersveld and Ahmed admit they have too few cases for analysis. The analysis then proceeds on the basis of a left-right alignment of parties, and defectors are assumed to be rational if they move to a party with an issue position similar to that of the party they are leaving. This seems to be exactly the wrong assumption, and the fallacious nature of the whole analysis emerges a few pages later (p. 111) in the next chapter when we learn that the left-right continuum is meaningless to most Indians.

Eldersveld and Ahmed repeatedly interpret questionable evidence as being in favor of their arguments. One justification for this is that a questionable piece of evidence is consistent with the general pattern of findings in the study. This is reasonable enough, but when detail after detail is questionable, one must begin to wonder about the general pattern as well. This problem is especially prominent in the discussion (chap. 19) of the "continued growth" in the maturity of the electorate between 1967 and 1971. Every major piece of evidence (increases in knowledge and efficacy, stronger correlation coefficients) is suspect because it is based on different electoral situations or different measurement techniques. Yet these alternative explanations are ignored, or they are rejected in footnotes on the grounds that a particular piece of evidence is consistent with others (p. 341, nn. 6-8).

Easily the most disturbing omission in the analysis is the failure to consider regional context in the final three-quarters of the book. Possible differences among states are simply not discussed. What makes this so astounding is that an early and all-too-brief chapter (chap. 5) clearly

demonstrates that there are pronounced differences among Indian states both in levels of party identification and other variables and in the relationships among these variables. In short, any simple discussion of the all-India level, and that means most of the book, is suspect.

By all means read this book if you are interested in Indian politics. It advances important arguments and provides a wealth of fascinating detail about Indian political behavior. But read carefully. Further analysis is very much in order.

Immigrant Labor and Racial Conflict in Industrial Societies: The French and British Experience, 1945-1975. By Gary P. Freeman. Princeton, N.J.: Princeton University Press, 1979. Pp. xiv+362. \$20.00.

Barbara E. Schmitter
Duke University

In the post-World War II period most advanced industrial countries in the West have experienced in-migration of foreign labor. While such migration to the United States is as old as the nation itself, large-scale and sustained employment of foreign labor is more recent in Europe. The social and political issues raised by immigration, however, often appear similar.

Immigrant Labor and Racial Conflict in Industrial Societies provides a detailed analysis of how two European countries, Great Britain and France, have attempted to deal with the sociopolitical problems of immigration. It is a welcome addition to the existing English language literature on European migration and its (often unforeseen) consequences. In an area where single-country case studies and/or economic analyses predominate, it is particularly welcome because of its comparative framework and its political perspective. Gary Freeman is aware of the basically economic causes of migration but suggests that "economics is the place to begin for an understanding of contemporary immigration, but cannot alone give a satisfactory explanation" (p. 312).

As a political scientist Freeman views immigration and its social and political consequences (i.e., racial tension and conflict) within the wider perspectives of crises in advanced industrial societies and the ability of elites to address them. His comparative framework permits him to identify differences and similarities in how the elites (primarily the state and its agencies) and counterelites (primarily trade unions and left-wing parties) have attempted to deal with the issues. While noting a recent overall convergence of immigration and racial policies in the two countries, he nevertheless identifies a number of important differences that he explains in terms of historical, structural, and cultural differences, for example, differences of "political culture," economic planning and manpower policy, and former colonial policy and administration. Such differences have r

mitted France to place immigration within a more concerted manpower and planning policy from the start, something the British have attempted to emulate but have by and large been unable to accomplish. Thus, while the British have attempted to deal with racial conflict by enacting special legislation concerning discrimination, the French have relied on their existing criminal code; while the British approach the welfare of the immigrant population through existing welfare agencies and provisions applying to all citizens, the French have created special agencies to deal with the immigrants' problems of adjustment in general or the more specific problems of housing and schooling.

As indicated in the title, Freeman is less concerned with immigration in general than with immigration and racial conflict. However, limiting his analysis to "racial conflict" presents a problem for the comparison at hand. Although France and Great Britain employ the largest numbers of nonwhite or racially distinct immigrants among major European immigration countries, the relative proportion of white (or European) to nonwhite (or North African, West Indian, Indian, or Pakistani) differs. Only one-third of the resident immigrant population in France is of non-European origin; in Britain nonwhites outnumber Europeans two to one. Furthermore, while the majority of British immigrants have also been British subjects, the same does not hold for France. The author appears to be aware of these crucial differences, but he treats them too lightly. Thus, independent of the other variables, the differences in sheer numbers of racially distinct immigrants who also happen to be British citizens must account for some of the differences between British and French policies: the greater salience of "race" in Britain is clearly related to the fact that the majority of the immigrants there came with full citizenship rights and have consequently posed greater problems. Freeman seems to recognize the importance of citizenship but does not treat it as an independent variable or explicate its meanings precisely. For example, it might have been useful to include a comparative analysis of the nature of citizenship, entitlements, and naturalization policies in the two countries. Related to citizenship is the question of the colonial legacy. Freeman argues that differences in former colonial policies ("indirect" vs. "direct" rule) are linked to present migration policies. While this argument is clearly correct, it should have been extended to cover the postcolonial relationships between sending and receiving countries. The specific nature of decolonization in the French-Algerian case and the postcolonial relationship between these two countries are very different from the British experiences. As an independent nation Algeria has pursued an aggressive policy aimed at the protection of its citizens, while simultaneously opposing their integration into French society. Algerian policies have clearly been complementary to the "melter" or "assimilationist"-oriented French immigration and/or migration policies.

Freeman presents an intelligent and knowledgeable analysis of British responses to the presence of racially distinct minority groups. A major shortcoming of the book is its lack of methodological rigor, something that the author appears to be aware of, pointing out

that his is not a "definitive" but rather a speculative study. His aim is not to test specific hypotheses but rather to generate them. While this approach has permitted Freeman to explore a number of suggestive hypotheses, in the end the reader may be somewhat confused about the relative importance of the specific variables discussed.

Community and Commitment: Religious Plausibility in a Liberal Protestant Church. By Wade Clark Roof. New York: Elsevier-North Holland Publishing Co., 1978. Pp. x+277. \$16.95.

Benton Johnson
University of Oregon

In recent years sociologists of religion have given a great deal of attention to studying the conflict between theological liberals and conservatives which has periodically threatened the unity of the mainline Protestant denominations for much of the 20th century. In *Community and Commitment* Wade Clark Roof develops and defends the thesis that the theological disagreements involved in such conflict are in large part the product of more basic cognitive and evaluative orientations associated with the extent to which people identify with and participate in their local residential communities. To its active participants, the church, like the country club, is primarily a local institution. It should not be surprising, then, to find that Christian beliefs and practices are closely associated with the narrow and parochial perspectives on life typical of those whose horizons do not extend beyond their local communities. It is breadth of perspective, not theological commitment alone, that underlies much of the controversy of modern church life.

Roof's data consist of questionnaires returned in 1968 by 486 adult white Episcopalians randomly selected from the Diocese of North Carolina. The response rate was 60%. Using items taken from the works of Dobriner, Dye, and Thielbar, Roof constructed an attitudinal index of local community reference based on Merton's famous distinction between locals and cosmopolitans. Confidence that Roof's index measures breadth of perspective is strengthened by his finding that locals are more likely than cosmopolitans to live in towns and smaller cities, to have resided in their communities for many years, to belong to local service clubs and fraternal orders, and to be self-employed. Locals are older than cosmopolitans and are less likely to subscribe to news magazines, to have completed college or done postgraduate work, and to have switched to the Episcopal church after belonging to another denomination. In line with findings from other studies, Roof found no sex differences and only small income differences between locals and cosmopolitans.

The findings which Roof reports are basically in line with his expectations. Locals are considerably more likely than cosmopolitans to participate

in church activities, to have one or more close friends in the congregation, to feel they fit in well at church, and to subscribe to orthodox beliefs. They are also somewhat more likely than cosmopolitans to engage in private devotional practices and to say that their faith has great meaning for them. These differences withstand controls for age, socioeconomic status, and community size. A path analysis reveals that localism remains a good predictor of traditional beliefs and practices even when several key variables are simultaneously controlled. Of special interest is the light Roof's data throw on the attitudes of the laity toward such issues as racism, anti-Semitism, and the church's involvement in social controversies. Although doctrinal orthodoxy is positively related to conservative positions on these issues, a path analysis shows that this relationship is partly spurious. Localism turns out to be a stronger predictor of conservative attitudes than either orthodoxy or education is.

Although these and other findings are intriguing and deserve attention, the book has a number of shortcomings. To begin with, much of it is simply hard to follow, largely because its basic thesis is neither fully explicated nor defended (the first paragraph of this review summarizes only what I take to be its solid sociological core). After wrestling with the text I became convinced that this basic thesis consists of a moral drama informed by Roof's theological views. It incorporates, among other things, the judgment that theological liberals are more authentically Christian than theological conservatives, as well as the dubious assumption that once upon a time most white American Protestants were more cosmopolitan and liberal-minded than they are today. Its lack of redeeming hope for the future reflects the pessimism which has lately overcome both religious and secular liberals. The grand story which Roof is struggling to tell contains some straight sociology, but on balance it is a piece of social science fiction.

On a more strictly sociological level, Roof seems unaware of how difficult it is to define the localism-cosmopolitanism distinction in such a way that it is conceptually independent of that imperfectly interrelated complex of issues involved in the conflict between liberal and conservative perspectives on modern life. I am inclined to suspect that Roof has grabbed the liberal-conservative tiger by one of its many tails. If he has, then his findings, though hardly trivial, are not as newsworthy as he makes them out to be.

Roof's use of up-to-date statistical methods is commendable, but it does not excuse his failure to supply the reader with such elementary facts as the age and sex composition of his sample or how he coded the occupational status of women. It is also puzzling that Roof did not exploit his questionnaire more thoroughly, for it contains a number of unused items clearly relevant to his arguments, for example, an item on political preference, an item on travel, and an item on where respondents went to college and what they majored in. Finally, it was most unsettling to discover that at least two of his tables—3.7 and 4.10—contain gross errors. Roof will gladly supply the corrected versions on request.

Violence against Wives: A Case against the Patriarchy. By R. Emerson Dobash and Russell Dobash. New York: Free Press, 1979. Pp. xii+339. \$14.95.

Mildred Daley Pagelow
University of California, Riverside

Violence against Wives: A Case against the Patriarchy is a valuable contribution to the growing collection of literature on violence in the family. R. Emerson Dobash and Russell Dobash's study concentrated on the specific crime of wife beating, unlike some others that investigated all types of intrafamily violence. Following the tradition of Max Weber, this research team used a variety of methods to study history, individuals, institutions, and communities and then focused on "embedding our analysis of individual violent behavior in the wider social and cultural context" (p. x). The project, begun in the early 1970s, was set up in a structural conflict framework and has three main components: extensive sociohistorical research; a comprehensive and in-depth case study of 109 victims who stayed at refuges for battered women and their children in Scotland; and a content analysis of police and court records from two Scottish cities. To broaden the scope of the research and to free the study from possible culture-specific limitations, Dobash and Dobash utilized an international communications network of knowledgeable persons involved in the issue, and they frequently refer to relevant studies conducted recently in other geographic settings.

Since it is interestingly and clearly written, the book can be useful to readers outside academia, such as service providers, advocates for victims, community leaders, policymakers, and victims themselves. *Violence against Wives* is also a welcome resource for all social scientists interested in family violence, victimology, or social problems, but especially for sociologists looking for scientifically sound research on woman battering. For those who have conducted research on this topic, the book provides interesting findings for comparison with their own; every good study contributes to our understanding of this widespread social problem. For others who want to begin a research project, the book details the Dobash and Dobash research design, the methods employed, interviewing techniques, and so forth, for duplication elsewhere.

In addition, it should be read by every serious scholar of the family. The first five chapters could be adopted as supplementary reading for classes on the family, because they contain a thorough historical review of the development of the patriarchal nuclear family and its hierarchical power structure. The extremely impressive bibliography probably represents one of the richest collections of literature and documents on the family; some sources date back to the 1500s. This vast array of materials gives evidence of the researchers' rigorous approach as they build "a case against the patriarchy." They follow the growth of intimate relationships,

dating practices, courtship, and marriage. From interviews and other evidence, they document changes that occur in relationships shortly after marriage, providing strong empirical support for the "shock theory of marriage," the "Pygmalion Effect," and the process of "dwindling into a wife" described by Jessie Bernard (*The Future of Marriage* [New York: Bantam, 1973]). The authors demonstrate that a fertile field for violence is prepared by traditional gender-role expectations, the socialization of young people into wives and husbands, and the legitimization of power and authority vested in the spouse with greater physical strength. It seems almost inevitable that some husbands resort to physical violence to control and dominate their wives. The patriarchal family system allows husbands to do what they will to their wives and children provided they do not go "too far."

Ample evidence supporting learning theory's contribution to understanding woman battering is provided by the victims, whose statements clearly show that husbands are rewarded, not punished, for their violence. Rewards come to batterers in the form of their wives' growing fear and submissiveness and efforts to placate and please them. Violent husbands receive tacit consent/approval from the social system because of the refusal of communities and institutions to show displeasure or even to acknowledge the violence and the refusal of police to enforce laws to protect wife-victims.

Direct quotations from victims used throughout the text contain Scottish colloquialisms, but the authors provide a glossary (as well as tables and a detailed description of the study in appendixes). Most respondents appear to come from the lower socioeconomic strata, although social class characteristics are not provided. A working-class sample may represent a shortcoming, since it prevents this study from dispelling the myth, which persists despite evidence to the contrary, that violence is restricted to the poor.

The book concludes with suggestions for both short- and long-term changes necessary before husbands will stop beating their wives. Not surprisingly, the most important individual and social change is rejection of the patriarchal family with its hierarchical power structure so that husbands will no longer be given and take the "right" to control and dominate their wives.

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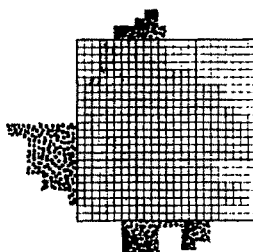
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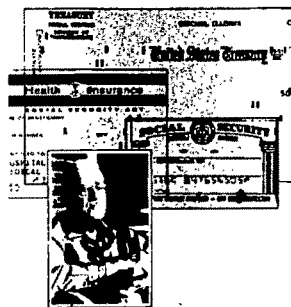
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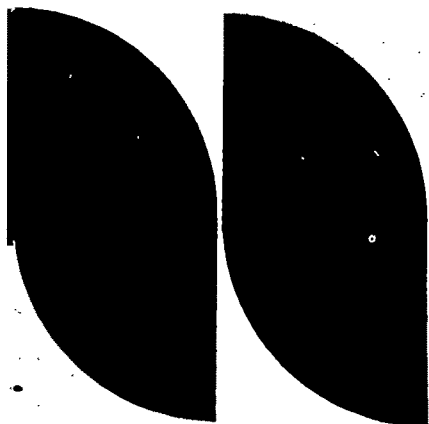
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